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ERRATA IN VOL. XXIV.

Page 353, line 7, *for* surface *read* vapour.
" " " 15, *for* persistence *read* substance.

ERRATA IN VOL. XXV.

Page 103, line 2, *for* manubrium *read* lamina.
" 203, " 4 from bottom, *for* western *read* eastern

OBITUARY NOTICES OF FELLOWS DECEASED.

George Poulett Scrope. It is scarcely possible at the present day to realize the conditions of that intellectual "reign of terror" which prevailed at the commencement of the present century, as the consequence of the unreasoning prejudice and wild alarm excited by the early progress of geological inquiry. At that period, every attempt to explain the past history of the earth by a reference to the causes still in operation upon it was met, not by argument, but by charges of atheism against its propounder; and thus Hutton's masterly fragment of a 'Theory of the Earth,' Playfair's persuasive 'Illustrations,' and Hall's records of accurate observation and ingenious experiment had come to be inscribed in a social *Index Expurgatorius*, and for a while, indeed, might have seemed to be consigned to total oblivion. Equally injurious suspicions were aroused against the geologist who dared to make allusion to the important part which igneous forces have undoubtedly played in the formation of certain rocks; for the authority of Werner had acquired an almost sacred character; and "Vulcanists" and "Huttonians" were equally objects of aversion and contempt.

To two men who have very recently—and within a few months of one another—passed away from our midst, science is indebted for boldly encountering and successfully overcoming this storm of prejudice. Hutton and his friends lived a generation too soon; and thus it was reserved for Lyell and Scrope to carry out the task which the great Scotch philosopher had failed to accomplish, namely, the removal of geology from the domain of speculation to that of inductive science.

Born in the year 1797, and educated successively at Harrow and Cambridge, George Poulett Thomson enjoyed the advantage of a considerable amount of foreign travel during his earlier years. By the advice of his university friends, Sedgwick and Dr. E. D. Clarke, he soon began to devote much of his attention to the phenomena of volcanos, and between the years 1818 and 1821 carefully studied the principal volcanic districts of Italy. He married in 1822 the heiress of the Scrope family, and having adopted her name, set out on a series of geological explorations in Auvergne, Southern Italy, the Ponza Islands, the Euganean Hills, and subsequently in the Siebengebirge and the Eifel, which occupied him till the close of the year 1823. So marked an effect would appear to have been produced upon his mind by the great Vesuvian eruption of 1822—which he was so fortunate as to witness, and which, indeed, inspired his first contribution to geological literature—that from this time forward he seems to have been confirmed in his devotion to that branch of geological

inquiry which, throughout his after life, he so successfully pursued. The result of Scrope's studies and investigations, thus carried on independently and almost unaided, was to make him an enthusiastic supporter of the Huttonian doctrine, that causes similar in kind to those now in operation were quite competent to have produced all past geological changes, and at the same time a most determined opponent of Werner's teaching concerning the aqueous origin of basalt, and of Von Buch's theory of "elevation-craters."

In 1824 Scrope was elected a Fellow of the Geological Society; and having returned to England, he sought the society of those engaged in similar inquiries with himself, and in Charles Lyell found an earnest fellow-student who was able to understand and sympathize with his geological convictions. Most of the members of the infant Geological Society seem at that time to have come to a tacit agreement to lay aside all consideration of those questions relating to the philosophy of their science, which had, in the case of the generation then passing away, been the means of provoking so much heated and embittered controversy. To three of their number, however, such practical abnegation of their responsibilities was impossible; and it is to the boldness and sagacity of Lyell, Scrope, and De la Beche that geology is indebted for the initiation of its modern advance and development. At the period when Scrope returned from the continent Lyell had long been engaged in that patient collection of facts illustrating the changes produced on the earth's surface by the operations of existing causes, which at a subsequent date enabled him to produce his incomparable 'Principles of Geology'; and between the two thinkers the closest friendship soon became established, attended with a freedom of intercourse which was doubtless of great advantage to both of them in preparing for their joint attack upon the prevailing geological errors.

Scrope wrote several memoirs relating to the geology of the countries he had visited, and in 1825 published his 'Considerations on Volcanos.' The dominant idea in this remarkable work is well illustrated in the passage which follows an enumeration of the effects of atmospheric agencies, of the circulation of water on the earth's surface, and of volcanos and earthquakes in causing the destruction and reproduction of rocks, changes of level, and the transference of new rocks from the earth's interior to its surface. These the author declared to be "changes which in their general characters bear so strong an analogy to those which are suspected to have occurred in the earlier stages of the world's history, that, until the processes which give rise to them have been maturely studied under every shape, and then applied with strict impartiality to explain the appearances in question, and until after a long investigation and with the most liberal allowance for all possible variations, and an unlimited series of ages, they have been found wholly inadequate to the purpose, it would be the height of absurdity to have recourse to any

gratuitous and unexampled hypothesis for the solution of these analogous facts."

It is interesting to notice, too, in this work, published now more than half a century ago, many striking anticipations of later geological discoveries—such as the action of water in producing volcanic eruptions, and in causing the liquidity of lavas; the presence of the same agent in deep-seated rocks, and its influence in the formation of those of granitic character; the effects of pressure in producing cleavage and foliation; and the necessarily more or less *local* character of all geological "formations."

It must be confessed, however, that the 'Considerations on Volcanos was a work little calculated to promote the cause which the author and his friend had so much at heart, namely, the removal of the prejudices which hindered the progress of geological inquiry. Its bold and vigorous attacks on the positions of the "Cataclysmists" roused their most determined opposition, while the numerous and sometimes rather crude speculations into which the author allowed himself to be drawn, in his attempt at "the establishment of a new theory of the earth," afforded only too many opportunities for telling retorts, which were eagerly taken advantage of.

In December 1826 Scrope was elected a Fellow of the Royal Society. That he knew how to profit alike from the judicious criticism of friends and the unsparing ridicule of opponents, was clearly enough shown when in 1827 his second work, 'On the Geology and Extinct Volcanos of Central France,' made its appearance. In this essay, which is still everywhere recognized as one of the classics of geological literature, the author succeeded in demonstrating, even to the most incredulous, the power of rivers to excavate the valleys in which they flow; and his friend Lyell pronounced a just eulogium on it when he said:—"We consider Mr. Scrope's work the most able that has appeared since Playfair's 'Illustrations of the Huttonian Theory,' in support of the doctrine that 'valleys have been shaped out progressively by the action of rivers, or of such floods as may occur in the ordinary course of nature.'"

It was now the turn of Lyell to take the field in the controversy with the Catastrophists, and to bring to the support of the Huttonian doctrines that vast mass of patiently collected facts, that moderation in statement and candour in argument, and that calm, persuasive style—occasionally, but involuntarily, flashing into eloquence—which gave such charm and power to the 'Principles of Geology,' and which still recall so vividly to all who knew him the deep enthusiasm, curbed by sound judgment, which distinguished its author.

During the composition of this great work the two friends were in constant correspondence. Lyell had wisely determined to undermine the positions of his adversaries, rather than to arouse their opposition by

direct attacks; and to his more ardent friend was committed the congenial task of applying and driving home the arguments of the 'Principles' in a series of reviews of the work. The first and second volumes were introduced to the public by appreciative and discriminative notices in the 'Quarterly,' which were written by Scrope; and the completion of the third edition in 1835 was made the occasion of a final article from the same pen.

In these able reviews, which are admitted to have produced a very great effect at the time they were written, and which may be still read with much profit, Scrope took up certain positions somewhat in advance of his friend in geological theory. Thus we find him, even at this early date, demurring to the too absolutely uniformitarian doctrines of Hutton and Lyell, and maintaining views very similar to those developed by Prof. Huxley in his Anniversary Address to the Geological Society in 1869. On the question, too, of the relative influence of subaerial and marine denudation in originating the forms of the earth's surface, Scrope at this early date maintained views which his friend was not prepared to accept till some years later.

Unfortunately, however, for geological science, Scrope had by this time almost forsaken scientific for political labours; but of his useful and honourable public career this is not the place to speak. Nevertheless when, in 1858, he found his friend Lyell again engaged in a controversy with the supporters of the "Theory of Elevation-Craters," he determined once more to bear his part in meeting the new arguments of their old opponents; and the publication of his able memoir "On the formation of Cones and Craters" was the consequence of this resolve. As he now began gradually to withdraw himself from the sphere of politics, Scrope found time to revisit his former haunts, and to prepare new and greatly improved editions of his earlier works; and these have been translated into the principal European languages. During his declining years, which decaying strength and increasing blindness compelled him to spend in almost complete retirement, he followed with interest the progress of thought in connexion with his favourite science, and watched with jealousy its excentric development, contributing from time to time many a suggestive essay or trenchant criticism to the scientific periodicals. Not a few of the younger students of that branch of the science which he had himself so successfully cultivated were indebted to him, during these years of increasing feebleness, when he could no longer take the field, not only for counsel, sympathy, and encouragement, but for friendly aid in pursuing their researches. But the death of Lyell, his early friend and fellow-labourer, with whom to the last he maintained an affectionate correspondence, produced a great shock to his weakened frame; and within a few months thereafter he passed peacefully away.

Peter Andreas Hansen, born at Tondern in Schleswig, on the 8th of December, 1795, was the son of Nikolai Hansen, a prosperous gold- and silversmith of that town. Young Hansen attended the town school, where he learnt the elements of Latin and French, and showed special aptitude for mathematics and physics.

After his confirmation he chose watch-making for his calling, and went to Flensburg to serve his apprenticeship in that business. He soon distinguished himself by his skill and ingenuity in mechanical construction, and then set to work on his own account at mathematical studies. His circumstances, and also his father's wishes, were opposed to his ardent desire to study at a University; and he therefore at the end of his apprenticeship returned home to his parents, and in the year 1818 began his wanderings. He first passed some months in Berlin, where he found some occupation under a master who was one of a French colony settled there, and in whose family he acquired some familiarity with the French language.

At the end of the year 1819 he returned to Tondern, and settled down in his father's house as a watchmaker. But as early as the spring of 1820 the influence of a physician, Dr. Dirks, who was interested in matters appertaining to mathematics and physics, and who recognized Hansen's talents, gave a decided turn to his course of life. Dirks succeeded in gaining the consent of the father for the young man to go to Altona to join Professor Schumacher, who was there entrusted with the management of the Danish measurement of an arc of a meridian. Schumacher received Hansen very kindly, and exerted himself to obtain for him an appointment in the measurement. He was, however, in the first instance unsuccessful; and Hansen, who had meanwhile begun to work at Astronomy in the Round Tower, then the Copenhagen Observatory, was thinking of going to Göttingen to study under Gauss. Finding that Gauss was prevented by the measuring of an arc of the meridian in Hanover from lecturing, he was at last induced by Schumacher, and with the royal consent, to go at his own expense to Altona (in 1820) to take part in the measurement in Holstein.

After the completion of this task he returned to Tondern, from which place he was recalled to Copenhagen by Schumacher in January 1821. He was employed regularly on the survey.

Before long, through the influence of Schumacher, the king became interested in Hansen, and from that time forth the latter received many personal proofs of appreciation and recognition of his services, of which he always cherished a grateful recollection.

In the summer of 1822 Schumacher sent Hansen to Heligoland to assist in some astronomical observations for the determination of geographical positions in conjunction with some English savants.

Schumacher became more and more intimate with Hansen, and a friendship grew up between them which was only to be dissolved by death.

Before long Hansen's eminent services to science attracted the notice of the astronomical world; and in the year 1825, when Encke left the Seeberg at Gotha to take the superintendence of the Observatory of the Berlin Academy, Hansen was selected to conduct the Observatory on the Seeberg, with the title of Professor, a post which he retained for nearly half a century, namely, to the end of his life. The small salary (of 600 Thalers) attached to this appointment obliged him for a number of years to undertake calculations for Ephemerides for the Danish and English Governments.

He lived in the Observatory itself from the year 1825 to 1839; but as the arrangements and fittings were no longer adapted to the requirements of science, and besides the building itself was too much out of repair to bear much longer the wear and tear of weather, Hansen, with the consent of the Duke, removed to the town of Gotha, and in the southern suburb built himself a house with a little private observatory, in which the meridian circle was set up. Here he worked from the year 1842 to 1857, until the new Ducal Observatory was established, which was fitted up under his superintendence in such a perfect manner, though on a very modest scale, that it has since served as a model to several larger institutions.

The work Hansen accomplished at Gotha embraces almost every branch of practical and theoretical astronomy; and if no regular and comprehensive series of observations has been made under him, the cause of this deficiency lies in the insufficient funds at his disposal, which neither admitted of the payment of assistants nor of the purchase of large instruments. But though Gotha could not in these respects rival other great observatories, it possessed an astronomer who was enabled by his mechanical genius to do much in improving the art of observation by ingenious improvements in the arrangements and use of his instruments. The apparatus and methods which he invented for the investigation of errors of division, for the prevention of flexure, for the registration of observations, for the parallactic motion of telescopes mounted horizontally, as well as numerous original contrivances, such as those which were applied in the building of the present observatory, obtained the general approbation of astronomers. His works on the use of the Fraunhofer Heliometer, the Transit-instrument, and the Equatorial have become classical in spherical astronomy.

But it was especially Hansen's rare mathematical ability that enabled him to carry out the great works which make an epoch in the department of physical astronomy known as the Theory of Perturbations. As early as the first years of his residence on the Seeberg, he published in the 'Astronomische Nachrichten' the main principles of his new theory of perturbations, which in the course of years he employed in the accurate investigation and calculation of the motion of the moon, of the sun, of the greater and lesser planets, and of the comets.

Aided by his remarkable facility in calculation (for four-figure logarithms he scarcely required the aid of the tables), he in 1853, conjointly with Olufsen and with pecuniary assistance from Denmark, at the request of the Society of Sciences at Copenhagen, completed his solar tables; and likewise in 1857 the lunar tables printed by the English Admiralty, for which the Parliament of Great Britain voted him the sum of £1000.

In 1838 he published the theory of the moon's motion in a special work, 'Fundamenta nova investigationis orbitæ veræ quam Luna perlustrat;' the exposition of the theoretical calculation of the perturbations employed in the lunar tables appeared in 1862–64, in two elaborate treatises published in the Transactions of the Royal Society of Sciences in Saxony. An appendix treats of the verification of chronological eclipses. He wrote a series of treatises on the theory of the absolute Perturbations of the Asteroids, to which he appended tables of Egeria.

The comet disturbances he treated in two special works—the one entitled "Ermittelung der absoluten Störungen in Ellipsen von beliebiger Excentricität und Neigung," 1843 (which was translated into French by Mauvais); the other a prize treatise of the French Academy, 'Mémoire sur le calcul des Perturbations qu'éprouvent les comètes.' As examples, he gives the disturbances of Encke's comet by the Earth and Saturn.

For his 'Untersuchungen über die gegenseitigen Störungen des Jupiters und Saturns' he had received in 1830 the prize offered by the Berlin Academy. A further posthumous memoir, "Ueber die Störungen der grossen Planeten, insbesondere des Jupiters," has just been published in the Transactions of the Saxon Society. In the same Transactions he published, amongst other things, very elaborate memoirs on the "Theorie des Aequatoreals" (1855), on "Theorie der Sonnenfinsternisse und verwandten Erscheinungen" (1858), on the "Bestimmung der Sonnenparallaxe durch Venusvorübergänge vor der Sonnenscheibe" (1870), in relation to the Transit of 1874, two memoirs connected with dioptrical researches (1871 and 1874), besides a long series of memoirs on the Calculus of Probabilities and the higher Geodesy (1865–1869), to which he was led by his taking part in the European measurement of an arc of a meridian.

One of his latest memoirs treats of the determination of errors in the division of a graduated rectilinear scale, and was written with a view to the expected photographic measures of the Transit of Venus.

Among the memoirs of the London Astronomical Society we may mention two important papers on the Inequalities of long period in the Moon's motion (1847), and on the Figure of the Moon (1854). In the former he examines the influence of Venus on the mean longitude of the moon, and in the latter he endeavours to show that the centre of the figure of the moon does not coincide with her centre of gravity, but that the latter is about 59 kilometres further from us than the former.

After the death of Schumacher, Hansen shared for some time the editorship of the 'Astronomische Nachrichten' which Schumacher had

founded. Petersen was for a time his companion in this editorial work at Altona. Of the numerous contributions he made to this periodical we need only mention, in addition to those already spoken of on the theory of perturbations, those of early date on the transit-instrument and on the meridian circle, on eclipses and occultations of Stars, on refraction of light, on the determination of the latitude, on the calculus of probabilities and the method of least squares, on different geodetic problems, on the disturbances of Encke's comet by a resisting medium, &c.; and at a later period especially the papers on the calculation of special perturbations by mechanical quadratures and the reduction of places to the ecliptic of the date. An immense number of essays on various subjects of interest have appeared in the Reports of the Transactions of the Mathematical and Physical Class of the Saxon Society; of these we need merely allude to the papers on the solution of a system of linear equations; on spherical harmonics; on ideal coordinates; on Keppler's problem; the ecliptic tables, &c., together with the analysis of the same; on the arrangements of the new Ducal Observatory at Gotha; on the determination of the orbit of a heavenly body from three observations; on the secular variation of the mean longitude of the Moon, and the alteration of the day's length by the gradual decrease of the rate of rotation of the Earth (April 1863); on the computation of triangulations; on the centre of gravity of spherical triangles; on a new telescope-stand; on the application of photography for observing the Transit of Venus, &c.

Hansen published papers in various other periodicals, as, for instance, in the 'Comptes Rendus' of the Paris Academy, in the Monthly Reports of the Berlin Academy, in the Monthly Notices of the London Astronomical Society, in the Results of the Magnetic Society, in Schumacher's Astronomical Jahrbuch, the Mathematical Works of Jacobi, the Mathematical Journal of Gutten in Wilna (über das Repsold'sche Aequatoreal), the Memoirs of the Naturforschenden Gesellschaft in Danzig (which latter Society awarded him the prize for his treatise, "Theorie der Pendelbewegung mit Rücksicht auf die Gestalt und Umdrehung der Erde," 1853), and so on. In conclusion we may add to this short summary the Memoir on the Fraunhofer Heliometer, 1827, and the 'Commentatio de gradus praecisionis computatione,' written on occasion of Olbers's Jubilee in 1830.

Hansen did not escape controversy. Some of his works and their results have been attacked by German, French, English, and American astronomers; to these attacks such men as Pontécoulant, Lubbock, Encke, Brünnow, Peters, Baeyer, Weingarten, Newcomb, and Delaunay have lent their names. No one more than Hansen regretted the bitterness which is more or less inseparable from such discussions, though he felt it a duty to science to give distinct expression to his convictions.

He received various honorary titles and decorations, and numerous recognitions of his scientific position. Most of the learned societies of Germany and other countries elected him a member. The Royal Society,

of which he was a foreign member, conferred upon him the Copley Medal in 1850. In 1842, and again in 1860, he received the Medal of the Royal Astronomical Society.

A great number of his contemporaries in astronomy and mathematics enjoyed the privilege of his acquaintance and instruction. His pupils retained a grateful sense of the amiability and patience with which he laboured to make them worthy disciples of his science.

Hansen had many offers of other honourable and advantageous appointments; amongst others one at Dorpat as successor to Struve in 1839, one as successor to Bessel in Königsberg in 1847, one at Copenhagen; and in the year 1866 he was offered that of Astronomer to the Berlin Academy.

In the year 1828 he married the eldest daughter of the Oberforstmeister Braun. He retained his vigour of body and mind to a considerable age, and his facility of writing never left him. At times he almost lived at his writing-table, not seeming to feel the need of recreation either of body or mind. Healthy sleep was sufficient for the restoration of his powers. He did not take bodily exercise; and in his latter years was seldom willing to make any excursion or journey. He twice visited England, mainly to promote the publication of his Lunar Tables; and he deeply gratified the Director of the Chief Russian Observatory, Pulkova, by his presence at the 25th anniversary of the founding of that great institution.

His last years were saddened by a disease of the eyes which altogether prevented him from reading, and even rendered writing very difficult. In the last months of his life he had the additional suffering of a liver complaint. His death took place on the 28th of March, 1874. He had in the beginning of that month completed the manuscript paper "On the Determination of errors in the Division of a Rectilinear Scale," and sent it to the printer. The *post mortem* examination of the head showed a finely formed brain with a remarkably thin skull.

The main feature of his character was an ardent love of truth; what he had once recognized as true he maintained with all the energy of conviction, caring little whether others were convinced or not; and he sometimes found it difficult to understand how an opinion contrary to his own could be honestly held. Thus he sometimes considered himself aggrieved when he was opposed in things which he had made his own by conscientious study. But when convinced of the excellence of another view, he would at once give his unqualified adhesion to it.

As Hansen had not studied either at a Gymnasium or a University, and had thus been compelled to forego the systematic training of any high school, he owed all the many-sided learning which he acquired to his own untiring diligence and thirst for knowledge. He improved his acquaintance with the French and Latin languages which he had begun at school, as his French and Latin papers sufficiently show. He had also a fair knowledge of English, and even made some

progress in Russian. In his latter years he often recited from memory passages from Homer and Horace. Hansen's personal appearance was noble and imposing; his tall and stately form with his early grey hair gave him a venerable appearance. His whole demeanour, as well as his expression of countenance, announced a man of intellect, and one whose opinions were the result of matured thought. He took little interest in the commonplaces of society, and thereby gave many people the impression of being reserved, but he was gladly and freely communicative in intercourse with those who took a real interest in scientific questions. He willingly acknowledged any suggestion offered to him on such subjects, and enjoyed on this account especially his personal intercourse with the celebrated mathematician Jacobi, during the stay of the latter at Gotha, and felt deeply the loss caused by his early death.

He had great taste for music, and played both on the piano and the harmonium. Another recreation to which he occasionally resorted was chess. He had not much appreciation of the beauties of nature, owing probably to shortsightedness and the peculiarity of his eyes, he being to a considerable extent colour-blind. Accustomed in early youth to a flat sea-shore, he felt rather oppressed than attracted by the romantic scenery of the Thuringian forest.

He was an affectionate and devoted husband and father, and in his old age took the greatest delight in the society of his little grandchildren.

His predilection for mechanical contrivances, which was one of his chief sources of recreation after any continuous mental labour, continued in later years; and when his eyesight began to fail him, he with his own hands introduced improvements in a most ingenious and artistic watch, which he had contrived during his residence on the Seeberg, and which, besides other things, indicated the mean time, the apparent solar time, and the sidereal time. He also took a steady interest in the machine factory which his son had erected in Gotha.

For nearly forty years Hansen conducted the Detail Survey of the Gotha domains with untiring zeal and care, and had the satisfaction of completing it before his death. Hansen held the appointment of Commissioner of the Ducal Government, and was for a long period President of the "Permanent Commission," which post he only relinquished at last on account of his health. He was also an efficient President of the Commission appointed by the German Empire for the preparation for observing the Transit of Venus. Both these scientific undertakings gave him an opportunity of making elaborate and valuable theoretical investigations.

His collection of scientific books has been, by the enlightened care of the Grand Ducal Government and with the consent of the Landtag, purchased for the Library of the Observatory. With the most praiseworthy liberality his family have delivered up to the Astronomical Society of Leipzig the whole of his valuable collection of manuscripts, thus affording a further proof of the interest ever taken by Hansen in the labours of that institution, notwithstanding that he was not a member of it.

Sir Charles Lyell was the eldest son of Charles Lyell, Esq., of Kinnordy, in Forfarshire, where he was born on the 14th of November, 1797. He inherited from his father a strong taste for natural-history pursuits, and in early boyhood devoted himself to them with enthusiasm in the New Forest, to which his family had removed not long after his birth. Destined for the legal profession, he studied at Exeter College, Oxford, and took his M.A. degree in the year 1821. He was duly called to the bar; but by this time his bias towards the life of a scientific student had grown so decided, that the practice of the law became increasingly irksome to him. He had studied geology under Buckland, whom he had accompanied to the field on those equestrian excursions which the merry Professor used to lead over the surrounding country. We find him already, in February 1824, elected Secretary of the Geological Society of London, and in 1826 he entered the Royal Society. His first geological memoir was on some freshwater marls in his native county of Forfarshire; it was read in December 1824: containing a comparison of the recent with ancient deposits of the same kind, it showed the pathway of inquiry which even then he had deliberately chosen, and along which he was to journey to the end of his life as the great apostle of the doctrine that the Present alone affords the key to the Past.

Having recognized at the beginning of his career that the true progress of geology could best be advanced by a careful collection and discussion of all facts bearing upon the present changes of the earth's surface, he devoted himself for several years to a diligent study of all accessible works of travel from which trustworthy information could be obtained regarding modern geological changes. During this time he seems to have written no scientific memoirs for publication; but by the end of the spring of 1828 he had completed the sketch of his 'Principles of Geology.' In May of that year he accompanied Mr. and Mrs. Murchison to France, and spent some time with them in the scientific circles of Paris, among the volcanic rocks of Auvergne and in the interesting valley of the Rhône. This journey formed the turning-point in his career. Instead of returning to London and resuming his professional work, he wrote to his father stating that after the fullest consideration he had at last decided to give up the law and devote himself to science as the occupation of his life. Having taken this determination, he struck southwards into Italy and Sicily, and was soon immersed in those researches in tertiary geology which formed one of the great features in the scientific work of his life.

The first volume of his 'Principles' was published in January 1830. Its appearance at once placed its author in front of the geologists of his day—a position acknowledged even by those who would not admit his doctrine that the present order of nature should be taken as a measure and guide in explaining former geological changes. Before the second volume appeared in January 1832, he had been elected Professor of

Geology in King's College, London—an appointment, however, which he did not long retain. His summers were devoted to excursions through different parts of the British Islands and to tours on the continent, not so much with the view of doing original field-work himself as to see with his own eyes the ground and the rocks described by others, and thus to be better enabled to realize their descriptions and to judge of the relative importance of their contributions to geology. In this way he traversed Europe, from the mountains of Scandinavia to the shores of Sicily, and extended his travels into the Canary Islands. Anxious still further to enlarge his experience, he went to the United States and spent some time in a geological tour there, of which the results were published in 1841 in his volumes of a 'Visit to the United States.' A few years later he again crossed the Atlantic and collected materials, which appeared in 1845 in his 'Second Visit to the United States,' as well as in numerous papers published in various scientific journals.

Though he wrote many minor papers and a few large memoirs on original researches of his own, most of his work appeared from time to time in the successive editions of his 'Principles' and 'Elements.' Among his most important memoirs should be mentioned his paper on the Consolidation of Lava on steep slopes upon Etna, published in the 'Philosophical Transactions' for 1858. This paper may be regarded as having finally exploded the elevation-crater theory of Von Buch, although the admirable memoirs of Scrope had already given that theory its death-blow.

Perhaps the best idea of the solid services rendered by Lyell to geology is obtained by looking back at the condition of the science when he first began to study it, and by contrasting that state with the luminous exposition of the subject in the early editions of his 'Principles.' To men who had been compelled to gain their general view of geology from such works as Daubuisson's 'Traité' or Cuvier's 'Theory of the Earth,' the appearance of Lyell's volumes must have been of the nature of a new revelation. From vague statements about early convulsions and a higher intensity of all terrestrial energy culminating in periodic catastrophes, they were led back, with rare sagacity and eloquence, to the living, moving world around them, and taught to find there, in actual progress now, the analogues of all that they could discover to have been effected in the geological past.

The keynote which Lyell struck at the very outset was, that in geology the past can be understood only through the present, that the forces now in operation are quite powerful enough to produce changes as stupendous as any which have taken place in former times, provided only that they get time enough for their task.

These views were not promulgated for the first time by the author of the 'Principles of Geology.' In cruder form they had been earnestly urged by Hutton, and eloquently illustrated and extended by Playfair; but after much turmoil and conflict of opinion, they had very generally been allowed

to sink out of sight. On the continent, indeed, they had never excited much attention, and were for the most part ignored as mere vague speculation; in this country they had been only partially adopted even by those who professed to belong to the Huttonian school: so that it was, in one sense, as a new doctrine that they were taken up by Lyell, and enforced with a wealth of illustration and cogency of argument which rapidly gained acceptance for them in Britain, and eventually led to their development in every country where the science is cultivated.

In one important respect Lyell's teaching differed from that of his predecessors. Up till his time little had been made of organic remains as monuments of former physical changes as well as records of the history of the progress of life upon the surface of the earth. The stratigraphical labours of William Smith, followed by the palæontological researches of Cuvier and Brongniart, opened fields of inquiry of which their predecessors never dreamed. The old beliefs were being rudely shaken, and in this transition-state of the science there was needed a leisured thinker who could devote a calm judgment and a facile pen to the task of codifying the scattered observations which had accumulated to so vast an extent, and of evolving from them the general principles which they seemed to establish, and which, when clearly announced, could not fail greatly to stimulate and guide the future progress of geology.

Such was the task which Lyell set before himself half a century ago. In its discharge he devoted himself with special ardour to the development of that biological side of geology which owes, if not its existence, at least its rapid and wide spread to his influence. Though not himself, in the strict sense, either a zoologist or botanist, he kept himself throughout his life abreast of the progress of the biological sciences, and on terms of intimate relationship with those by whom that progress was sustained in this country and abroad. He was, in the true meaning of the word, a naturalist. He had in his day few equals in the grasp which he could take of natural-history subjects in their geological bearings. Thus the geographical distribution of plants and animals received more and more ample treatment from him as he advanced in years; the succession of living forms in time gave him a theme for accurate and eloquent description. In fact the breadth of his conception of what geology ought to be was perhaps even more conspicuously marked in this biological side than in that which treated of inorganic operations. He enlisted in his service every branch of natural history which could elucidate the story of the earth and its inhabitants; and not merely the published information on these questions, but many of the floating ideas of discoverers, found their first exposition and illustration in his pages.

Probably no scientific work, except the 'Origin of Species,' has during the lifetime of its author exercised so powerful an influence upon the science which it illustrates as Lyell's 'Principles of Geology.' No fewer than eleven editions appeared, each of them marking a distinct and some-

times a very great advance. At first the descriptive part, relating to the succession of the stratified formations of the earth's crust, was included in the larger work; but this was soon separated, and expanded into an independent volume under the title of 'Elements of Geology,' of which (including the 'Student's Elements') the author lived to edit eight editions. Of these, too, each as it appeared was hailed as the summing-up of a calm and impartial judge of the evidence and arguments in all the disputed stratigraphical questions of the day. When the discussion first arose as to the nature and significance of the worked flints found in the valley of the Somme and elsewhere, Lyell at once put himself in the front by collecting all the available information and publishing it, in 1863, in his 'Antiquity of Man.'

Sir Charles Lyell's position, as a foremost thinker among the geologists of his day, was fully acknowledged during his lifetime. He twice held the Chair of the Geological Society. He presided over the British Association at Bath in 1864. He received the honour of D.C.L. from his own University in 1855. The Royal Society gave him the Copley Medal in 1858. He was chosen a member of the chief learned societies of Europe and America. By his own Sovereign he was knighted in 1848; and, as a further mark of Her Majesty's appreciation, he was raised to the dignity of a Baronet in 1864. He married a daughter of the late Mr. Leonard Horner, F.R.S. Throughout his long and honoured career she joined to the fullest in his labours, accompanying and aiding him in his journeys, assisting him in his literary work, entering into his geological speculations with the heartiest sympathy, and, above all, sharing his friendships and throwing over them, and over the social gatherings at his house, the charm of her genial manner and conversation. She predeceased him in 1873, leaving no children. Sir Charles himself died on the 22nd of February, 1875, and, as a fitting close to his illustrious life, was publicly buried in Westminster Abbey.—A. G.

Dr. Neil Arnott was born at Arbroath, in Scotland, on the 15th May, 1788, and died in London on the 2nd March, 1874. He passed his childhood at Upper Dysart, and began his education partly with his mother, a woman of great energy and ability, and partly in the parochial school of Lunan, near Arbroath. From his earliest years he gave great attention to all natural objects around him, and in after life he often referred to the experience thus acquired as his introduction to the phenomena of the physical world.

Neil Arnott entered the Aberdeen Grammar School in November 1798, and he continued there three years. He went into the Bursary Competition at Marischal College at the beginning of the session of 1801. He was then thirteen, and older than the average of boys at the same stage in the school. He came in sixth, and was entered a student of Marischal College, where he went through the accustomed course of

four sessions, devoting his time to the study of the classics and mathematics, to civil and natural history, as well as chemistry, botany, and zoology.

In the third year of the curriculum he took up the subject of Natural Philosophy, which appears to have had for him an absorbing interest. The Professor in the University was Patrick Copland, a man gifted with remarkable powers of elucidating the phenomena of the science by experiments, and of attracting and fixing the attention of his young pupils. Among these none profited so much by Copland's lectures as the subject of this memoir. In this department Neil Arnott felt himself thoroughly at home, and, aided by the friendly counsel and encouragement of the Professor, he made great and rapid progress. He carried away full notes of these lectures, and turned them to account in his after studies. In other points, too, he benefited greatly by Copland's instructions, *i.e.* in selecting from daily life familiar illustrations of natural phenomena, and in the invention and construction of the most simple forms of apparatus for the purpose of experimental demonstration. After a successful career of study, Neil Arnott obtained his M.A. degree in the year 1805.

He selected the medical profession for his future career, and commenced the study of medicine in Aberdeen, where it was known that he had worked hard in order to qualify himself for entering one of the London Hospital Schools. His wishes in this respect were soon gratified. He arrived in London on the 29th September, 1806, when in his nineteenth year, and entered as a pupil at St. George's Hospital, under Sir Everard Home. Through the influence of Sir Everard he, at a later period, obtained an appointment as surgeon in the East-India Company's medical service. Much of the experience of sea-life which he thereby obtained he afterwards turned to good account in preparing the work by which his name is so well known to the scientific world,—the 'Elements of Physics.' Numerous observations on the waves, currents, tides, winds, and storms, and on the depth and colour of the sea were made by him, and afterwards incorporated in the chapters of this work. He left England on his first voyage to China in 1807, before he had completed his nineteenth year, and after a disastrous course, which took him across the Atlantic to Rio, he landed at the Cape of Good Hope. He there ascended the Table Mountain and made those meteorological observations which are recorded in the 'Physics.' He returned to London in 1809, and made a second voyage to China in 1810.

It was during these voyages, and when in charge of troops, that his attention was specially directed to sanitary matters: ventilation, warmth, clothing, food, air, and exercise were subjects which came before him in a practical form, and many ingenious contrivances were resorted to by him in order to restore and maintain in a healthy condition the invalided men who had been placed under his care. He was so successful in these

efforts that he lost during the voyage home only one man who was hopelessly diseased, and on his return he received the thanks of the military authorities.

In 1811 he commenced practice in London, in Hunter Street, Brunswick Square. He was acquainted with the French, Spanish, and Italian languages; and he had among his patients a large number of foreign refugees who resided in that neighbourhood. He obtained the diploma of the College of Surgeons in 1813. Although fully engaged in medical practice, Neil Arnott's mind was still much directed towards chemistry and physics, and in this year he gave at the Burton Rooms a course of lectures on Natural Philosophy applied to Medicine. The novelty and utility of this course rendered it highly attractive to medical men. At a later date (1825), when residing in Bedford Square, he gave two courses of lectures on the same subject, chiefly to members of the medical profession. He declined, however, to continue these courses; and in the year 1827 he published the substance of them in his 'Elements of Physics.'

In 1814 the University of Aberdeen conferred upon him the degree of M.D. He practised for many years as a physician, and held the appointments of physician to the French and Spanish Embassies. As a physician Dr. Arnott placed more confidence in regimen than he did in drugs. He made many useful mechanical suggestions for the treatment of certain diseases, such as hernia, stricture, &c. It was from 1838 to 1855 that he was in the height of his professional career. He then withdrew from practice, and devoted his time almost exclusively to scientific subjects, including also those of a sanitary nature. In this year he published an account of his smokeless grate, a modification of the open fire-grate, but possessing many of the advantages of the stove. This invention included a complete combustion of smoke and a great economy of fuel with a steadiness and endurance of the fire. It was in reference to this invention that in 1854 the Rumford Medal of the Royal Society was awarded to Dr. Arnott.

In 1832 he first made known the use of the Hydrostatic or Water-bed, which has proved of such important service in medical practice. Devoting his attention to sanitary appliances, including the proper methods of warming and ventilating dwelling-houses, hospitals, and infirmaries, he introduced the stoves which are well known by his name. In his essay on "Warming and Ventilation," published in 1838, he gave a full description of his stove. For this and other novel appliances in the treatment of disease and the preservation of public health, the Jurors of the Universal Exposition of Paris, in 1855, awarded to him a Gold Medal, to which the Emperor Napoleon III. added the Cross of the Legion of Honour.

On the foundation of the University of London, in 1836, Dr. Arnott was appointed one of the original Members of the Senate. In 1837 he

was named one of the Physicians Extraordinary to Her Majesty, and in the following year he was elected a Fellow of the Royal Society. In 1854 he was requested by the President of the General Board of Health to become one of the Medical Council; and it was at this period that he devoted a large portion of his time to education and public works.

As the inventor of the "Arnott Stove," the "Arnott Ventilator," and the Water-bed, it is not likely that his name will soon be forgotten; but it deserves to be recorded in his honour that he refused to patent any of his inventions. His great object through life was to benefit others, and not to obtain pecuniary profit. Sir Arthur Helps, in one of his later works, says truly of Dr. Arnott, "His whole life was given to the service of his fellow men. A truer reformer in the best sense of the word never existed." One great secret of Dr. Arnott's success as a writer on natural philosophy was, that from his earliest days he was an acute observer of all that went on around him. Nothing bearing upon physics escaped his notice. In addition to this faculty of observation he possessed happy powers of description. The reader was not only instructed, but made to feel a deep interest in the subject. Instruction was thus rendered a pleasing recreation. His earnest wish was to make the path of learning easy to all; and the reception of his 'Elements of Physics,' the first edition of which appeared in 1827, is a proof of his success in this respect. There are few educated men of the past generation who will not remember the interest with which they read the first volume of this excellent work; and it is not too much to say that the learned and the unlearned, the student and the philosopher, have benefited by its perusal. This work did more for the encouragement of the study of Natural Philosophy than all the works on the subject which had preceded it. Within five years of its publication five large editions were called for, and, although not then complete, it was reprinted in America and translated into several foreign languages. In November 1829 appeared the first part of the second volume. The work underwent six editions during the life of the author, and a posthumous seventh edition has lately appeared.

In 1861 he published his 'Survey of Human Progress,' and this was followed by various monographs on educational subjects.

In 1856 Dr. Arnott married the widow of one of his oldest friends, Mr. Knight. This lady was the daughter of James Hunt Holley, Esq., of Bleckling, in Norfolk. She was an accomplished woman, and the match was in every way suitable. She survived her husband upwards of two years. She had the same philanthropic and educational views, and lived to carry out his intentions in reference to the endowment of the Scotch Universities. The desire of both was to encourage the study of Natural Philosophy. In 1869 Dr. Arnott granted to the University of London £2000, and to each of the four Universities of Scotland (Aberdeen, Edinburgh, Glasgow, and St. Andrews) £1000, while, subsequently to his

death, Mrs. Arnott granted an additional sum of £4000 to be divided among these Universities. Thus within the period of seven years Dr. and Mrs. Arnott had contributed the sum of ten thousand pounds for the promotion of scientific knowledge.

Dr. Arnott was a man of genial disposition, and had a large circle of friends. He took a delight in the society of these friends and in the progress of scientific research, until the infirmities of age compelled him in a great measure to withdraw from social intercourse.

He died in the 86th year of his age, and up to the last year of his life his mind was still actively occupied in devising and maturing new projects or inventions. Among these may be mentioned a chair-bed for the prevention of sea-sickness and a floating breakwater. It was the delight of his life to devise means of ameliorating suffering and adding to human comfort.

Dr. Arnott died in the Roman Catholic faith, and by his own desire his body was buried in the Dean Cemetery, at Edinburgh, in the grave in which the remains of his mother and other members of his family are deposited. An obelisk with an appropriate inscription in commemoration of himself and them has been erected over the grave.

Anders Jonas Ångström was born on the 13th of August, 1814, at the works of Lögdö, in Medelpad (one of the most northerly provinces of Sweden), where his father was chaplain. Although the latter never obtained any advancement in his profession beyond the position of Comminister, he managed to make his scanty income suffice for the educational expenses of his three sons at the secondary school and at the gymnasium of Hernösand. Of these sons, the eldest Johan, now a medical man at Örnsköldsvik, is well known for his botanical researches, and the youngest, Carl Arendt, is Professor of Practical Mechanics at the Polytechnic School of Stockholm. The second son, Anders Jonas, after studying at the University of Upsala, paid special attention to physics and mathematics, but was obliged from time to time to interrupt his studies in order to give private lessons.

In 1839 he took the degree of Doctor of Philosophy, and shortly afterwards was appointed Professeur agrégé of Physical Science at the University of Upsala, the celebrated Rudberg at that time holding the appointment of Professor of Physical Science.

Ångström had a decided predilection for physics, but as, after the death of Rudberg, Adolphe Svanberg was, in 1841, appointed to the professorship of Physics, he saw no prospect of advancement for himself in that department at Upsala. He therefore accepted the appointment of Assistant Professor of Astronomy. In order to gain practice in making astronomical observations, he passed the year 1842 at the Observatory of Stockholm; but after his return to Upsala he occupied himself chiefly with the theoretical branches of the science, because at that time it was almost

impossible, owing to the want of a suitable observatory in Upsala, to make really accurate observations there.

Ångström's published works on astronomy are not numerous. The most important among them are that entitled 'Ad theoriam Cometarum additamenta' (which he published as an evidence of his competency for the post of Assistant Professor), and another, first published, in 1862, in the 'Transactions' of the Society of Sciences of Upsala.

This latter memoir illustrates Ångström's power of arriving at his end by a more direct process, but which others could only attain by long calculation. Both papers show considerable originality of thought. Thus, for instance, he never approved of the explanation generally adopted of the retardation of Encke's comet by the resistance of the cosmic ether; but his opinion was that it depended on the perturbations of the little planets situated between Mars and Jupiter. There is a note of his on this subject in the 'Comptes Rendus' of the Academy of Sciences of Stockholm for the year 1854. He considered that, as a rule, modern astronomers collect too many observations, gaining from them very few new results in comparison to the number and to the immense amount of labour expended.

The observations on the phenomena of magnetism set on foot by Gauss were originally considered as belonging to the domain of astronomy, an opinion still held by some persons. Gustave Svanberg had, as early as 1836, established at Upsala magnetometers which he had procured from Göttingen. Ångström became much interested in the observations made with the aid of these instruments after his engagement at the Observatory; and in the course of a tour abroad, which he made in 1843, he visited the Observatory of Bogenhausen, near Munich, in order to study the new magnetic apparatus which Lamont had constructed. It was more particularly the apparatus to be employed in travelling which attracted Ångström's attention. He obtained at Munich some of these instruments, and used them assiduously during the remainder of his journey, making magnetic observations at Göttingen, Paris, Brussels, and other places. Between the years 1850 and 1870 he made a large number of observations on magnetic intensity and inclination in different parts of Sweden, but they were never published.

Ångström was commissioned by the Academy of Sciences of Stockholm to work out the magnetic observations which had been made between the years 1851 and 1853, during the voyage round the world of the Swedish frigate 'Eugénie.' The observer, M. Johannson, had died shortly after the return of that expedition, and Ångström was considered to be the person most competent for calculating out these observations, because the English instruments employed by M. Johannson were very similar to those which Ångström was then using for his own obser vations.

The determinations of constants were executed between the years

1854–56, partly according to methods devised by Ångström. But the fact that the observer had sometimes neglected to determine some corrections which had to be applied to the observations, gave rise to scruples in the mind of the calculator, in consequence of which the work advanced so slowly that it was not finished and published until a short time before his death in 1874.

The report of these labours of Ångström forms part of the work published by the Academy of Sciences of Stockholm, 'A Voyage Round the World in the frigate 'Eugénie.''

In the year 1852 the King of Sweden granted new statutes to the Universities of the kingdom. Hitherto the duties of the assistant professors had not been clearly defined; but by these new statutes they were obliged to give regular public instruction. During the next few years Ångström held the professorship of analytical mechanics. Subsequently when, in consequence of enfeebled health, Professor Adolph Svanberg needed a temporary holiday, Ångström from time to time performed the duties of the Professor of Physics; and on the death of Adolph Svanberg he was appointed Professor of Physics at Upsala, and was at last free to devote all his energies to his favourite studies.

Ångström's most important papers are those on optics and on the theory of heat. His first treatise, published on the occasion of his being made Doctor of Philosophy, was on conic refraction, and that which he published when candidate for the office of Professeur agrégé bore the title 'De theoria lucis calorisque dissertatio.' Both treatises show his great erudition in these subjects. He had an extraordinary fertility of ideas and power of coordination. These intellectual qualities are conspicuous in his suggestive dissertation, 'Essay on a Mathematical Theory of Heat.' This paper was, however, never completed; and Ångström considered that it required to be completely recast.

Closely connected with this last treatise was the note he published in 1842, on the occasion of the Meeting of the Scandinavian Naturalists at Stockholm, and which appeared in Poggendorff's 'Annalen,' vol. viii., "Einige Beobachtungen in Betreff der Wärme und deren Theorie," as well as his researches on the transmission of the heat of one metal to another published, in 1860, in the Transactions of the Society of Sciences at Upsala.

To the theory of heat he contributed another important memoir, 'On the Temperature of the Earth at different depths,' for which he had calculated and worked out the observations on the temperature of the Earth made by Rudberg at Upsala between the years 1837 and 1846.

This work, and the ideas gained from his own previous experiments, doubtless formed the basis of the new method of determining the conducting-power of bodies for heat which he applied to solids in 1861, and which he extended to fluids in 1862. This method has attracted

much attention from physicists, from the fact that the determination of the conducting-power is made independently of their radiating-power.

Ångström's first treatise of any length on optics was that 'On Rectilinear Polarization and on the Double Refraction of Crystals with Three Oblique Axes,' by which he has contributed to the explanation of the optical properties of those crystals. With this Memoir is connected his note 'On the Molecular Constants of Monoclinic Crystals.' He also wrote on the question of the principal properties of the plane of polarization, and made experiments on the capacity of absorption of chlorophyl.

Ångström's important work, 'Optical Researches,' was presented to the Academy of Sciences of Stockholm in 1853. In this work he has shown that the spectrum of the electric spark is formed by the superposition of two spectra—one of them due to the metallic pole, the other to the medium through which the spark passes. Following up the observations made by Wheatstone and Masson, he found that the spectrum obtained from an alloy of two metals chemically combined with one another contains the spectrum of each of the two metals.

In the same memoir, Ångström propounds the theory that the only luminous rays which a vapour or gas can absorb are those which it emits when highly incandescent. It was apropos of this theory that Sir Edward Sabine, in his discourse to the Royal Society on the occasion of the election of M. Ångström as a Foreign Member, remarked that the memoir contains the fundamental principles of all subsequent progress in spectrum-analysis.

The continuation of the spectrum researches, published at short intervals by Ångström between the years 1860 and 1870, are well known to physicists. These are:—in 1861, a memoir on the lines of the solar spectrum; in 1863, a new determination of the lengths of the luminous waves; and in 1865, a memoir, published jointly by Ångström and Thalen, on the violet part of the solar spectrum, which paved the way for the great work which Ångström published in 1868, 'Researches on the Solar Spectrum,' containing the determinations, founded on exact measurements, of the length of waves for the different lines of Fraunhofer. His intention was also to treat in this manner the question of the double spectra of bodies; but death interrupted his labours, leaving him only time to discuss, in the 'Comptes Rendus' of the French Academy of Sciences, some ideas put forth by M. Wüllner.

Another work of the same sort, on the Spectra of the Metalloids, had been begun by Ångström some years previously, and was partly printed before his death. Its publication was completed in 1875 by Thalen, in the Transactions of the Society of Sciences of Upsala. A note of his on the Spectrum of the Aurora Borealis, which he was the first to examine,

is contained in the collection of Memoirs published on the occasion of Poggendorff's Jubilee in 1874.

It is much to be regretted that Ångström, in consequence of his great clearness of mind and facility in business matters, was, during his last years, so much occupied with the administration of the University and with other duties totally unconnected with his scientific work. He was Rector of the University of Upsala in 1870 and 1871, President of the Council of Economic Administration of the University from the year 1869 until his death, Secretary of the Society of Science of Upsala from 1867 also until his death, and a Member of the Administrative Council of the city of Upsala from 1868 to 1873.

Ångström did not for many years receive much recognition of his scientific work; but with time distinctions accumulated. In 1850 he was elected a Member of the Royal Academy of Science of Stockholm, in 1851 of the Royal Society of Science of Upsala, of the Royal Physiographical Society of Lund in 1866, of the Société des Sciences Naturelles de Cherbourg in 1867, of the Royal Academy of Science of Berlin also in 1867, Foreign Member of the Royal Society in 1870, of the Royal Society of Science in Copenhagen in 1873, and Corresponding Member of the French Institute in 1873.

Before becoming a Member of the Upsala Society of Science he received from that learned body pecuniary remuneration for researches communicated to them. Several prizes were awarded to him by the Academy of Science of Stockholm, and in 1870 the Royal Society conferred on him the Rumford Medal.

Angström regularly attended all the meetings of the Scandinavian naturalists. He visited England and France in 1843–44, in 1859, 1866, and 1867. He was a Knight of the Order of the Polar Star, made Commander of the Vasa of the first class in 1873, and Commander of the Italian Order of the Crown. He had a vigorous constitution, and it was only during the last few years of his life that he often complained of violent headache. On the 21st of June, 1874, he died of pachymeningitis, after an illness of three weeks, leaving a widow, one son, and one daughter.

Jean Louis Rodolph Agassiz was born on the 28th of May, 1807, at the village of Motier in the Canton of Freyburg in Switzerland. His father was pastor of Motier, and his ancestors had followed the same profession for six generations. At 10 years of age Jean Louis Rodolph was sent to the Academy of Biel, where he gave evidence of unusual ability. He early showed a strong taste for Natural History, and whilst at Biel began to collect insects; and later on he pursued a systematic study of the plants in the neighbourhood of Orbe near the Jura, to which place his father had moved.

He studied Classics at the Lausanne Academy, and in his 18th year entered on the study of Medicine. He spent two years at Zürich, and thence went to Heidelberg, where he studied Anatomy under Tiedemann, Botany and Zoology under Bischoff and Leuckart. He went in the following year to the University of Munich, where there were at that time many distinguished professors, among them Oken.

At Munich his inclination for the study of Embryology was fostered by his residence in the house of Döllinger. Even at this early period of his career Agassiz showed the strong leaning towards a combination of Natural History and Metaphysics which was his characteristic through life; and whilst studying medicine with a view of making it his profession, he still found time to attend Schelling's course of Mental Philosophy for four consecutive years.

Of his teachers at Munich he has since said,—"Our professors were themselves original investigators, daily contributing to the sum of human knowledge. They were not only our teachers, but our friends. The best spirit prevailed among professors and students. We were often companions of their walks, often present at their discussions; and when we met for conversation or to give lectures among ourselves, as we constantly did, our professors were often among our listeners, cheering and stimulating us in all our efforts after independent research. My room was our meeting-place, bed-room, study, museum, library, lecture-room, fencing-room, all in one: students used to call it 'The Little Academy.'"

At the age of 21 his reputation at the University of Munich was such that Martius entrusted to him the task of describing the fishes collected in Brazil by Spix. This work was published in 1829, under the title "Selecta genera et species Piscium, quos in itinere per Brasiliam peracto collegit et pingendos curavit Dr. J. B. de Spix: digessit, descripsit et observationibus illustravit Dr. L. Agassiz."

Agassiz had already taken the degree of Doctor of Philosophy, and during the following year he passed examinations in Medicine and Surgery.

His work for Martius had led him to make a special study of Ichthyology; and before long he extended his researches from living species to fossil, and entered on that vast field which was to yield him so rich a harvest.

On leaving Munich he resided for a time in Paris, where he acquired the friendship of Cuvier and improved his acquaintance with Humboldt, who became from that time his friend and counsellor for life.

In 1832 he began his career as a teacher. He applied to M. Louis Coulon to obtain for him a position as Professor of Natural History in the Gymnasium of Neufchâtel. No professorship of the kind then existed there; but M. Coulon set to work and raised money enough to guarantee a salary of 2000 francs for three years, and Agassiz was installed as the Professor. There being neither Museum nor Lecture-room, Agassiz

was forced to give his lectures in the Town Hall; but in spite of defective appliances he soon raised his subject to the first rank among those taught in the Gymnasium. He sent for all the specimens he had collected in Germany, constantly added new ones, and by-and-by had a large collection for use. He worked hard at original investigations, constantly employing two artists, Weber and Dinkel, and a painter, Jacques Burkhardt, an old fellow-student at Munich, who became his life-long friend and companion. Stahl, afterwards known as the best modeller at the Jardin des Plantes, was then employed at Neufchâtel; Hercule Nicolet was persuaded to set up there a large lithographic establishment, where were published the last plates of the 'Poissons Fossiles,' those of the 'Poissons d'eau douce,' of the embryology of *Coregonus*, of the works on the Glaciers, and of the Echinoderms.

In 1832, the Société des Sciences Naturelles à Neufchâtel, of which the "Little Academy" at Munich may be said to have been the germ, was founded. The first meeting was held in December, when Louis Coulon was chosen as President and Agassiz as Secretary of the Natural-History Section.

Agassiz held his professorship at Neufchâtel from 1832 until 1846, and during that period got through an enormous amount of work. His work for Martius had led him into palæontology; and the result of his extensive study of fossil fishes was the discovery that the scales of fishes correspond by four kinds of structure to four large natural divisions, which he called Ganoids, Placoids, Cycloids, and Ctenoids. With this basis and with the aid of his intimate knowledge of the skeleton he was enabled to tabulate all the known fossil species to the number of 1000, and these results he published as 'Recherches sur les Poissons Fossiles,' in 5 vols., with about 400 excellent plates. This work occupied ten years in going through the press. It contains the germs of many of the theories he subsequently advocated so zealously in his public lectures. In the preface is found the first notice of his theory of the correspondence between geological succession and embryological development.

The preparation of this book involved an immense amount of work. He had to travel with an artist in order to examine and copy the specimens which could not be sent to Neufchâtel. The expense also was so far beyond his means, that he incurred heavy debts which hampered him for many years. They were not discharged until he had spent many years in the United States, travelling from place to place, giving public lectures on Natural History. At last, finding that this constant travelling interfered too much with his duties at Harvard, he established at Cambridge a school for young ladies, he himself teaching botany, physiology, and geology. This proved eminently successful, and relieved him from all his embarrassments.

After the publication of his work on 'Poissons Fossiles' he came to England to study the fossils of this country, and in 1844 published an

elaborate account of those discovered in the Old Red Sandstone of the Devonian system.

In the midst of all this heavy work he entered on other investigations. He had already turned his attention to the vast ice-masses which furrow the sides of the Swiss mountains, and in 1834 made a report on the observations of Hugi concerning the structure of glaciers. In 1837 he had as President to give the opening discourse to the members of the Helvetic Society of Natural History assembled at Neufchâtel. It was the celebrated "Discours sur l'ancienne Extension des Glaciers." In this discourse he carried to their logical conclusion the facts already observed by Venetz and Charpentier, that boulders are transported and rocks scratched and polished by glacial action; and inasmuch as Switzerland is strewn with these boulders, and exhibits in many places the scratchings and polishing of rock surfaces, he boldly asserted that the whole of Switzerland and also the northern parts of Europe had been covered in former ages by a sheet of ice of vast thickness. This heresy fell like a thunderbolt on the Assembly. Leopold von Buch, the greatest geologist of that time, lost all control over himself, and severely denounced the new theory. When shown the scratched surfaces near Neufchâtel, he replied that the slides of the schoolboys had made them; and he retired at last exclaiming, "O Sancte de Saussure, ora pro nobis." This violent opposition only spurred Agassiz to fresh exertions; and for eight successive seasons he made a series of explorations in the neighbourhood of Mont Blanc and in the Bernese Oberland. With the determination of ascertaining the intimate structure and the movements of ice formations, he established himself in the summer of 1840 on the Median Moraine of the Aar Glacier, and lodged his party, consisting of Desor, Vogt, Burkhardt, and Celestin Nicolet, under a large block of gneiss. This comfortless abode, which was invaded by frost at night and by trickling water in the daytime, was facetiously called "L'hôtel des Neufchâtelais." In 1842 a hut was built on the bank which overhangs the left side of the glacier, and this served as a shelter during the remainder of their visits.

In 1840 and 1841 Agassiz published in French and German his 'Études sur les Glaciers,' accompanied by fine plates. His 'Système Glaciaire,' with its maps and illustrations, did not appear until 1847.

In the midst of these geological and palæontological studies Agassiz found time for a series of careful experiments in moulding. In 1839 his paper appeared, "Sur les Moules de Mollusques vivans et fossiles." In this paper he showed that the soft parts of Mollusca impress their form on the interior of the shell, which form can be reproduced by a cast whose inequalities will represent those of the original animal; so that the casts of mollusks found in great numbers in certain formations could no longer be considered worthless. He first made interior casts of living shells, studied them side by side with the animals, and applied the knowledge of

the animals thus obtained to the fossil casts. This essay was followed by his 'Études critiques sur les Mollusques fossiles.'

At about the same period his attention was attracted to the study of the Radiata and of Embryology. Under his direction experiments were conducted through several seasons on artificially fecundated eggs of the Swiss White-Fish, *Coregonus palœa*, by Karl Vogt; and in 1842 the account was published as a part of the 'Poissons d'eau douce.' It is worth noting that the Government of Neufchâtel in that same year issued directions to fishermen for the impregnation of fish eggs.

Agassiz had turned his attention to Echinoderms almost as early as to Fishes. In 1834 he had published a paper, "Ueber die äussere Organisation der Echinodermen," in the 'Isis;' and in 1839 he published an admirable anatomical essay on *Astrophyton*. He prosecuted these investigations with extraordinary zeal, and was aided by Desor. Between the years 1838 and 1842 appeared his "Monographies d'Echinodermes vivans et fossiles." From the beginning of his studies he felt the need of having a systematic record of what specialists in palæontology and zoology were doing. He commenced an index, arranged alphabetically and zoologically, of all the generic names introduced into science from the time of Linnæus; this was accompanied by the proper bibliographical references, and formed the commencement of the reform in zoological nomenclature, which was becoming entirely unmanageable. The work was revised and enlarged by 22 of his colleagues, each of whom took a group. The whole was completed in 1846, under the name of "Nomenclator Zoologicus." The bibliographical part was subsequently published by the Ray Society; it was edited by the late Mr. Strickland.

In 1845, on the suggestion of Lyell, Agassiz had been invited by Mr. John A. Lowell to come to Boston and deliver lectures before the Lowell Institute. About the same time, the King of Prussia had been persuaded by Humboldt to present him with a sum of money in aid of a Scientific Mission to America. Agassiz was thus induced to cross the Atlantic Ocean in 1846; and when in 1847 Mr. Abbot Lawrence offered to found for him a professorship of Zoology and Geology in the Scientific School at Harvard College, he obtained his release from his engagements in Europe and accepted the position.

In 1848 he explored Lake Superior, and his account of the observations made there was edited by Mr. J. Elliot Cabot. At the request of Prof. Bache, of the Coast Survey, he passed the winter of 1850 among the Florida Reefs, where he determined the law of growth by which that peninsula has gradually extended southwards by the successive formation of reefs, keys, and mudflats.

In 1852 he was appointed Professor of Comparative Anatomy at the Medical College of Charleston. This appointment did not interfere with his duties at Cambridge, his lectures at Charleston being delivered during the winter vacations of Harvard College. He remained there for only

two terms, when he was compelled by the state of his health to give up the appointment.

In 1853 he published a paper on the newly discovered viviparous fishes of California, and also began to work up the vast amount of materials which he had collected in America. The publications of the American Academy of Boston, of the Boston Society of Natural History, and of the American Association for the Advancement of Science contain a large number of papers on the Natural History of the United States. He was also for many years an active contributor to the pages of the 'American Journal of Arts and Sciences.' He sketched out a series of essays, which were to be entitled "Contributions to the Natural History of the United States." 2500 names appeared on the subscription list of this work. He hoped to publish ten volumes of this series, but only actually published four, leaving a fifth unfinished. These four are:—An Essay on Classification; North-American Testudinata; the Embryology of Turtles; and 'The Acalephs,' under which were included Monographs of the Ctenophoræ, Discophoræ, and Hydroidæ, and an essay on the Homologies of the Radiata. His chief assistant in their preparation was the late Prof. H. J. Clark. These books were illustrated by first-rate lithographic plates, the best of which were drawn by Sonrel.

The 'Essay on Classification' is the crowning work of Agassiz. The erudition displayed in this work is remarkable, and the grasp of facts, intricate and numerous in their relations, is quite amazing. In nothing is this better exhibited than in his celebrated demonstration of embryological, geological, and zoological succession. He shows that in many orders the species which first appear in the older beds resemble the embryo of the highest species now living; and, moreover, that this fossil and this embryo have characteristics in common with the living species that stand lower in the zoological scale. Thus, among living Crustacea, the Brachyurans stand highest; but the embryo of the Brachyuran has a long tail like the Macrurans, which are characteristic of the middle geological periods, and the living forms of which are zoologically inferior to the Brachyurans.

The last years of Agassiz's life were devoted to founding a large museum arranged to show his views of the relations of living animals among themselves, and their connexions in the geological and embryological successions.

His own collections, which had gradually outgrown the means of a single individual, formed the nucleus of a Museum of Comparative Zoology. An endowment-fund was commenced by the generosity of one of his friends, Mr. F. C. Gray, the cooperation of Harvard College and the State of Massachusetts adding to its means at different times. On the death of Agassiz a "Memorial Fund" was raised by subscription, ample enough, it is hoped, to carry out some at least of his cherished ideas.

This Museum has been the nursery for nearly all the Professors of

Natural History in the United States, most of whom are pupils of Agassiz. He did more than any one man to break down the old curriculum of the public schools, and to force the introduction of the teaching of science. His own lectures introduced popular scientific disquisitions into the Lyceum courses, so universal in America. He was an admirable lecturer, fascinating to his audience, and carrying them along by his own enthusiasm; and few even of the public men of America had such a deep hold upon the community as Agassiz. His management of the State Legislature, upon whom he long depended for the support of the museum, was as simple as it was effective. Farmers, merchants, lawyers, and business men, who cared very little for Natural History, freely gave to Agassiz, whose motives were always unselfish, and whose sole aim was the advancement of science.

His wish was to leave this museum as a legacy to the people of America. He gradually gave less time to special investigations and more to the museum, which was to show the forms of life in a connected order.

He brought back a vast collection of specimens from his expedition to Brazil, the results of which expedition are described by Agassiz and his wife in the work 'A Journey to Brazil.' This expedition was undertaken at the cost of Mr. Nathaniel Thayer, who unasked, offered to defray all the expenses, personal and scientific, of six assistants; and eventually did even more than he had promised, continuing to meet all the expenses that were incurred until the last specimen was stored in the Cambridge University. But not even the sight of familiar fishes, that reminded him of old times at Munich and of Spix and Martius, could turn Agassiz again to special studies. He worked on as indefatigably as ever, keeping up his relations with the public, his interest in education, his voluminous correspondence, and giving popular lectures.

But the strain of all this was too great, and in 1869 he was seized with a cerebral attack. He, however, recovered from this, and in the year 1871 joined an exploring-expedition under the direction of the Coast Survey. The steamer 'Hassler' was fitted out expressly to make deep-sea dredgings along the coasts of South America and the west coast of North America. Agassiz had become deeply interested in the results of the deep-sea dredgings he made in 1866 and 1867 in the Straits of Florida, during an expedition under the direction of the Coast Survey, in which he was accompanied by Count Pourtalès. Although the plans of the 'Hassler' Expedition were most carefully matured, it was not a success as far as dredging-operations were concerned. He submitted cheerfully to all the hardships of the voyage round Cape Horn to California, and came back laden with fresh collections. In the course of this expedition a careful exploration was made of the Sargasso Sea, and a nest-building fish discovered, and other important contributions made to Natural Science. His life ended happily; he saw his Museum well supplied with funds and rapidly progressing in size and order. By the aid of the gift of

the Island of Penikese and of a large sum of money, he was enabled to found a summer school of Natural History. The school started with about fifty pupils, and Agassiz had the gratification of founding the first school of the kind in the world. This additional strain on his powers at a season when Agassiz had usually taken a holiday from his ordinary work was too much for his already enfeebled health. On Tuesday, December 2nd, four days before he was attacked with his last illness, he gave an address before the Massachusetts State Board of Agriculture, at Fitchburgh, where he lectured not only with ease but with an unwonted energy, an evidence, no doubt, of cerebral disturbance. This over-exertion was so apparent that, by order of his physicians, he gave up an engagement to lecture at New Haven on the 8th. On the 5th he met his students, and on the 6th, while at work in the Museum of Comparative Zoology, he was taken suddenly ill and retired immediately to his house and bed, never to leave them alive.

His family physician, Dr. Morril Wyman, and his old friend Dr. Brown-Séquard were almost constantly with him during his last illness. He died on the 14th December, 1873.

His funeral was attended at Appleton Chapel, Harvard University, by a vast assembly of mourning friends from Boston and many other towns far and near. The flags of the Municipality of Boston were hung half-mast high, and the bells were tolled during the obsequies. To the solemn music of the "Dead March in Saul" the family and a few near friends, with the University Authorities, left the Chapel for Mount Auburn Cemetery, where now rest the remains of L. J. R. Agassiz.

At the time of his death Agassiz was engaged in his discussion of the "Evolution of Types," the first paper of which appeared in the 'Atlantic Monthly' for December.

Agassiz was much opposed to the theories of Darwin. His old scientific friends, who, one after another, joined the evolutionists, never could understand how he, who had so early in his career pointed the way to what is now one of the strongest proofs of evolution, could resist his own arguments. As a matter of history, it is an interesting record to turn to the pages of his German edition of Buckland's 'Mineralogy and Geology,' and read the notes, many of which would pass as the work of the most advanced evolutionist. But in his later years he was eminently a theistic philosopher. His argument against the doctrine of evolution has been thus described:—

1. There is order and system in organic nature, such as indicates thought. 2. The evolution of species of plants and animals one from another by natural causation is tantamount to a denial of this. 3. Therefore doctrines of evolution are untrue.

Agassiz received the Monthyon and Cuvier prizes and the Copley and Wollaston Medals. He was a Foreign Member of the Royal and Linnean Societies, a Foreign Associate of the French Academy of Sciences, and a member of most other learned Societies and Academies.

Agassiz early married a gifted sister of Professor Alexander Braun, by whom he had three children, who survived him—two daughters, and a son, who has made numerous valuable contributions to our knowledge of Comparative Anatomy and Embryology. His second wife (née Cary), the ever helpful companion and associate in voyage and travel and in literary and scientific work, survives him.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

May 4, 1876.

Capt. F. J. O. EVANS, R.N., C.B., Vice-President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair as follows:—

Capt. William de Wiveleslie Abney, R.E.
Prof. Henry Edward Armstrong, Ph.D.
Rev. William B. Clarke, M.A., F.G.S.
James Croll, F.R.S.E.
Edwin Dunkin, Sec. R.A.S.
Prof. John Eric Erichsen, F.R.C.S.
David Ferrier, M.A., M.D.
Col. Augustus H. Lane Fox.
Prof. Alfred Henry Garrod, M.A.
Robert Baldwin Hayward, M.A.
Charles Meldrum, M.A., F.R.A.S.
Edward James Reed, C.B.
Prof. William Rutherford, M.D.
Robert Swinhoe, F.R.G.S.
Prof. Thomas Edward Thorpe, Ph.D.

The following Papers were read:—

I. "Supplementary Note on the Theory of Ventilation" (see Paper read on the 28th of January, 1875). By FRANCIS S. B. FRANÇOIS DE CHAUMONT, M.D., Surgeon-Major, Army Medical Department, and Conjoint Professor of Hygiene, Army Medical School. Communicated by Prof. STOKES, Sec. R.S. Received March 8, 1876.

In my previous paper I endeavoured to establish a basis for calculating the amount of fresh air necessary to keep an air-space sufficiently pure

for health, taking the carbonic acid as the measure. The results showed that the mean amount of carbonic acid as respiratory impurity in air undistinguishable by the sense of smell from fresh external air was under 0·2000 per 1000 volumes *. My object in the present note is to call attention to the relative effects of temperature and humidity upon the condition of air, as calculated from the same observations.

If we adopt the figures of Class No. 1 (that is "fresh," or not differing sensibly from the external air) we find the following :—

Temperature.	Humidity.	Carbonic acid.
63° F.	73 per cent.	0·1943 per 1000 volumes.

If, now, we arrange the observations according as they differ from the above standard of temperature and humidity, and note the record of sensation attached to each, we may ascertain how far the said record departs (if at all) from what it ought to have been as calculated from the actual CO_2. To do this we may employ the numerical values of the different classes, taking No. 1 (fresh) as unity, thus :—

Class.	Sensation.	Value.
No. 1.	Fresh	1·00
2.	Rather close	2·13
3.	Close	3·46
4.	Extremely close	4·66

Taking each observation and dividing the CO_2 found by the mean quantity of No. 1, viz. 0·1943, we get a number which will give the *theoretical* value of its effect upon the se nes; and by comparing this with the *actual* value of the *recorded* sensation, we can note whether the difference is *plus* or *minus*, if any. All observed quantities of CO_2 below 0·1943 are considered equal to that number, and all quantities above 0·9054 as equal to it, as the sense of smell does not seem capable of differentiating quantities except between those limits.

Out of 458 fully recorded cases, 186 gave a recorded sensation *in excess* of the theoretical value—that is, the air seemed less pure than would have been expected from its CO_2. In these the average temperature and humidity were both above Class 1.

* In the former paper the amount was given at 0·1830 per 1000; but on revising the calculations, a previously unobserved error was found in one of the constants employed, the correction for which would have the effect of altering the figures a little, the changes being as follows.—

Classes.		Respiratory impurity as CO_2. Original figures.	Corrected figures.
No. 1.	Fresh	0·1830	0·1943
2.	Rather close	0·3894	0·4132
3.	Close	0·6322	0·6708
4.	Extremely close	0·8533	0·9054

Except for the sake of rigid accuracy the difference is immaterial, as I adopted 0·2000 as the limit of respiratory impurity in an air-space well ventilated, and the corrected number 0·1943 is still below that.

152 cases gave a recorded sensation *below* the theoretical value—that is, the air seemed purer than would have been expected from its CO_2. In those cases the average temperature was above, but the average humidity below the mean of Class 1.

120 cases gave a recorded sensation that exactly corresponded with the theoretical value. In those cases the average temperature was above and the average humidity below the mean of Class 1.

Arranging these results and putting F for the temperature in degrees of Fahrenheit, and H for the humidity per cent., we have :—

$$+\ 58^\circ{\cdot}6\ F + 86\ H = +\ 197{\cdot}70 \quad [1] \quad \left\{ \text{Aggregate difference of the recorded and the theoretical value of sensation.} \right.$$

$$+\ 230{\cdot}8\ F - 82\ H = -\ 117{\cdot}37 \quad [2] \quad \text{Do.}$$

$$+\ 244{\cdot}0\ F - 91\ H = 0 \quad [3] \quad \text{Do.}$$

Adding the two last equations, we have,

$$+\ 474^\circ{\cdot}8\ F - 173\ H = -\ 117{\cdot}37 \quad [4] \quad \text{Do.}$$

From [1] and [4] we can determine the respective values of F and H, which are as follow :—

$$F = 0{\cdot}4730 \qquad H = 1{\cdot}9765$$

Or, stated in terms of CO_2, by multiplying by 0·1943,

$$F = 0{\cdot}0919 \qquad H = 0{\cdot}3833 \text{ per } 1000 \text{ vols.}$$

Taking F as unity, we have,

$$F : H :: 1{\cdot}0000 : 4{\cdot}1789$$

Or an increase of 1 per cent. of humidity has as much influence on the condition of an air-space (as judged of by the sense of smell) as a rise of 4°·18 of temperature in Fahrenheit's scale, equal to 2°·32 Centigrade, or 1°·86 Réaumur.

This may be taken as a proof of the powerful influence exercised by a *damp* atmosphere, corroborating the conclusions arrived at by ordinary experience; and it follows that as much care ought to be taken to ensure proper hygrometric conditions as to maintain a sufficiently high temperature. This is especially the case in the wards or chambers of the sick, in which regular observations with the wet and dry-bulb thermometers ought to be made; these would probably give a valuable indication of the condition of the ventilation, either along with or in the absence of other more detailed investigations. Thus a room at the temperature of 60° F. and with 88 per cent. of humidity contains 5·1 grains of vapour per cubic foot: suppose the external air to be at 50° F. with the same humidity, 88 per cent.; this would give 3·6 grains of vapour per cubic foot; to reduce the humidity in the room to 73 per cent., or 4·2 grains per cubic foot, we must add the following amount of external air,

$$\frac{5{\cdot}1 - 4{\cdot}2}{4{\cdot}2 - 3{\cdot}6} = 1{\cdot}5,$$

or once and a half the volume of air in the room. If the inmates have each 1000 cubic feet of space, it follows that either their supply of fresh air is short by 1500 cubic feet per head per hour, or else that there are sources of excessive humidity within the air-space which demand immediate removal.

II. "On the Effect of Heat on the Chloride, Bromide, and Iodide of Silver." By G. F. RODWELL, F.R.A.S., F.C.S., Science Master in Marlborough College. Communicated by Professor TYNDALL, F.R.S. Received March 10, 1876.

III. "On the Effects of Heat on some Chloro-brom-iodides of Silver." By G. F. RODWELL, F.R.A.S., F.C.S., Science Master in Marlborough College. Communicated by FREDERICK GUTHRIE, F.R.S., Professor of Physics in the Royal School of Mines. Received April 13, 1876.

IV. "On the Absorption-Spectra of Bromine and Iodine Monochloride." By H. E. ROSCOE, F.R.S., and T. E. THORPE. Received March 16, 1876.

(Abstract.)

The paper contains the results of an exact series of measurements of the absorption-spectra of the vapours of the element bromine and of the compound iodine monochloride, made with the object of ascertaining whether the molecules of these two gases vibrate identically or similarly, their molecular weights and colour of the vapours being almost identical. The two spectra, which are both channelled, were compared simultaneously by means of one of Kirchhoff's 4-prism spectroscopes, the position of the lines being read off by reflection on an arbitrary scale. In order to determine the wave-lengths of these bands, the wave-length of each of 27 air-lines lying between the extremes of the absorption-spectra was ascertained by reference to Thalén's numbers; whilst for the purpose of reducing the readings of the absorption-bands to wave-lengths a graphical method was employed, the details of which are given in the paper. This method appears to be one of general applicability for the plotting of spectra.

Tables then follow giving the wave-lengths of 66 bands of each absorption-spectrum; and a map accompanies the text in which the bands are drawn to a scale one half that of Ångström's "Spectre Normal."

A careful comparison of these Tables and of the map shows that, although both spectra contain a large number of lines which are nearly coincident, the spectra as a whole are not identical, either when the vapours are examined at high or low temperatures, or when the lengths of the columns of absorbing gas are varied.

V. "On the Origin of Windings of Rivers in Alluvial Plains, with Remarks on the Flow of Water round Bends in Pipes." By Professor JAMES THOMSON, LL.D., F.R.S.E. Communicated by Prof. Sir WILLIAM THOMSON, F.R.S. Received March 14, 1876.

In respect to the origin of the windings of rivers flowing through alluvial plains, people have usually taken the rough notion that when there is a bend in any way commenced, the water just rushes out against the outer bank of the river at the bend, and so washes that bank away, and allows deposition to occur on the inner bank, and thus makes the sinuosity increase. But in this they overlook the hydraulic principle, not generally known, that a stream flowing along a straight channel and thence into a curve must flow with a diminished velocity along the outer bank, and an increased velocity along the inner bank, if we regard the

Fig. 1.

flow as that of a perfect fluid. In view of this principle, the question arose to me some years ago:—*Why does not the inner bank wear away more than the outer one?* We know by general experience and observation that in fact the outer one does wear away, and that deposits are often made along the inner one. *How does this arise?*

Fig. 2.

The explanation occurred to me in the year 1872, mainly as follows:— For any lines of particles taken across the stream at different places, as

A_1B_1, A_2B_2, &c. in fig. 2, and which may be designated in general as AB, if the line be level, the water-pressure must be increasing from A to B, on account of the centrifugal force of the particles composing that line or bar of water; or, what comes to the same thing, the water-surface of the river will have a transverse inclination rising from A to B. The water in any stream-line C D E* at or near the surface, or in any case not close to the bottom, and flowing nearly along the inner bank, will not accelerate itself in entering on the bend, except in consequence of its having a *fall of free level* in passing along that stream-line†.

But the layer of water along the bottom, being by friction much retarded, has much less centrifugal force in any bar of its particles extending across the river; and consequently it will flow sidewise along the bottom towards the inner bank, and will, part of it at least, rise up between the stream-line and the inner bank, and will protect the bank from the rapid scour of that stream-line and of other adjacent parts of the rapidly flowing current; and as the sand and mud in motion at the bottom are carried in that bottom layer, they will be in some degree brought in to that inner bank, and may have a tendency to be deposited there.

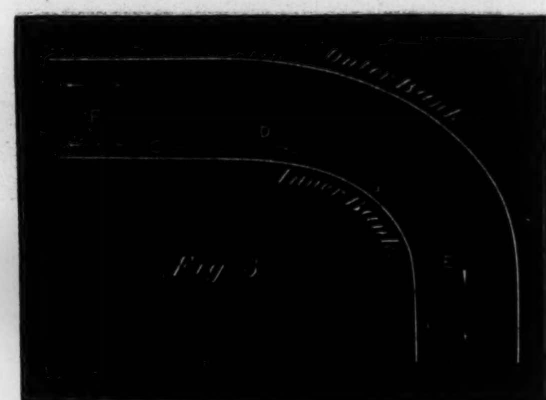


Fig. 3

On the other hand, along the outer bank there will be a general tendency to descent of surface-water which will have a high velocity, not having been much impeded by friction; and this will wear away the

* This, although here conveniently spoken of as a stream-line, is not to be supposed as having really a steady flow. It may be conceived of as an average stream-line in a place where the flow is disturbed with eddies or by the surrounding water commingling with it.

† It must be here explained that by the *free level* for any particle is to be understood the level of the atmospheric end of a column, or of any bar, straight or curved, of particles of statical water, having one end situated at the level of the particle, and having at that end the same pressure as the particle has, and having the other end consisting of a level surface of water freely exposed to the atmosphere, or else having otherwise atmospheric pressure there; or, briefly, we may say that the *free level* for any particle of water is the level of the atmospheric end of its *pressure-column*, or of an equivalent ideal pressure-column.

bank and carry the worn substance in a great degree down to the bottom, where, as explained before, there will be a general prevailing tendency towards the inner bank.

Now, further, it seems that even from the very beginning of the curve forward there will thus be a considerable protection to the inner bank. Because a surface stream-line C D, or one not close to the bottom, flowing along the bank which in the bend becomes the inner bank, will tend to depart from the inner bank at D, the commencement of the bend, and to go forward along D E, or by some such course, leaving the space G between it and the bank to be supplied by slower-moving water which has been moving along the bottom of the river perhaps by some such oblique path as the dotted line F G.

It is further to be observed that ordinarily or very frequently there will be detritus travelling down stream along the bottom and seeking for resting-places, because the cases here specially under consideration are only such as occur in alluvial plains; and in regions of that kind there is ordinarily*, on the average, more deposition than erosion. This consideration explains that we need not have to seek for the material for deposition on the inner bank in the material worn away from the outer bank of the same bend of the river. The material worn from the outer bank may have to travel a long distance down stream before finding an inner bank of a bend on which to deposit itself. And now it seems very clear that in the gravel, sand, and mud carried down stream along the bottom of the river to the place where the bend commences, there is an ample supply of detritus for deposition on the inner bank of the river even at the earliest points in the curve which will offer any resting-place. It is especially worthy of notice that the oblique flow along the bottom towards the inner bank begins even up stream from the bend, as already explained, and as shown by the dotted line F G in fig. 3. The transverse movement comprised in this oblique flow is instigated by the abatement of pressure, or lowering of free level, in the water along the inner bank produced by centrifugal force in the way already explained.

It may now be remarked that the considerations which have in the present paper been adduced in respect to the mode of flow of water round a bend of a river, by bringing under notice, conjointly, the lowering of free level of the water at and near the inner bank, and the raising of free level of the water at and near the outer bank relatively to the free level of the water at middle of the stream, and the effect of retardation of velocity in the layer flowing along the bed of the channel in diminishing the centrifugal force in the layer retarded, and so causing that retarded water, and also frictionally retarded water, even in a straight channel of approach to the bend, to flow obliquely towards the inner bank, tend very

* That is to say, except when by geological changes the causes which have been producing the alluvial plain have become extinct, and erosion by the river has come to predominate over deposition.

materially to elucidate the subject of the mode of flow of water round bends in pipes, and the manner in which bends cause augmentation of frictional resistance in pipes, a subject in regard to which I believe no good exposition has hitherto been published in any printed books or papers; but about which various views, mostly crude and misleading, have been published from time to time, and are now often repeated, but which, almost entirely, ought to be at once rejected.

VI. "On the Modification of the Excitability of Motor Nerves produced by Injury"*. By G. J. Romanes, M.A., F.L.S. Communicated by Prof. Sanderson, M.D., F.R.S. Received April 13, 1876.

§ 1. If the gastrocnemius of a frog be placed in a horizontal direction on non-polarizable electrodes with its convex surface uppermost, one may generally observe that the muscle is somewhat more sensitive to minimal stimulation, supplied by *closure* of the constant current, when the femoral end rests on the kathode, than when this end rests on the anode. Conversely, under similar circumstances the gastrocnemius is more sensitive to minimal stimulation, supplied by *opening* of the constant current, when the femoral end rests on the anode, than when this end rests on the kathode. In view of the other facts of electrotonus, the present ones are of interest; because, as the sciatic nerve enters the gastrocnemius near the femoral end of the latter, and then spreads out its peripheral ramifications as it advances, in the experiments just mentioned one electrode is in almost immediate contact with the nerve-trunk where it enters the muscle, while the other electrode supports the part of the muscle that contains only peripheral nervous elements. It is therefore to be expected, upon the theory of electrotonus, that the muscle under these conditions should prove itself most sensitive to the closing shock when the nerve-trunk rests on the kathode, and most sensitive to the opening shock when the nerve-trunk rests on the anode.

It is to be observed, however, that although this expectation is in most cases fulfilled, it is not so invariably. Different gastrocnemius muscles, though treated as far as possible in exactly the same way, manifest considerable differences, both in their general sensitiveness to electrical stimulation, and in their relative sensitiveness to interruptions of the ascending and of the descending currents. Even the same muscle, if rapidly prepared, will generally be found to undergo fluctuations in these respects from minute to minute. Attributing this fact to the unnatural conditions which the experiment imposed on the process of nutrition, I conducted some observations on muscles while they were still attached to the body

* For further details, remarks, statements of methods, &c., see a fuller notice in the forthcoming (July) Number of the 'Journal of Anatomy and Physiology.'

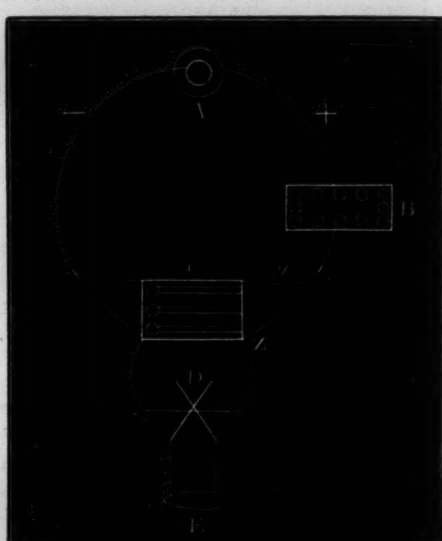
of the frog; but the results yielded by this method were not more uniform than those which I had previously obtained by the method of rapidly preparing and observing excised muscles.

§ 2. If the gastrocnemius of a frog be placed on non-polarizable electrodes in the position already described in § 1, and if care has been taken not to injure the attached sciatic nerve, I find that upon now dividing this nerve, either near or just within the muscle, remarkable alterations ensue, not only, as is already known, in the *general* sensitiveness of the muscle, but also, and more particularly, in its *relative* sensitiveness to make and to break of the current. The following are the mean results yielded by a large number of experiments:—

Descending make		Ascending make		Descending break		Ascending break	
before cutting.	after cutting.	before cutting.	after cutting.	before cutting.	after cutting.	before cutting.	after cutting.
24	27	36	46	2	32	1	1½

In this Table the word "descending" means passage of the current from the femoral to the tarsal end of the gastrocnemius, and "ascending," of course, passage of the current in the opposite direction. "Cutting" means section of the sciatic nerve just after it enters the muscle; and the numbers represent the relative sensitiveness of the muscle to the stimuli which are indicated above them*. I have appended a diagram (p. 11),

* The numbers are thus obtained:—Suppose A to be the battery, B a set of resistance-coils, C a rheochord, D a commutator, and E the muscle. By removing a plug



from B the resistance is increased, and therefore the current through E is diminished. But the effect of removing a plug from C, although likewise that of increasing the resistance through the whole circuit, is to *augment* the current passing through E. For, previous to removing a plug from C, the current branched at x, and the resistance

which is intended to represent, in a graphic form, the numerical relations set forth in the above Table. In each couplet contained in that diagram the left-hand line represents the sensitiveness of the muscle to the stimulus indicated before cutting, while the right-hand line represents the sensitiveness of the muscle to the same stimulus after cutting. As in the Table, so in the diagram, all the proportions are referred to the ascending break as to a unit—this being the stimulus to which the muscle is least sensitive, and for which, therefore, the strongest current is required in order to elicit a contraction.

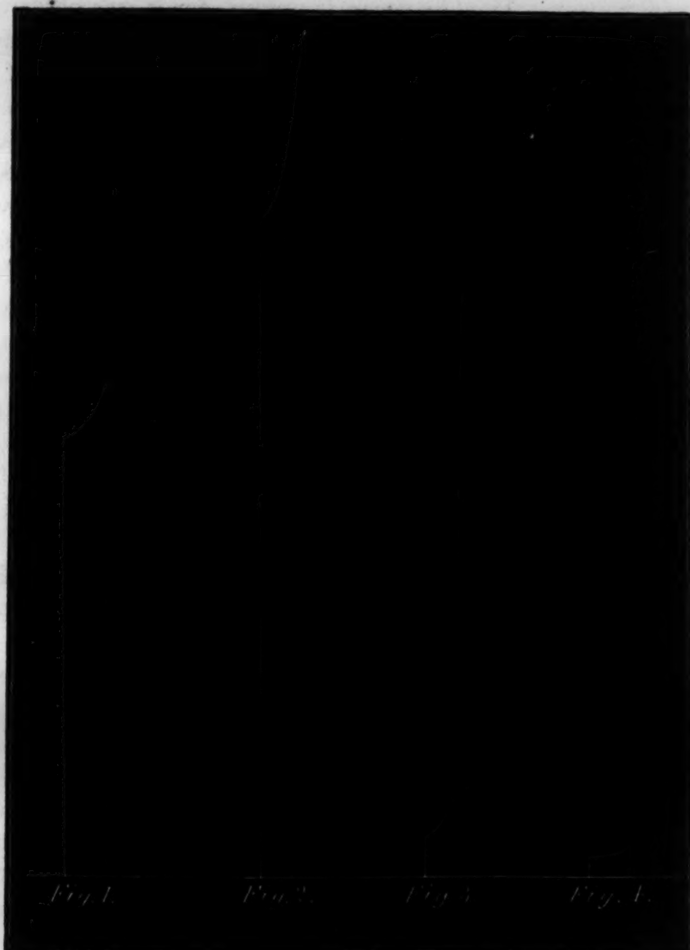
With regard to these results, I may offer the following observations. In the first place, it is evident that the increase of excitability shown by the muscle after cutting is affected to an extraordinary extent by the *direction* of the current; and, further, that the manner in which it is so affected is very instructive when considered in relation to the known facts of electrotonus. For just as before cutting the *normal* sensitiveness of the muscle is greatest to the closing excitation when its femoral end (or nerve-trunk) rests on the kathode, and to the opening excitation when this end rests on the anode, so after the general sensitiveness has been exalted by cutting the *exaltation* shows itself in a far higher degree to the closing excitation when the femoral end (or severed nerve-trunk) rests on the kathode, and to the opening excitation when this end rests on the anode. Thus it is that the curves in figs. 2 and 3 are so much steeper than those in figs. 1 and 4. The only fact, then, that does not seem to admit of any very satisfactory explanation is the altogether *disproportionate* increase of excitability which the muscle after cutting exhibits to the descending break (fig. 3) as compared with the ascending make (fig. 2). This fact, therefore, led to the following experiments.

§ 3. Dr. Burdon Sanderson suggested that if we suppose the breaking excitation to be of a more *instantaneous* nature than the making one, the fact in question might admit of a probable explanation; for in this case the breaking stimulus would bear more resemblance to an induction-shock than would the making stimulus; and as it is well known how sensitive nerve is to the induction-shock, we might reasonably conclude that, when the sensitiveness of the nerve is increased by section, it would

in E being high as compared with that in C, the principal part of the current takes the course x, y, C, A. But if a plug be removed from C, the resistance in C is increased, and a proportional amount of the current takes the direction x, z, E, A. Hence the effect of removing a plug from B is that of diminishing the current in E, while the opposite effect results on removing a plug from C.

Such being the apparatus, in all my experiments I removed one plug from B, and thus worked with a current of constant intensity so far as the whole circuit was concerned. The requisite variations in the intensity of the stimuli were, of course, effected by the rheochord C. Now the numbers in the above Table are obtained by a very simple calculation. Suppose, for instance, that the minimal ascending break contraction requires 18 ohms' resistance to be thrown into the rheochord, while the minimal ascending make only requires ·5 to be thrown in, then the relative sensitiveness of the muscle to the ascending break and make would be approximately represented by the numbers 1 : 36.

probably become more than proportionally increased to the more sudden stimulus. In order to test the correctness of this hypothesis, Dr. Sanderson further suggested that the period of the muscle's latent stimulation before and after cutting should be taken, and also that the following experiment should be tried. By means of an appropriate apparatus,



1. Descending make. 2. Ascending make. 3. Descending break. 4. Ascending break.

the uncut muscle was to have supplied to it a galvanic stimulus of measured duration; and this duration was to be graduated down to the point at which the break succeeded the make with a rapidity just sufficiently great to prevent the muscle from responding to either stimulus. The strength of the current remaining unaltered, the nerve was then to be cut through at the usual place; and, lastly, it was to be observed whether or not the muscle was thus rendered more sensitive to stimuli of short duration. So far as this part of the inquiry has as yet proceeded, the results are as follow.

Section of the nerve (either just above the knee or immediately after

it enters the muscle) is in all cases attended with a marked increase of sensibility to stimuli of short duration; *i. e.* stimuli of much shorter duration are able to evoke responsive contractions in the muscle after cutting than are required to do so before cutting. At first, therefore, it seemed that this experiment was confirmatory of the hypothesis which it was designed to test. This, however, is not so; for it was observed that the increased sensitiveness in question was only shown when the femoral end of the muscle rested on the kathode, while it was scarcely, if at all, apparent when this end rested on the anode. This fact, of course, led to the inference that the augmented excitability to stimuli of short duration had reference, not to the opening, but to the closing excitation. Accordingly I fitted up an appropriate arrangement of wires and keys, by which I could at pleasure throw in ordinary opening and closing excitations, or the closing and opening excitations of short duration. In this way it was easy, by comparing in the two cases the nature of the contractions (which in almost every muscle presents some idiosyncratic differences on make and break), to obtain an optical proof that my inference was correct. The exalted sensitiveness of the muscle after section of its nerve to stimuli of short duration had reference exclusively to the closing excitation.

This fact is of interest in itself, but it fails to answer the question as to why section of a nerve causes so disproportionate an effect on its sensitiveness in the muscle to the excitation which is supplied by the descending break. Nor have I any satisfactory answer to give to this question, unless the following consideration may be deemed so. Before section of the sciatic nerve, the gastrocnemius muscle is immensely more sensitive to the ascending make than to the descending break (figs. 2 and 3, left-hand lines). Consequently, when the general sensitiveness of the nerve is increased by section, the increase has not so much room (so to speak) for its occurrence in the one case as in the other. Seeing that the minimal make contraction occurs at a point so much nearer to zero of the current's intensity than does the minimal break contraction, when both these minimals are reduced still further by nerve-section, the latter minimal has a much wider range through which it is free to fall than has the former. Of course this fact need not prevent the lesser fall from being numerically proportional to the greater one, however small the observed differences may be. The question, however, is as to how far a strictly *numerical* proportion is in this case a fair one. I think we must certainly hold that the value as a stimulus of any given increment of current is determined by the proportion which such increment bears to the intensity of current that is required to produce adequate stimulation. In other words, any given unit of electrical intensity has more influence as an excitant if added to a current of a small number of units (a weak current) than if added to a current of a large number of units (a strong current). But if this is so, it follows that *subtraction* of a unit from a strong current must have less effect than

subtraction of the same unit from a weak current. Now when the general excitability of the muscle is raised by cutting, the effect is that the muscle is able, both in the case of the ascending make and in that of descending break, to afford (as it were) to part with some units of the stimulating influence which were previously required to cause adequate stimulation. Hence, forasmuch as the sum of such units which it had to spare before cutting was so much less in the case of the make than in that of the break, in the case of the make each unit must have been of a correspondingly greater value as a stimulant. Consequently, when both the minimals are reduced by cutting, the reduction may take place in a strictly proportional manner; only, if the proportion has reference to the *value of the electrical units as stimulants*, it follows, from what has been said, that there will probably be no *numerical* proportion between the two ratios.

In favour of this explanation, it is to be remembered that, as already stated, nerve-section produces much more than a proportional effect in the ascending make as compared with the descending break, in respect of increasing the excitability of the muscle *to stimuli of short duration.* It is as though the comparatively small number of units of *electrical intensity* by which the minimal make is diminished through nerve-section represents a great actual increase in excitability, *when this is estimated by some other method*; or, to turn to the diagram, it seems as though the small distance through which the curve in fig. 2 passes as compared with the curve in fig. 3 really represents an increase of excitability much more important than the curve expresses: it seems as though it is just because the difficulty of ascending (so to speak) increases in so rapid a ratio as its curves approach the zero level, that the steep curve of the descending break terminates at, or below, the point where the much less steep curve of the ascending make begins. This appears to be so, because, on testing the increase of excitability by means of stimuli of short duration, it is found that the relatively low curve in fig. 2 represents what would doubtless be a relatively steep curve, if it were possible to institute the numerical comparisons in the case of stimuli of minimal duration, as it is possible to do so in the case of stimuli of minimal intensity.

These remarks, however, are only made by way of suggestion; and I confess that, *à priori*, I should certainly not have expected so great a disproportion to subsist between the curves in figs. 2 and 3.

§ 4. Sometimes severe section of a tolerably well-curarized muscle will be followed by a development of the breaking contraction treated of in § 2. I attribute this fact to incomplete poisoning of the nerve-elements in the muscle; for the following experiments prove conclusively that in an uncurarized muscle the development of the breaking contraction after cutting is a purely nervous effect.

(*a*) Section of the sciatic nerve just above the knee causes all the characteristic alterations in the minimal makes and breaks, and this nearly

as well as does section of the nerve in the muscle. Moreover the higher up the nerve is cut, the less is the degree in which these characteristic alterations occur, until, if the section be made at about the origin of the femur or one third of its length lower down, no trace of these alterations can be detected.

(*b*) Stimulating the sciatic nerve with acids, alkalies, &c., and warming it has the same sort of effects as cutting.

(*c*) Throwing the end of the sciatic nearest the gastrocnemius into kathelectrotonus has a well-marked effect of the same kind; while throwing the same part into anelectrotonus has the opposite effect, though not in so strongly marked a degree.

(*d*) Severe galvanic tetanization of the gastrocnemius is frequently followed by an increase of sensitiveness to the descending break nearly as remarkable as that which follows cutting. As this effect does not seem to occur in well-curarized muscles, I conclude that it must be due to an increase in the excitability of the intramuscular nervous elements through injury.

§ 5. Another method which I employed to test the effects of nerve-section on excitability was one which, in the first instance, I fell upon accidentally. It consisted in joining up the non-polarizable electrodes with a continuous bridge of clay made perfectly flat on its upper surface. Care being taken to keep this surface uniformly moist, the sciatic nerve in a nerve-muscle preparation was laid upon it; so that when the current passed through the clay bridge a portion of it also passed through the sciatic nerve, thereby stimulating the attached muscle. The advantage of this method consists in the facility with which different parts of the nerve-length may be stimulated to the exclusion of other parts. By a curious coincidence, Prof. Rutherford appears to have been working at this subject at about the same time as myself, though quite independently of me. It was only a few days ago that I became aware of this fact by observing an article in this month's Number of the 'Journal of Anatomy and Physiology,' in which Prof. Rutherford states his methods and results. As nearly all the latter agree in every particular with those which I obtained, I am now relieved from the necessity of detailing them. It is desirable, however, to state that, viewed in the light of my other experiments, these results amount to this:—When a few millimetres of nerve-length, including the extreme nerve-section, rested on the clay, a much less strength of current was required to produce the breaking contraction in the muscle than when any other portion of the nerve of equal length was allowed to rest on the clay. That is, in Prof. Rutherford's words, "*the striking fact, however, is that without altering the strength of the current* all the phenomena of Pflüger's law could be obtained by transmitting it through a central, middle, or peripheral portion of nerve, at one time in an ascending, at another time in a descending direction."

It may be worth while to state, as showing the astonishing excitability

of the extreme nerve-section, that if the nerve, while hanging in a vertical direction over the flat surface of the clay bridge, be lowered until the section just touches the flat surface of the clay, it may frequently be observed that the attached muscle responds to make and to break of the current. Yet this must be a case of almost complete transverse stimulation of nerve; for, thinking that there might possibly be some passage of the current from the clay into the nerve in a semilenticular form, I tried a number of times the effect of ligaturing a nerve with a fine human hair, then with a fine pair of scissors making the transverse section as close beneath the ligature as possible, and, lastly, lowering the nerve-section on the clay as before. In no one case, however, did I succeed in obtaining any results similar to those which I obtained with unligatured nerves. It may be stated that in all these experiments with the clay bridge, I graduated the amount of nerve-length to be laid on it by means of a horizontal glass rod firmly fixed to the tube of a microscope. The free end of the rod was pointed, and usually passed between the tendo Achillis and the tibia, the latter having been previously severed at the knee. The sciatic nerve was thus allowed to depend in a vertical direction, and could be very accurately adjusted upon the clay bridge by means of the rack-work which moved the tube of the microscope.

§ 6. During the course of the above investigation concerning the effects of nerve-injury on excitability, several other facts of interest were incidentally observed. It seems desirable, therefore, to add a brief account of these facts.

When an uncurarized muscle is in a state of moderately strong tetanus from the passage of a rather weak galvanic current, it may occasionally be observed that some part or parts of the muscle begin to *pulsate* in a strictly rhythmical manner—the parts concerned alternating their periods of tetanus with periods of repose, sometimes at about the rate which is observable in a frog's lymphatic heart, and sometimes faster. I have counted such pulsations through more than 100 revolutions, without a single intermission and in perfectly regular time throughout. That this interesting phenomenon is exclusively due to the intramuscular nervous element is, I think, proved by the fact that I have never seen it to occur in any one of the hundreds of curarized muscles which I have this year subjected to the influence of the constant current. Moreover, on one occasion I noticed a very good instance of rhythmical pulsation in a partly tetanized gastrocnemius, when I happened to have the attached sciatic on another pair of electrodes. Of course it occurred to me to try the effects of throwing the nerve near the muscle first into anelectrotonus and then into kathelectrotonus. The results were most decided. With a current of properly graduated intensity passing through the gastrocnemius, it was always quite easy to inhibit the pulsating effect in the muscle by throwing the attached nerve into anelectrotonus, while the pulsations were always seen to recommence as soon as the polarizing

current in the nerve was broken. Conversely, if the nerve was thrown into kathelectrotonus, the pulsating effect could be produced in the muscle by a current of less intensity than was required to produce this effect when the nerve was either in anelectrotonus or in the normal state.

§ 7. I have made several experiments with the view of showing the major influence of the kathode on closing, and of the anode on opening, in the case of well-curarized muscle; but on the present occasion it seems unnecessary to describe more than one.

If the curarized sartorius of a frog is placed on non-polarizable electrodes, and is somewhat stretched in a longitudinal direction by means of two weights attached to its two ends, it may almost invariably be observed (especially when the contractions become sluggish by exposure of the muscle) that upon closure of the circuit, and during all the time of its passage, the substance of the muscle *draws* towards the kathode, while on the kathode itself the substance of the muscle heaps up and spreads out in a very beautiful and distinctive manner. On now reversing the current, all the phenomena take place in the reverse way. Hence, by placing any minute body anywhere on the muscle between the poles, this body may be seen to travel some distance towards the kathode every time the current is reversed. Again, if a small transverse incision be made in the muscle anywhere between the poles, it gapes towards the kathode every time the current is reversed. Lastly, if two appropriately weighted levers be attached one to each end of the muscle, when the current is passing in one direction the lever nearest the kathode is raised; whereas when the current is reversed this lever, which is now nearest the anode, falls, while the other lever rises.

§ 8. If the copper wire terminals of a Daniell's element be taken one in each hand, and the strength of the current be graduated down to the point at which minimal stimulation is obtained by placing on a fresh muscle first the anode and then the kathode, it may invariably be observed that if this order is reversed, by first laying on the kathode and then the anode, no contraction will be given unless the strength of the current is somewhat increased. This curious fact may be observed equally well on curarized and on uncurarized muscles. It is independent of the direction of the current, and is not affected either by insulation of the muscle or by placing it on a gas-pipe. The phenomenon is likewise unaffected by placing the anode or the kathode in an unclosed circuit of a Grove's cell upon the muscle, and then experimented with the weakened circuit from the Daniell's cell as before. It may be observed that the long muscles of the thigh, either *in situ* or excised, are best adapted for making these experiments *.

* Until a short time ago I was not aware that any difference had as yet been detected between the effects of anodic and of kathodic closure. My attention, however, has now been directed to the observations of Hitzig, in which he finds that on minimal stimula-

May 11, 1876.

Dr. GÜNTHER, M.A., Vice-President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On some Thallophytes parasitic within recent Madreporaria." By P. M. Duncan, M.B., F.R.S., President of the Geological Society. Received March 17, 1876.

(Abstract.)

After noticing the works of Quekett, J. P. Rose, Wedl, and Kölliker on the filament-shaped parasites within recent and fossil molluscan shells and scales, and his own researches into and descriptions of corresponding growths in Madreporaria from the Silurian and Tertiary rocks, the author proceeds to explain the method of investigation employed in the examination of recent corals. The range of the parasites is then stated to be, in corals from the littoral zone down to 1095 fathoms, and from Davis Straits to the tropical coral-seas, and their lowest known temperature habitat is that of 31°·5 Fahr.

A list of species examined is given, and then the long slender canals with their included filamentous organisms are described. Then the method of entry of the growth is stated, and its relation to the organic basis of the coral sclerenchyma is explained. The reproduction by conidia and oospores is also explained. After noticing that the direction, branching, and size of the parasites depend upon the special peculiarities

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tion of the brain anodic closure is more effective than kathodic. This, of course, is precisely the reverse of what I find to be true of muscle; and as the fact of such a difference existing between the two cases is very remarkable, I may observe that it appears to confirm Hitzig's views concerning the reversed relations that subsist between central and peripheral galvanic stimulation.

I may also observe that I have repeatedly tried whether there is any difference to be detected between anodic and kathodic closure in the case of motor nerves, but hitherto without success. Yet, as it seemed to me very improbable that there should be any difference between nerve and muscle in this respect, I had intended to investigate the matter still further before publishing any thing with regard to nerve. I now find, however, that Hitzig's results with regard to brain had induced Engesser (Pflüg. Arch. x. p. 157 &c.) carefully to investigate the question with regard to motor nerves; and the conclusion he arrives at is that no difference can in their case be detected between the effects of anodic and of kathodic closure. Therefore, as this result agrees with my own, it seems desirable that I should here acknowledge the agreement. No one, so far as I can ascertain, has as yet published any thing in this connexion with regard to muscle.—G. J. R., May 5th, 1876.

of certain corals, the author discusses the classificatory position of the vegetable form. Naming it *Achlya penetrans*, he suggests that it belongs to a group whose life-cycle is complicated by marine and subaerial conditions, and infers that *Achlya*, *Saprolegnia*, *Botrytis*, *Peronospora*, *Empusina*, and possibly *Bryopsis* are so many names of the same organism under these different conditions. Believing in the necessity of an arbitrary name, he prefers that of *Achlya*. Finally an instance of a parasite resembling what is called *Saprolegnia ferax*, Ktz., in a littoral coral is given.

II. "On the Calculation of the Trajectories of Shot." By W. D. Niven, M.A., F.R.A.S. Communicated by J. Clerk Maxwell, F.R.S., Professor of Experimental Physics in the University of Cambridge. Received March 24, 1876.

(Abstract.)

The solution of the equations of motion of a shot is necessarily approximate, because the resistance cannot be expressed by a single exact formula, and, moreover, there are very few formulæ which are capable of affording an easy solution. The results which Hutton obtained by means of the ballistic pendulum were exhibited in the shape of a simple formula; and in like manner Piobert and Didion, who also used the ballistic pendulum, reduced their results, and were able to give simple formulæ. No one can doubt, however, that, in point of accuracy and extent of information, their results are inferior to those which Mr. Bashforth obtained by means of his chronograph and screens. Now the formulæ which Hutton and Didion gave apply only to spherical shot; and even for that kind of shot they do not agree with Mr. Bashforth's results except for a limited range of velocities. Mr. Bashforth makes no attempt to formulate his results, but produces them in the shape of two Tables, one for spherical shot, the other for cylindrical. The nature of the reductions of his experiments, and the fortunate circumstance that for a large range of values of the velocity the resistance varies nearly as the cube of the velocity, render it convenient to express the resistance in the form μv^3, where v is the velocity and μ a variable coefficient. In fact, if d is the diameter of the cross section of the shot in inches, and W its weight in lbs., the retardation due to resistance is

$$\frac{d^2}{W}K\left(\frac{v}{1000}\right)^3,$$

where K is a number which is tabulated for every 10 feet of velocity. The question, then, is to solve the problem of the motion of a shot in conjunction and agreement with the Tables for K. The problem is a very important one, not only to the gunner but the gun-maker, there being many practical questions, for example, connected with the dimen-

sions of shot, which could be dealt with more satisfactorily if there existed an easy method of calculating ranges. Mr. Bashforth gives one solution in his treatise, and the object of this paper is to give another.

The expressions here proved depend upon three integrals, which may be defined for ogival-headed shot as follows :—

$$V_n = \frac{180g}{\pi}\int_n^{1700} \frac{\overline{1000}|^3 dV}{KV^4},$$

$$S_n = \int_n^{1700} \frac{(1000)^3 dV}{KV^2},$$

$$T_n = \int_n^{1700} \frac{(1000)^3 dV}{KV^3}.$$

Of these integrals the two last have been already tabulated by Mr. Bashforth : the first is now given as low as $n=900$. The integrals are calculated between every 10 feet, for which the values of K are given, the arithmetical mean of K over the interval being taken.

Let A B be a portion of the trajectory of a shot ; let the inclinations at A and B be α and β, and the horizontal components of the velocity at the same points p and q. Then it is proved that the inclination $\bar{\phi}$ of the chord A B is approximately

$$\frac{\alpha+\beta}{2} + \frac{p-q}{p}\,\frac{\alpha-\beta}{6} \text{ in the ascending branch,}$$

and $$\frac{\alpha+\beta}{2} - \frac{p-q}{p}\,\frac{\beta-\alpha}{6} \text{ in the descending branch.}$$

If it be assumed that the inclination of motion between A and B has the mean value $\bar{\phi}$, the following four equations constitute the approximate solution of the problem, and the limits of the integrals are such as to make the results from the assumption approximate to the actual case :—

$$V_{q\sec\bar{\phi}} - V_{p\sec\bar{\phi}} = \frac{d^2}{W}\,D\sec\bar{\phi}, \quad \ldots\ldots\ldots\ldots \quad \text{(a)}$$

where D is the number of degrees in the difference between the inclinations at A and B ;

$$\frac{d^2}{W}X=\cos\bar{\phi}(S_{q\sec\bar{\phi}}-S_{p\sec\bar{\phi}}), \quad \text{(b)}$$

$$\frac{d^2}{W}Y=\sin\bar{\phi}(S_{q\sec\bar{\phi}}-S_{p\sec\bar{\phi}}), \quad \text{(c)}$$

$$\frac{d^2}{W}T=T_{q\sec\bar{\phi}}-T_{p\sec\bar{\phi}}, \quad \text{(d)}$$

X, Y being the horizontal and vertical distances respectively between A and B, and T the time.

The first equation gives q; and it will in general be sufficient in that equation to put $\bar{\phi}=\frac{\alpha+\beta}{2}$, because the secant of a small angle varies slowly. If, however, the angle of projection is large, it will be necessary to operate twice with the equation (a), the first time to determine an approximate value of q, the next time to determine a more accurate value after having obtained an approximation to the correct value of $\bar{\phi}$. In the (b) and (c) equations the more accurate value will be employed in the cosine and sine, which occur as factors outside of the integrals.

As an example of the method, take the case of a shot fired from a 38-ton gun. The following are the data:—Diameter of shot 12·5 inches, weight 810 lbs., angle of projection 3°, velocity 1400 feet, height of muzzle above ground 14 feet.

Let the work be first taken over the whole of the ascending branch. The first thing to do is to find q from the formula (a).

We have

$$\log(p\sec 1\tfrac{1}{2})=\log(1400\cos 3^\circ\sec 1\tfrac{1}{2})$$
$$=3{\cdot}1456812;$$
$$\therefore p\sec 1\tfrac{1}{2}=1398{\cdot}6.$$

From the V_v Table,

$$V_{p\sec 1\frac{1}{2}}=1{\cdot}0568.$$

Again,

$$\log\frac{d^2}{W}=\bar{1}{\cdot}2853350$$
$$\log 3=\cdot4771213$$
$$\log\sec 1\tfrac{1}{2}=\cdot0001488$$
$$\overline{\bar{1}{\cdot}7626051}$$
$$\therefore \frac{d^2}{W}3\sec 1\tfrac{1}{2}=\cdot5789;$$
$$\therefore V_{q\sec 1\frac{1}{2}}=1{\cdot}0568+\cdot5789.$$
$$=1{\cdot}6357.$$

∴ from the V Tables we find

$$q\sec 1\tfrac{1}{2}=1290.$$

More accurate value of $\bar{\phi}$:—

$$1^\circ\ 30' + \frac{1398{\cdot}6 - 1290}{1398{\cdot}6} \times 30'$$

$$1^\circ\ 32'{\cdot}35$$

$$\log \cos \bar{\phi} = \bar{1}{\cdot}9998445$$

$$\begin{array}{r} \log \sin \bar{\phi} = \bar{2}{\cdot}4274621 \\ 16429 \\ \hline \bar{2}{\cdot}4291050 \end{array}$$

From Mr. Bashforth's Tables for S_v and T_v :—

$$\begin{array}{rr} S_{1290} = 1920{\cdot}5 & T_{1290} = 1{\cdot}3007 \\ S_{1398{\cdot}6} = 1355{\cdot}3 & T_{1398{\cdot}6} = \ {\cdot}8796 \\ \hline \text{Difference} = \ 565{\cdot}2 & {\cdot}4211 \\ \log X = 2{\cdot}7522022 & \log Y = 3{\cdot}4668672 \\ \bar{1}{\cdot}2853350 & \bar{2}{\cdot}4291050 \\ \hline 3{\cdot}4668672 & 1{\cdot}8959722 \\ \bar{1}{\cdot}9998445 & \\ \hline 3{\cdot}4667117 & \\ & \log T = \bar{1}{\cdot}6243852 \\ & \bar{1}{\cdot}2853350 \\ & \hline & {\cdot}3390502 \end{array}$$

$$\therefore X = 2929, \quad Y = 78{\cdot}7, \quad T = 2{\cdot}183.$$

The projectile having ascended 78·7 feet will have to fall through $78{\cdot}7 + 14 = 92{\cdot}7$. In the descending branch we shall again integrate over 3°, because a good deal of the calculation will then have been done for us in the work for the ascending branch.

Our p here is the q of the last arc, so that

$$p \sec 1\tfrac{1}{2} = 1290\,;$$

$$\therefore V_{p \sec 1\frac{1}{2}} = 1{\cdot}6357\,;$$

$$\therefore V_{q \sec 1\frac{1}{2}} = 1{\cdot}6357 + {\cdot}5789$$

$$= 2{\cdot}2146.$$

$$q \sec 1\tfrac{1}{2} = 1207{\cdot}4.$$

$$\bar{\phi} = 1^\circ\ 28'.$$

Difference of S's = 488·1

Difference of T's = ·3912,

$$X = 2529{\cdot}5$$

$$Y = \ 64{\cdot}76$$

$$T = \ 2{\cdot}028.$$

The projectile has still 28 feet to fall through.

As an approximation let us put

$$\frac{d^2}{\mathrm{W}} \cdot 28 = \sin 3^\circ 20' \left(\mathrm{S}_{q \sec 3^\circ 20'} - \mathrm{S}_{p \sec 3^\circ 20'}\right).$$

We should get

$$\mathrm{S}_{q \sec 3^\circ 20} = 2502{\cdot}3\,;$$

$$\therefore q \sec 3^\circ 20' = 1193{\cdot}0.$$

$\therefore$ using equation (a), $\mathrm{D} = 38'{\cdot}2$.

We may now put

$$\mathrm{X} = 28 \cot (3^\circ 19'{\cdot}1)$$
$$= 482{\cdot}8$$

and
$$\mathrm{T} = \frac{482{\cdot}8}{1200} \text{ nearly } {\cdot}402.$$

Summation of X's :—

$$\begin{array}{r} 2929 \\ 2529{\cdot}5 \\ 482{\cdot}8 \\ \hline \text{Range} = 5941{\cdot}3 \end{array}$$

Summation of T's :—

$$\begin{array}{r} 2{\cdot}183 \\ 2{\cdot}028 \\ {\cdot}402 \\ \hline \text{Time of flight} = 4{\cdot}613 \end{array}$$

The observed range and time of flight were in this particular case 6060 feet and 4·75 sec.

III. "Condensation of Vapour of Mercury on Selenium in the Sprengel Vacuum." By R. J. Moss, F.C.S., Chemical Laboratory, Royal Dublin Society. Communicated by G. Johnstone Stoney, F.R.S. Received March 25, 1876.

In the course of experiments on the electrical conductivity of selenium, a cylindrical bar of this substance in the vitreous state was enclosed in a glass tube which was attached to the exhaust-tube of a Sprengel pump. The bar of selenium was 45 millims. long and 3·5 millims. in diameter. Platinum wires were attached to the ends of it and passed through the sides of the glass tube. The tube was exhausted, and allowed to remain attached to the pump for four days. It was now found that the selenium had acquired a conductivity greatly exceeding that of the pure element in its most highly conducting condition. The experiment was repeated with the intention of observing the time required to produce conductivity. In forty-two hours the needle of a highly sensitive gal-

vanometer was slightly deflected when the selenium was placed in the circuit of ten Leclanché cells. The conductivity of the selenium increased rapidly for four days, when the experiment was unavoidably interrupted. On admitting air to the tube no change of conductivity was observed. The selenium was unaltered in appearance, even when examined microscopically. On breaking the bar it was found that the conducting-film was entirely superficial; it was not removed by rubbing forcibly with a cloth. Dilute nitric acid also failed to remove it. Bibulous paper moistened with solution of silver ammonio-nitrate was not stained by it (Merget, 'Comptes Rendus,' vol. lxxiii. p. 1356). It therefore appears highly probable that the film does not consist of uncombined mercury. As it has not hitherto been known that mercury combines with selenium at ordinary temperatures, a bar of selenium was immersed in mercury and allowed to remain undisturbed for six months. At the end of this time it was found that the selenium was coated with a highly conducting film. I could not dectect any difference between this film and those produced in the Sprengel vacuum. An attempt was now made to estimate the quantity of mercury required to produce the observed conductivity. A bar of selenium 125 millims. long and 2 millims. in diameter, having platinum wires fused into each end, was enclosed in a glass tube, containing also a minute globule of mercury about 0·5 millim. in diameter. The tube was exhausted by means of the Sprengel pump, and then hermetically sealed and detached from the pump. In 92 hours the bar began to conduct, and the conductivity increased rapidly from day to day for four days. On the fifth day, no increase being observed, it was supposed that air had leaked into the tube; and on examining it a flaw, which would account for the leakage, was detected. The tube was therefore again attached to the pump, exhausted, and again sealed, the defective portion being removed. The conductivity of the bar again increased from day to day, and is still steadily but slowly increasing (eleven days after the second sealing of the tube). Although the bar of selenium now possesses a comparatively low resistance, I cannot detect the slightest alteration in the size of the minute globule of mercury which has supplied the material for the conducting-film, extending over a surface one thousand times greater than that of the globule.

The granular modification produced by subjecting vitreous selenium to a temperature of 100° C. for three hours also acquires a great increase of conductivity when exposed to the vapour of mercury in the Sprengel vacuum.

As it is possible at any moment to arrest the formation of these conducting-films, bars of selenium of any given high resistance may be obtained in this way with great certainty and accuracy.

IV. "On Simultaneous Variations of the Barometer in India." By J. A. Broun, F.R.S. Received March 16, 1876.

[Plate 1.]

In this, the first of a proposed series of communications on the barometric variations, it may be useful to recall the views held by men of science as to the causes which produce them.

Since Pascal showed, by the experiments on the Puy de Dôme, that the height of the mercury in the tube was lower the higher the station, it was a natural conclusion when the column of mercury fell while the barometer remained in the same place, that this was also due to a diminution of the mass of air above it.

In order to satisfy the facts, afterwards discovered, connected with diurnal and annual variations of atmospheric pressure, hypotheses were proposed as to the modes in which the quantity of gravitating matter pressing on the barometer was increased or diminished. The actions of currents containing colder and denser or warmer and rarer layers of air, the accumulation of the air thus conveyed over a station, and the overflowing from one station to another were the most obvious methods of explaining the variations of mass. The fact that cold air enters along the floor of a room while the air heated by the fire ascends the chimney, was a suggestion applied on a large scale to the whole globe. The polar regions took the place of the door as the source of cold currents, and the tropical regions represented the fireplace; from this last the air ascended the great terrestrial chimney, passed over more northerly and southerly countries till it descended near the poles, to seek its way back to the place whence it came. This hypothesis is represented by engravings in many works treating of these variations, and the most ingenious figures are made to cover the earth's surface, showing how the aerial movements ought to be performed: *ought to be*, for there is a great want of the facts which should show that the currents really move as they have been represented.

It was found, however, that, even with the aid of these hypothetical movements, one of the most marked and most regular variations of atmospheric pressure could not be explained. The barometer rises till about 9 or 10 o'clock in the forenoon and evening, and descends till nearly 3 or 4 o'clock, morning and afternoon. The amounts of these rises and falls are themselves subjected to laws varying with many conditions which no system of ascensional currents can satisfy. The pressure of the vapour in the air was the only remaining source of variation depending on mass which presented itself; and a well-conceived hypothesis founded on this element in connexion with aerial currents was supposed to explain the whole phenomena.

When we seek for evidence that the causes proposed are either true or

sufficient, we fail to find it. From my own experience within the tropics, where the great diurnal oscillations of the barometer have to be explained by these currents, we may look in vain for any traces of them during weeks of the season when the oscillations are most marked. At stations near the sea, a slight breeze from the ocean during the day and a like breeze seaward during the night are the only movements perceived for days together; while at inland stations even these are unfelt, though occasionally local gusts of air sweep in one direction or another. From the summits of the South-Indian Ghats, where the Coromandel sea borders the eastern and the Malabar Sea the western horizon, clouds could be seen forming over sea and land, which scarcely moved, and disappeared near the places of their birth; the highest cirri which mottled the sky seemed frequently a fixed fretwork, or one which moved so slowly that the forms had changed before the direction of their motion could be determined. The clouds which occasionally ascended from the valleys to the mountain-tops remained so balanced in the air that the size of their droplets could be estimated under the object-glass of a microscope. During these days of calm from sea to sea, the barometer rises and falls, on the highest peaks as on the plains, with the regularity of clockwork.

When we transfer ourselves to the higher latitudes of the British Islands and seek for evidence of these currents, no such regular movements as the hypothesis requires can be observed: the only appearance of the current from the pole is to be found in occasional north-east surface-winds; the upper currents, as shown by the motion of the cirri, proceed on the average from the west. Here also the currents fail to follow their supposed courses.

The vapour-pressure supplementary theory fails completely whenever the diurnal variation of vapour-pressure is small compared with that of the barometer; its apparent success in other cases is a mere arithmetical result which could never support a careful comparison with facts.

When we turn our attention to the changes of barometric height which occur from day to day, the hypothesis of aerial currents seems to have a surer basis. These variations have been carefully studied by Sir J. Herschel, Mr. Birt, A. Quetelet, and others. The first considered that these oscillations "perhaps take their rise in local and temporary causes prevailing over great areas simultaneously, the principal no doubt depending on the prevalence of cloud, or clear sky, or dryness, over great tracts for several days or weeks in succession"*. He also thought that the movements of the atmosphere thus produced should be a cause of winds alternately progressive and retrogressive.

Quetelet, who projected the lines of maximum and minimum pressure on the map of Europe, suggested that the tropical current descending

* Report Brit. Assoc. 1863, p. 99.

42500

near the pole formed a kind of cap (*calotte*) to the globe, that this passing towards the equator in all longitudes, or advancing in sectors, would produce the atmospheric waves. "Les ondes, dans cette hypothèse, devraient se propager en même temps que les courants polaires des pôles vers l'équateur, et dans notre hémisphère du nord vers le sud"*. Yet after a study of the directions of the wind, he concludes, "Les directions des vents n'ont pas des rapports apparents avec les directions des ondes barométriques"†.

It is not necessary to criticise these hypotheses further. That heated air rises, and that currents of air are associated with great diminutions of atmospheric pressure, are facts which do not suffice to explain the great semidiurnal atmospheric tide, nor the sudden appearance of atmospheric waves with crests reaching from London to Pekin‡ and breadths of 1000 miles §.

It does not appear impossible, however, that other causes of varying atmospheric pressure may exist than change of the mass of air; in other words, that the attraction of gravitation may not be the only force concerned in the barometric oscillations: the following results, it appears to me, will require some additional cause for their explanation.

Having found that a marked variation of the horizontal force of the earth's magnetism is produced by the sun's rotation on his axis, and that the period of this rotation shown by the magnetic observations was nearly 26 days, I sought whether some effect might not be produced by the same cause on the atmospheric pressure. A discussion of the amplitudes of the diurnal oscillations of the barometer within the tropics for a period of 26 days gave no sufficiently marked result; if any such period exists the variation due to it appeared small: a similar discussion of the *irregular* diurnal oscillations in high latitudes gave a large variation. The latter movements are evidently connected with the daily changes which have been studied by Herschel and others. My attention was then directed to the changes of daily mean barometric pressure within the tropics; and for this investigation, Singapore, a station near the equator, was chosen, where the irregularities due to local causes might be supposed least. The variations of daily mean pressure there, when projected in curves, were found to resemble those previously obtained by me for the earth's magnetic force: large oscillations (for that region) occupied from 20 to 30 days for some months, then disappeared to reappear later‖.

* Sur le Climat de la Belgique, 4e partie, p. 80.

† Ibid. p. 91. ‡ Ibid. plate 4. § Brit. Assoc. Report, 1843, p. 70.

‖ The similar disappearance in the case of the magnetic variations has been found due to the opposing actions of the sun and moon. It has still to be determined whether this is not the case also for the barometric oscillations. See paper "On the Variations of the Daily Mean Horizontal Force of the Earth's Magnetism, &c.," Proc. Roy. Soc. vol. xxiv. p. 231.

The discussion of four years' observations gave for the length of the period 25·86 days, and mean amplitudes of oscillation (as represented by the term $a \sin \theta$) between 0·01 and 0·032 inch in different years.

These movements could not be traced in the observations made at Hobarton or Makerstoun, where the daily changes of pressure are increased tenfold by other causes, and probably by other modes of action of the same cause; but some of them were still found to exist at St. Helena*. It is not intended at present to enter into the consideration of the 26-day period (which will show, however, the existence of the other cause of varying pressure alluded to), but to examine all the various changes of barometric pressure within a given area and during a limited period of time. When stations are chosen which lie nearly in the same parallel of latitude or great circle of the sphere, it might be supposed, by the purely gravitational theorist, that, however great the distance, simultaneous variations of pressure are due to the rhythmical expansion and overflow of the atmosphere; in the following discussion it will be seen that this suggestion cannot be made use of.

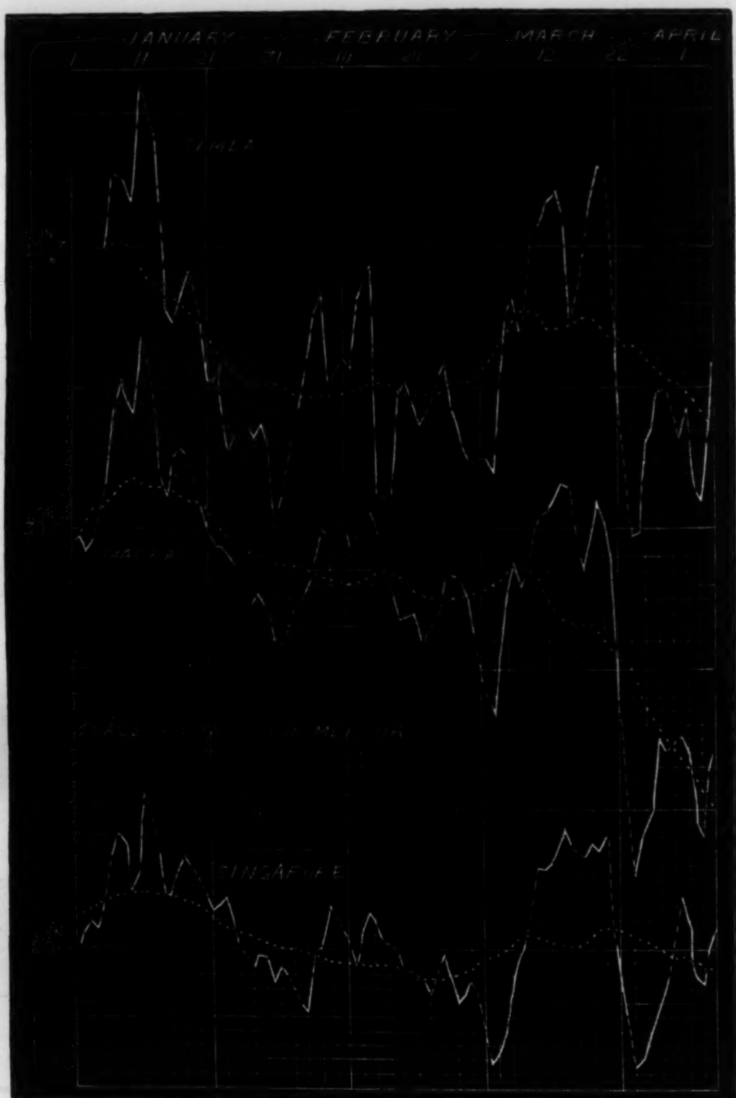
Hourly observations of the barometer made at the three following

stations during the months of January, February, and March, 1845, have been employed in this investigation†:—

* See 'Comptes Rendus de l'Académie des Sciences,' t. 75, pp. 16, 121.

† In the note already cited, which appeared in the 'Comptes Rendus,' the observations for the three months January to April were employed because they showed in the most marked manner the large oscillations of the 26-day period; the daily means then used were those for the Göttingen astronomical day, given with the printed observations to correspond with the daily means of the magnetic variations. These means, however, have the disadvantage that one day in every week is made up of the part of the astronomical day occurring on Saturday and the part occurring on Monday. In the present note the means for the *civil* days have been computed for Singapore and Madras. General Boileau has given the means for the *civil* days in the work cited below. It is due to the recent publication of his valuable series of observations that I have been able to include so large an area in my investigation.

	Lat. N.	Long. E.	Height in feet.
	° ′	°	
Singapore	1 19	104	Few *
Madras	13 4	80	27 †
Simla	31 6	77	7096 ‡



* Meteorological Observations made at the Honourable East-India Company's Magnetical Observatory at Singapore, by Captain C. M. Elliot, 1841–1845. Madras, 1850.

† Meteorological Observations, Madras, 1845, by Lieut.-Colonel Ludlow.

‡ Meteorological Observations made at the Magnetic and Meteorological Observatory at Simla, 1841–1845, under the direction of Lieut.-Colonel J. T. Boileau, F.R.S., Superintendent of the Observatory. London, 1872.

The daily mean height of the barometer, derived from 24 hourly observations for each *civil* day and for each station, are projected, p. 28. The relative positions and approximate distances (in English miles) of the three stations are given in the preceding figure.

The first movements to be considered in the figures (p. 28) are those shown by the dotted curves, which represent the mean barometric height for 27 days, including 13 days before and 13 days after each point. In these means the variations due to the solar rotation and lunar revolution periods may be considered approximately eliminated. It will be seen that at all the stations the height diminished from the 9th or 10th of January till the beginning of April: this movement is part of the annual variation, which is most marked at Madras, amounting to 0·3 inch from January to June, while it is only 0·06 inch at Singapore. At Simla in 1845 the range was about 0·25 inch, but less regular than at the other stations*.

The dotted curves show movements which appear independent of the regular annual variation, and which are similar at all the stations; these will require a larger series for their consideration.

When we examine the change of daily mean barometric height, three well-marked movements are to be seen at all the stations. The first terminates about the end of January, the second in the beginning of March, and the third towards the end of March; these movements are, I believe, due to the solar rotation, and perhaps the lunar revolution; they will be considered in another note†. The special object of this discussion is the examination of the changes of pressure from day to day.

It will be seen from the curves, p. 28, that in general, on whatever day the barometer attains a maximum (or minimum) at one station, a maximum (or minimum) occurs at the same time at the other stations. The means, however, for the civil days only are not fitted to show the exact hours of occurrence of the maximum or minimum: to determine these epochs with the greatest possible accuracy, the following method has been adopted. In order to eliminate the diurnal oscillation, the means of 24 hourly observations are always taken; but these means have been obtained for the 24 hours, having for their middle hour *each* hour of the

* The annual variation obeys local laws; considerable differences of barometric height exist for months on two sides of the southern Ghats, where the conditions of temperature and humidity are far from being the same. The diurnal variation in India, especially the descent from the forenoon maximum to the afternoon minimum, seems to depend, though to a less extent, on the same conditions. Mr. Buchan has lately shown by the difference of the amplitude of this oscillation for the volcanic region of South Italy and for the Spanish peninsula, that other conditions may exist ("On the Diurnal Oscillations of the Barometer," Trans. Roy. Soc. Edin. vol. xxvii. p. 406).

† It may be remarked here that the solar rotation appears to produce also, and more especially in high latitudes, a 13-day period, that represented by the term $b \sin 2\theta$ in the function of sines for the 26-day period.

day, instead of noon only. Thus the mean of the 24 hours from midnight of Sunday till midnight of Monday is considered the daily mean at noon of Monday; that from 1 A.M. of Monday to 1 A.M. of Tuesday is the daily mean at 1 P.M. of Monday; and so on for each successive hour and for each successive day in the week. These means before and after each turning-point are projected on an enlarged scale (p. 37); and from the calculated quantities we shall now seek the *hours* for which the daily mean atmospheric pressure was a maximum or minimum at each station.

1. January 7^{d}, 8^{d}. Fig. 1, p. 37. This is the first turning-point for all the stations*. We find from the means shown in this figure:—

		d	h
Highest mean,	Simla, January	7	4
„ „	Madras „	8	4
„ „	Singapore „	7	12

2. January 9^{d}. Fig. 2, p. 37.

		d	h
Lowest mean,	Simla, January	9	16
„ „	Madras „	9	17
„ „	Singapore „	9	13

The next maximum occurs on Sunday, for which there are no observations; it is easily seen, however, from the curves (p. 28) that this, the principal maximum for the whole period, occurred near January 11^{d} 12^{h} at all the three stations.

3. January 14^{d}–16^{d}. Fig. 3, p. 37.

		d	h
Lowest mean,	Simla, January	16	5
„ „	Madras „	14	14–20
„ „	Singapore „	14	12

It might be supposed that the retardation of the epoch of minimum at Simla was due to the greater descent of the barometer at that station; but the following maximum, which was also retarded, shows that this was due to some other cause:—

4. January 16^{d}–18^{d}. Fig. 4, p. 37.

		d	h
Highest mean,	Simla, January	18	6 ?
„ „	Madras „	17	10
„ „	Singapore „	16	12

Here the exact epoch of maximum at Simla can only be estimated from the curves, p. 28, since the barometer was still rising at the last observation on Saturday; the means for Simla have not been projected, fig. 4. An examination of the curves, p. 28, will show that there can be little doubt as to

* The observations were not made at Simla in the first week of the year.

the correspondence of movements at all the stations; yet the interval from the preceding minimum is so short, and the retardation of the minimum at Simla so considerable, that the latter occurs only a few hours before this maximum at Singapore.

A faint minimum and following maximum occur January 21–23 at all the stations, and these are followed by a minimum, January 24, at Simla, which occurs later (on Sunday) at the other stations; these movements and the slight maximum, January 28, 29, which could be examined are too small for any certain comparison.

5. January 30^d, 31^d. Fig. 5, p. 37.

			d	h
Lowest mean,	Simla,	January	30	19
„	„	Madras „	31	0
„	„	Singapore „	30	4

The principal minimum of this great movement occurs at the hours given for Simla and Madras; but the principal minimum occurs at Singapore, February 3rd. An examination of the observations on Saturday evening and Monday morning at Simla and Madras shows that a secondary minimum probably occurred at these stations on Sunday, February 2nd; this minimum may thus be described as corresponding to the minimum at Singapore.

6. February 6^d, 7^d. Fig. 6, p. 37.

			d	h
Highest mean,	Simla,	February	6	20
„	„	Madras „	7	9
„	„	Singapore „	7	5

The diminution of pressure to Saturday night is more marked at Simla than at the other stations (see curves, p. 28).

7. February 10^d. Fig. 7, p. 37.

			d	h
Lowest mean,	Simla,	February	10	20
„	„	Madras „	10	17
„	„	Singapore „	10	22

This minimum has nearly the same form at all the stations, a more rapid descent than the following rise.

8. February 13^d. Fig. 8, p. 37.

			d	h
Highest mean,	Simla,	February	13	13
„	„	Madras „	13	18
„	„	Singapore „	13	4

A large fall takes place at Simla on Saturday, February 15th, while at the other stations the diminution of pressure is comparatively small; there is in consequence a marked increase shown at Simla from Monday

17th to Tuesday 18th, which does not appear to have been felt at the other stations; there was, however, a slight maximum at Madras, 19^d 12^h, at Singapore (very slight) 20^d 0^h, corresponding with the marked maximum at Simla, 19^d 12^h.

9. February 20^d, 21^d. Fig. 9, p. 37.

		d	h
Lowest mean,	Simla, February	20	17
„ „	Madras „	21	2
„ „	Singapore „	21	15

This minimum is not shown at Singapore by the civil day means (curves, p. 28); but the character of the fall and the following rise is nearly the same at all the stations, though Singapore is retarded 22^h on Simla.

10. February 23^d–25^d. Fig. 10, p. 37.

		d	h
Highest mean,	Simla, February	24	18
„ „	Madras „	25	4
„ „	Singapore „	23	20 ?

The curve for Singapore is not given, several observations having been omitted on Monday morning (24th): from a comparison with the observations on Saturday, it does not appear probable that the maximum occurred earlier than 23^d 20^h. The general movement at Singapore during this week does not agree with those at the other stations, although a slight minimum at Madras (25^d 19^h) shortly after the maximum (fig. 10) seems related to a minimum at Singapore (see 20^d 0^h, p. 28).

11. March 3^d. Fig. 11, p. 37.

		d	h
Lowest mean,	Simla, March	3	23
„ „	Madras „	3	20
„ „	Singapore „	3	11

This is a very marked minimum preceding the great movement which ends March 24th; it is not seen in the civil day means (p. 28).

12. March 6^d, 7^d. Fig. 12, p. 37.

		d	h
Highest mean,	Simla, March	6	12
„ „	Madras „	7	2
„ „	Singapore „	7	10 ?

It may be questioned whether this secondary maximum was marked at Singapore otherwise than by the inflexion after 7^d 10^h.

13. March 12^d–14^d. Fig. 13, p. 37.

		d	h
Highest mean,	Simla, March	12	14
„ „	Madras „	14	10
„ „	Singapore „	14	0

The minimum preceding this maximum occurred on the Sunday: the barometric fall after the maximum is considerably greater at Simla than at the other stations; the epoch of maximum is thus accelerated at Simla, where the curve has a much sharper form than at the other stations. The minimum after this maximum again occurs on Sunday*.

14. March 19^d. Fig. 14, p. 37.

			d	h
Highest mean,	Simla,	March	19	9
„ „	Madras	„	19	3
„ „	Singapore	„	19	18

A great diminution of pressure begins immediately after this maximum at all the three stations, forming the most rapid barometric fall within five days throughout the three months. The exact hour of minimum cannot be given certainly for want of observations on Sunday; but from a comparison of the means for Saturday (March 22^d, curves, p. 28) with the daily means for each hour, p. 37, the following are probably within a few hours of the true times.

15. March 23^d, 24^d. Fig. 15, p. 37.

			d	h
Lowest mean,	Simla,	March	24	0?
„ „	Madras	„	23	22?
„ „	Singapore	„	24	4?

16. March 28^d. Fig. 16, p. 37.

			d	h
Highest mean,	Simla,	March	28	4
„ „	Madras	„	28	5
„ „	Singapore	„	28	20

As another maximum occurred at Simla after 29^d 0^h (Sunday), it is not certain which of the two should be compared with the maximum at the other stations. The minimum which follows this maximum occurs on Sunday.

17. March 31^d. Fig. 17, p. 37.

			d	h
Highest mean,	Simla,	March	31	23
„ „	Madras	„	30	18?
„ „	Singapore	„	31	8

This is a very small movement which has been felt earliest at Madras; it has nearly the same character at Simla and Singapore; the time for Madras is estimated from the Saturday mean (see curves, p. 28, March 29^d), and from the figure 17, p. 37.

* It may be pointed out that in the space of 66 days after the principal maximum (January 12^d to March 19^d), ten (chiefly secondary) maxima succeed each other at nearly equal intervals of 6½ days.

18. April 2^d. Fig. 18, p. 37.

			d	h
Lowest mean,	Simla,	April	2	14
„ „	Madras	„	2	19
„ „	Singapore	„	2	18½

This is a well-marked movement occurring in the middle of a week: that at St. Helena corresponding to it, February 17^d and 18^d, is also shown, fig. 18; this will be considered immediately.

If we now seek to arrange the maxima and minima which occur first for each station, we obtain the following results:—

Maxima and Minima earlier at Simla than at Madras or Singapore.

Maxima.

Fig.	Time at Simla.			Later at Madras.	Later at Singapore.
		d	h	h	h
1.	Jan.	7	4	24	8
6.	Feb.	6	20	13	9
8.	„	13	13	5	...
10.	„	24	18	10	...
12.	Mar.	6	12	14	22?
13.	„	12	14	44	34
14.	„	19	9	...	9
16.	„	28	4	1	16

Minima.

Fig.	Time at Simla.			Later at Madras.	Later at Singapore.
		d	h	h	h
2.	Jan.	9	16	1	...
5.	„	30	19	5	...
7.	Feb.	10	20	...	2
9.	„	20	17	9	22
15.	Mar.	24	0?	...	4?
18.	Apr.	2	14	5	4½

Maxima and Minima earlier at Madras than at Simla or Singapore.

Maxima.

Fig.	Time at Madras.			Later at Simla.	Later at Singapore.
		d	h	h	h
4.	Jan.	17	10	20	...
12.	Mar.	7	2	...	8?
14.	„	19	3	6	15
16.	„	28	5	...	15
17.	„	30	18?	29?	14?

Minima.

Fig.	Time at Madras.			Later at Simla.	Later at Singapore.
		d	h	h	h
3.	Jan.	14	17	36	...
7.	Feb.	10	17	3	5
9.	„	21	2	...	13
11.	Mar.	3	20	3	...
15.	„	23	22?	2?	6?

Maxima and Minima earlier at Singapore than at Madras or Simla.

Maxima.

Fig.	Time at Singapore.			Later at Madras.	Later at Simla.
		d	h	h	h
1.	Jan.	7	12	16	...
4.	„	16	12	22	42?
6.	Feb.	7	5	4	...
8.	„	13	4	14	9
10.	„	23	20?	32?	22?
13.	Mar.	14	0	10	...
17.	„	31	8	...	15

Minima.

Fig.	Time at Singapore.			Later at Madras.	Later at Simla.
		d	h	h	h
2.	Jan.	9	13	4	3
3.	„	14	12	5	41
5.	„	30	4	20	15
11.	Mar.	3	11	9	12
18.	Apr.	2	18½	0½	...

From these results it appears that the maximum happened—

			h
Earlier at Simla than Madras....	7	times on an average of	16
,, ,, Singapore..	6	,, ,,	16
,, Madras than Simla ..	3	,, ,,	18
,, ,, Singapore..	4	,, ,,	14
,, Singapore than Madras	6	,, ,,	16
,, Simla ,, ,,	4	,, ,,	22

The mean interval between the occurrence of a maximum at any two of the stations is $16^{h}\cdot7$. The maximum happened first at Simla 43·3 times per cent., first at Singapore 33·3 times per cent., and first at Madras 23·3 per cent.

We find that the minimum happened—

			h
Earlier at Simla than Madras....	4	times on an average of	4
,, ,, Singapore ..	4	,, ,,	8
,, Madras than Simla....	4	,, ,,	11
,, ,, Singapore ..	3	,, ,,	8
,, Singapore than Madras.	5	,, ,,	8
,, ,, Simla ..	4	,, ,,	18

The mean interval between the occurrence of the minima at two stations is $9^{h}\cdot6$; there was little difference in the number of cases first at any station.

On the whole the number of cases considered is not sufficiently great to give any weight to the conclusion that the maximum or minimum happens oftenest first *at any of the stations**.

There were four cases (6, 8, 14, and 16) in which the maxima occurred at *all* the three stations within a space of less than 16 hours. In these cases the mean intervals between the occurrence of the maxima—

At Simla and Madras .... = 6·25 hours.
,, Singapore .. = 10·75 ,,
At Madras and Singapore . = 12·00 ,,

Or the mean interval for any two of the three stations was 9·7 hours.

There were four cases (2, 7, 15, and 18) in which the minima occurred at *all* the stations within a space of 6 hours. In these cases the mean intervals were for

Simla and Madras ...... = 4·0 hours.
,, Singapore .... = 3·4 ,,
Madras and Singapore.... = 3·9 ,,

* All the cases have been employed; if we were to reject the cases for which the times are not certain to a few hours (indicated by ?) the final results for the mean intervals would be little affected; the mean interval for the maxima and minima would be 15·1 and 10·5 hours respectively.

For all the cases, as well as for these, the minima occur more nearly simultaneously at *all* the stations than the maxima.

When the various local causes (including the different annual and diurnal variations) which may accelerate or retard the epochs at different stations are taken into account, we may conclude that the maxima and minima occur nearly simultaneously over the area embraced in this investigation; and it may be added that they are as nearly simultaneous as the daily mean maxima and minima of the horizontal force of the earth's magnetism for different stations on the earth's surface. (See plate 28, vol. xxii. Trans. Roy. Soc. Edin.)

It has been already stated that other causes of change of atmospheric pressure enter in higher latitudes than those here considered; how far the changes agree with each other, and how far they differ from those in low latitudes, will be the subject of another note. It was found that at St. Helena the movements now studied were much smaller in general than in India, and sometimes in opposite directions*. I have, however, chosen the well-marked movement, March 31 to April 5 (which has nearly the same amplitude at St. Helena as at the Indian stations), for comparison. The 24-hourly means for the four stations are projected, Nos. 4, 6, 7, and 8, Plate 1. The epochs for this movement have been already given (17 and 18). We find from the curve No. 4, Plate 1,

At St. Helena, the maximum Mar. $31^d\ 5^h$, the minimum Apr. $3^d\ 1^h$.

Thus the maximum occurred about 3 hours earlier than at Singapore, and the minimum about 6 hours later than at Singapore and Madras by the local hours†.

The amplitudes of the different movements have not been taken into consideration; it is obvious from the curves, p. 28, that they are on the whole greatest at Simla and least at Singapore. The barometric falls at the four stations, March 19 to March 24, are:—

	in.
Simla	0·27
Madras	0·27
Singapore	0·17
St. Helena	0·06

* Comptes Rendus, t. 75, p. 10.

† It should be observed that the hours employed are in all cases the local hours for each station; and though, on account of the epochs being sometimes earlier and sometimes later at each place, the mean intervals are not affected for the three Indian stations, where the greatest difference of longitude is not 2 hours, yet at St. Helena there is a difference with Singapore of nearly 7 hours; so that if the time is counted from a common first meridian the intervals would be, for St. Helena, the maximum 4 hours, the minimum 13 hours later than at Singapore.

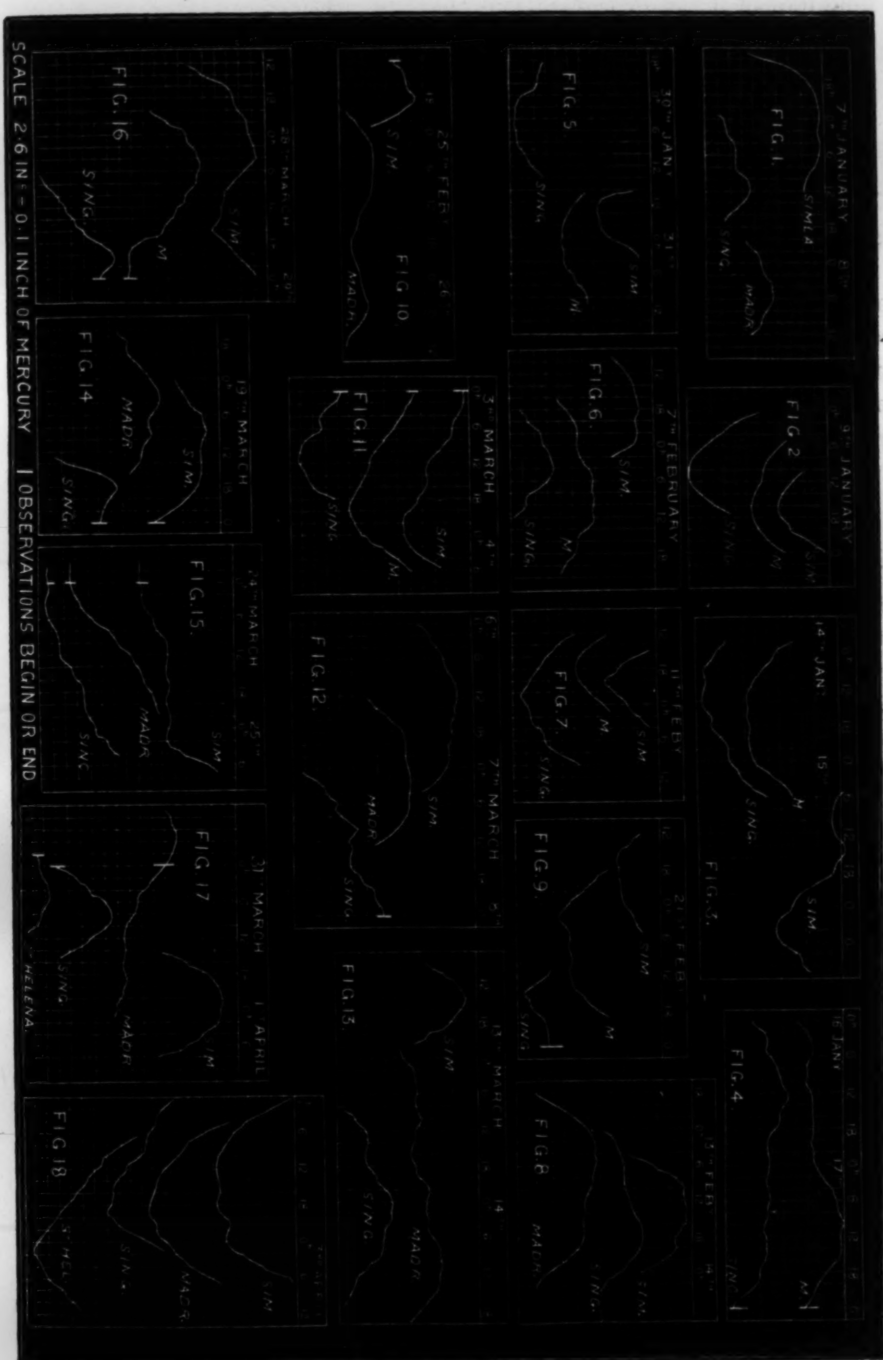


FIG. 1.
7TH JANUARY
SIMLA
SING.
MADR.
FIG. 2
9TH JANUARY
FIG. 3.
14TH JANY
FIG. 4.
16 JANY
FIG. 5.
30TH JANY
FIG. 6.
7TH FEBRUARY
FIG. 7.
11TH FEBY
FIG. 8
FIG. 9.
FIG. 10.
25TH FEBY
FIG. 11
3RD MARCH
FIG. 12.
7TH MARCH
FIG. 13.
FIG. 14
19TH MARCH
FIG. 15.
24TH MARCH
FIG. 16
FIG. 17
31ST MARCH
HELENA
FIG. 18
SCALE 2·6 INS = 0·1 INCH OF MERCURY
I OBSERVATIONS BEGIN OR END

While for the movement March 31 to April 3 they are:—

	in.
Simla	0·066
Madras	0·069
Singapore	0·083
St. Helena	0·070

The first movement seems to diminish on proceeding southwards from Madras; the second remains nearly constant from Madras to St. Helena, and is least at the most northerly station.

Perhaps the most remarkable fact brought out in this discussion is the fewness of the cases in which a change of pressure perceived at one station is not felt at the others; and this holds true for changes of not more than one hundredth of an inch of mercury. It will also be remarked that though Simla is 7100 feet above the sea, the movements are not diminished by this diminution of the superambient air; what part the higher latitude may have in this result cannot at present be told.

Remembering the distances of the stations, it will be seen that no theory of propagation of waves by convection-currents or by rarefactions and condensations due to them, founded on the facts as yet known to us, can explain these maxima and minima which occur nearly simultaneously on the level of the sea at Singapore and Madras, at St. Helena 1800 feet and at Simla 7100 feet above the sea-level, under conditions of temperature, humidity, clouds, winds, and weather dissimilar in every respect*. The resemblance of the variations considered to those of the earth's magnetic force suggests the idea that they may be due to different modes of action of the same cause.

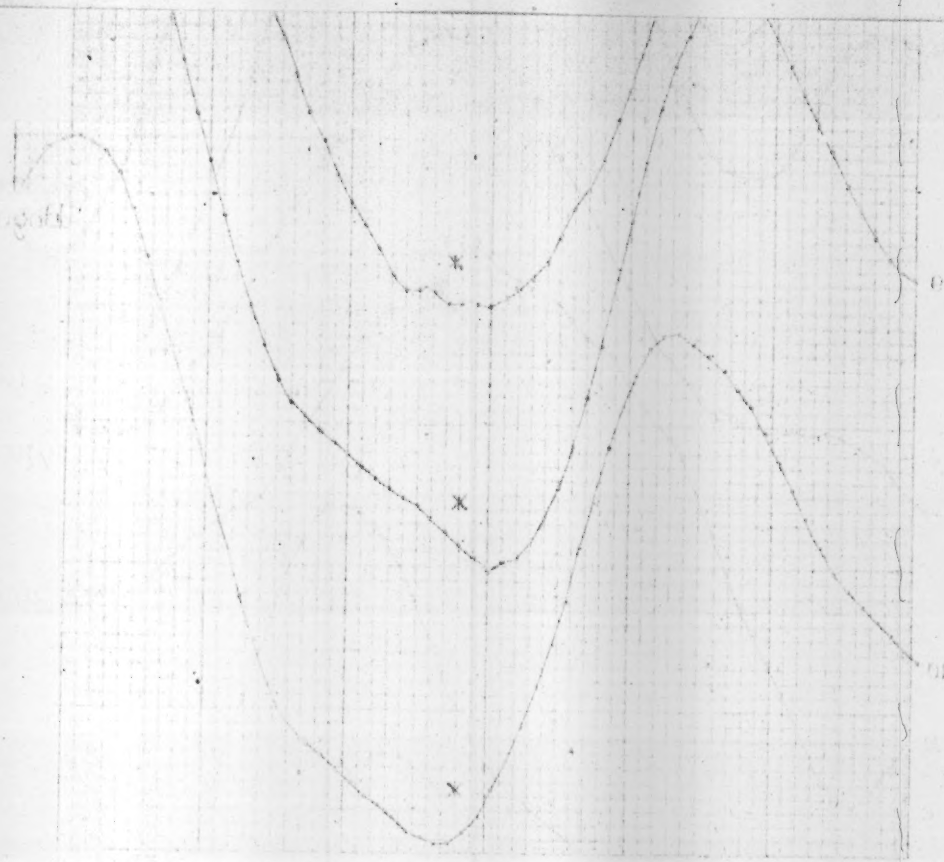
I have no doubt that by more extended investigations we shall arrive at the explanation of many meteorological phenomena by the action of this cause, which has not hitherto been taken into account. I shall conclude this note with the words of De Luc in the dedication of his work on the atmosphere to the members of the French Academy of Sciences:—"J'ai cru m'apercevoir, en étudiant la physique, que, depuis que cette science s'est assez étendue pour qu'on ait formé des systèmes sur presque tous les objets qu'elle embrasse, deux préjugés contraires nuisoient également aux progrès de la vérité; trop de défiance contre les solutions jugées impossibles, et trop de confiance dans celles qui sont adoptées"†.

Received May 24, 1876.

Additional remark.—On reading the preceding note to the Royal Society, I pointed out that one of the most marked exceptions to the

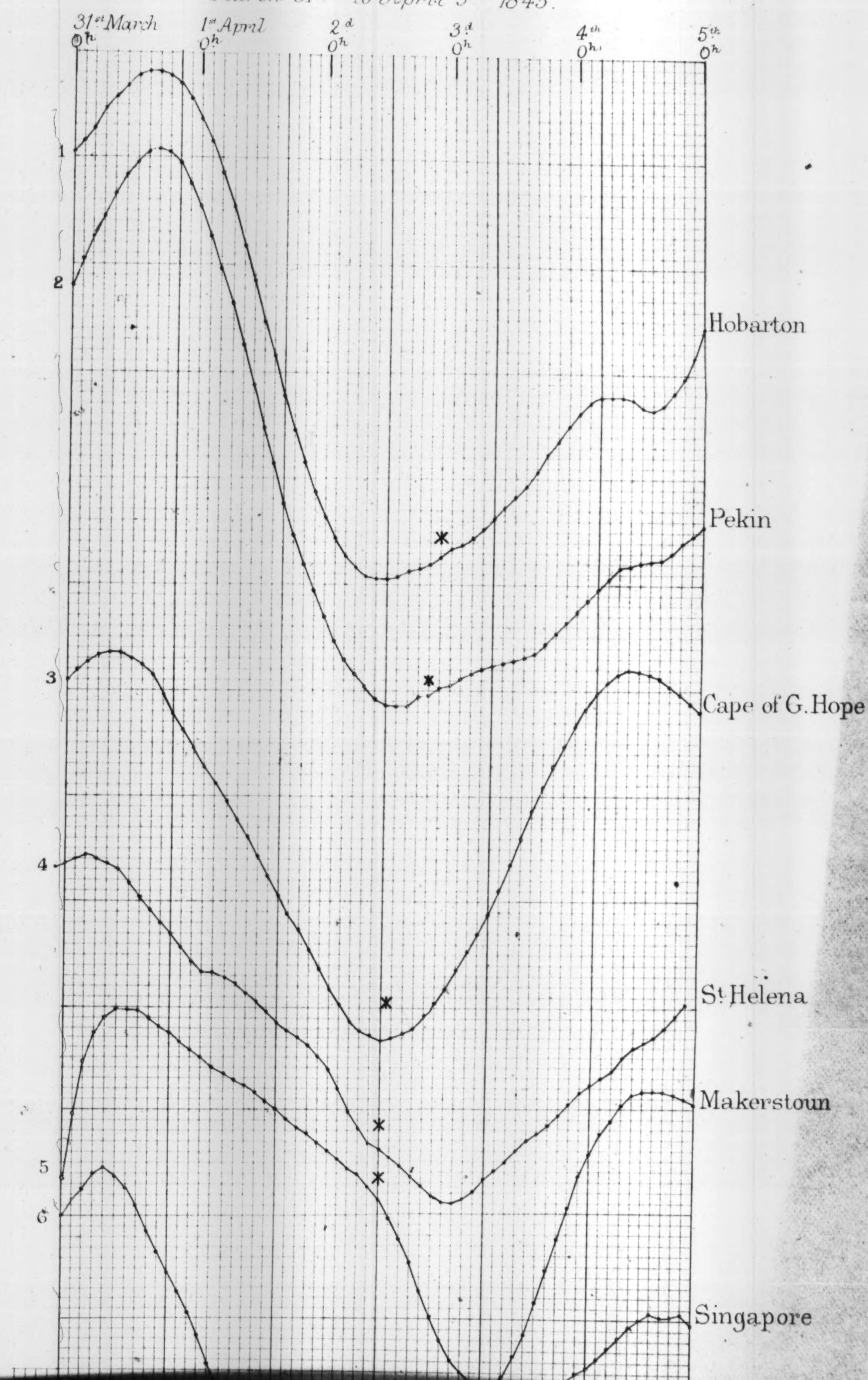
* *E. g.*, it snowed all day on the 24th January at Simla, while at Madras and Singapore the daily mean temperatures were those of the month at each place, or 70° and 79° respectively.

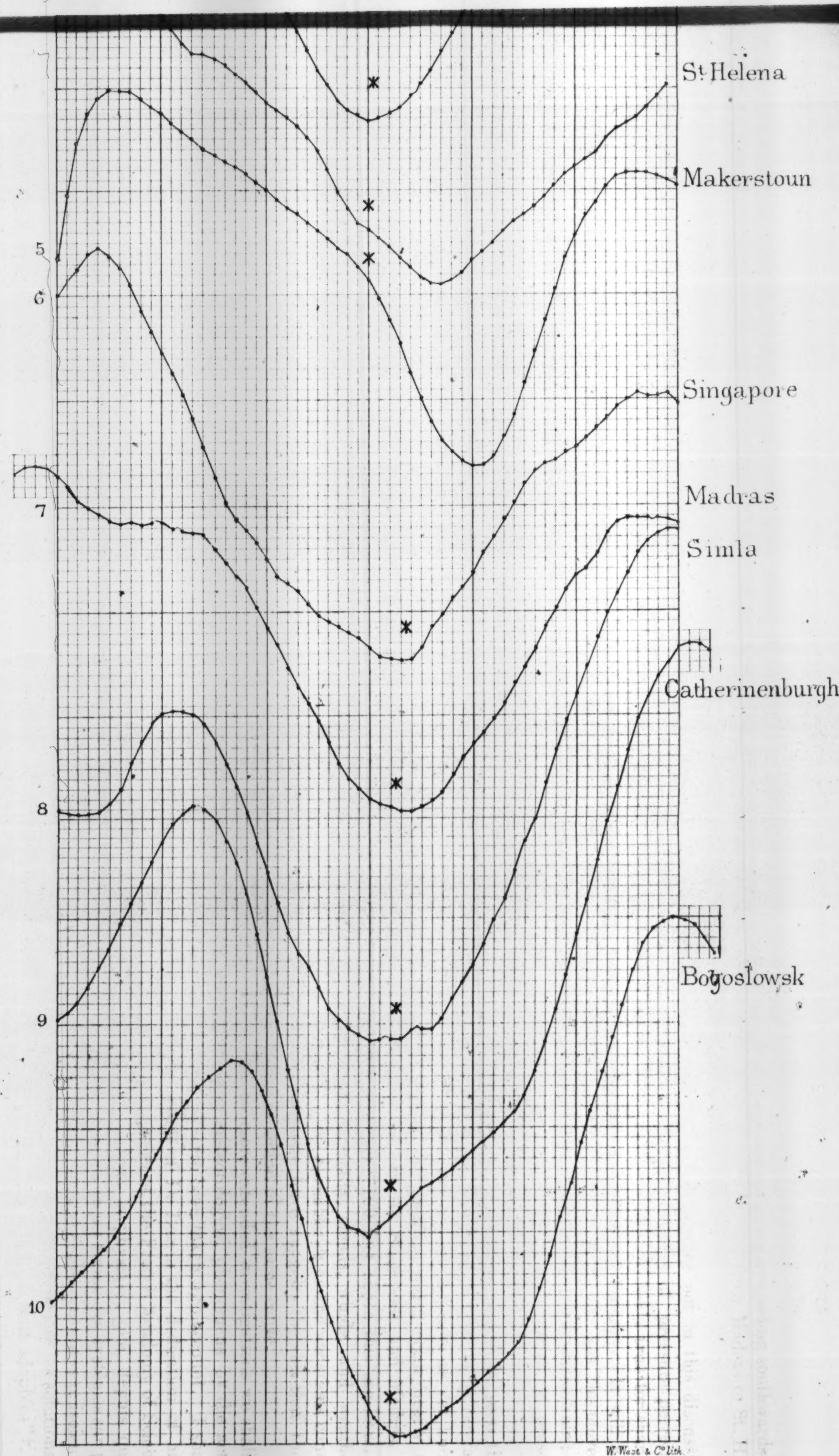
† Recherches sur les modifications de l'atmosphère. Par J. A. de Luc. Nouvelle édition. Par,s, 1784.



Proc. Roy. Soc. Vol. 25. Pl. 1.

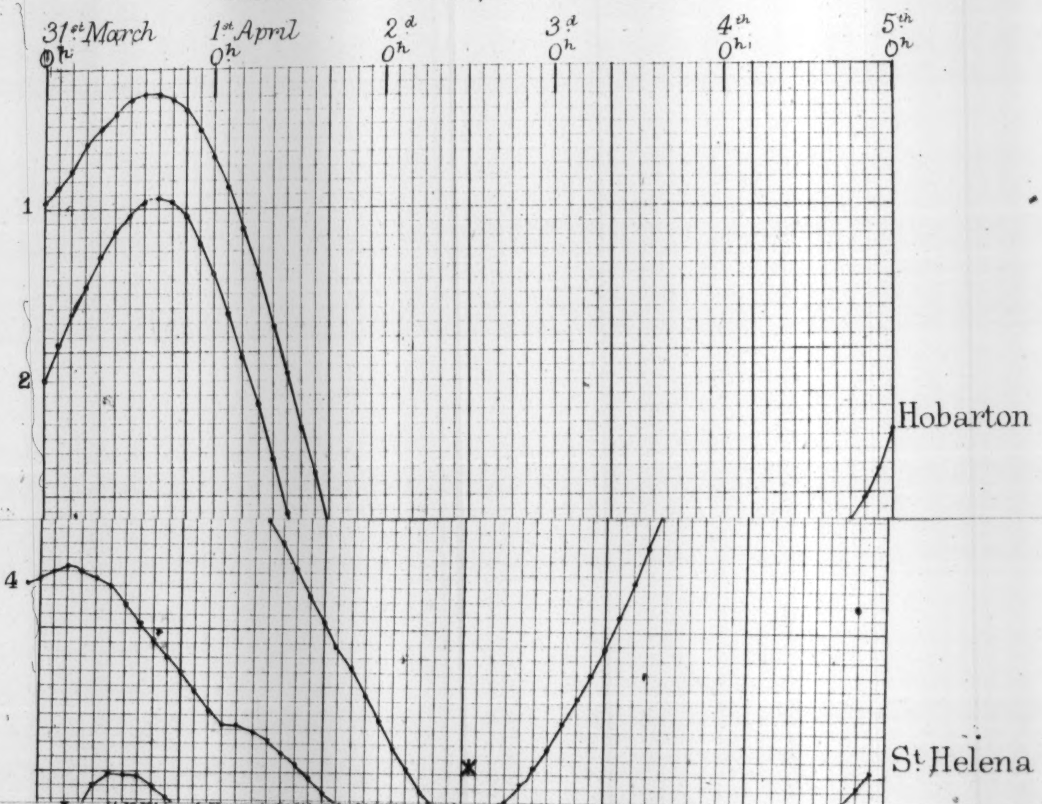
Daily mean barometric height corresponding to each hour March 31th to April 5th 1845.





Scales: ·8 inch = 0·1 inch of Mercury for Nos. 1, 2, 3, 9 & 10.
3·2 " = 0·1 " " " " 4, 6, 7, 8
1·28 " = 0·1 " " " " 5.
* April 2d. 12h. Greenwich mean time.

Daily mean barometric height corresponding to each hour
March 31th to April 5th 1845.



* April 2d 12h Greenwich mean time.

rule of similar movements at all the stations occurred on the 14th and 15th of February. From 10 A.M. of the 14th to 10 P.M. of the 15th, the mercury fell nearly 0·3 (three tenths) of an inch at Simla; no other fall of nearly equal amount occurred within the same space of time during the three months under consideration; yet this, the largest of all the atmospheric disturbances, was apparently unfelt at Madras and Singapore. On examining the weather registers at the three stations, it was found that there was a great thunderstorm at Simla, which began on the horizon on the 14th of February and continued throughout the 15th. There was nothing unusual at the other stations.

As the daily mean height of the barometer was less on Monday at noon than on Saturday at noon at Simla, the fall of the mercury probably continued during Sunday the 16th. This great atmospheric disturbance lasting during three days was not propagated even to Madras, the nearest station; while the smallest of the other movements, some less than 0·01 (one hundredth) of an inch, were felt nearly equally well, and nearly simultaneously, at all the three stations.

We see here a distinction between local causes of atmospheric disturbance and that other cause which produces so many nearly simultaneous movements; it is also easily understood that the larger deviations, of 16 or 24 hours, from absolute simultaneity may be due to similar though much smaller superposed local disturbances, a fact which an examination of the weather registers confirms.

On the other hand, the great continuous fall of the barometer at all the three stations, from the 19th to the 24th March, does not appear to have been accompanied by any other unusual atmospheric change at any one of the three stations.

V. "Supplementary Note on Simultaneous Barometric Variations." By J. A. Broun, F.R.S. Received June 20, 1876.

[Plate 1.]

It has been pointed out in the preceding note that as we leave the tropics and approach the higher latitudes we find greater apparent irregularity in the variations of the barometric height from day to day; these irregularities are due, I believe, to different causes—one being a change in the amount, and even in the direction, of the action of the cause which produces so frequently within the tropics similar and simultaneous movements. This change depends probably on local conditions which affect the medium through which the actions are produced. Other causes are to be found which produce variations in the mass of air above the barometer. It is not to be expected, then, that the agreement shown generally in the barometric movements at the Indian stations would appear were the investigation extended to higher latitudes; at the

same time the great similarity which has presented itself between the movements at St. Helena and at the Indian stations during the week March 31 to April 5, 1845, induced me to make a comparison for that week with the barometric movements at the Cape of Good Hope, nearly in the same longitude as St. Helena.

The results of this comparison were so important that observations at other stations were also examined.

The geographical coordinates of the different stations considered are as follow :—

Stations.	Latitude.	Longitude from Greenwich.	Height above Sea.
	° ′	h m	feet.
1. Hobarton	42 52 S.	9 50 E.	105 *
2. Pekin	39 54 N.	7 46 E.	Few †
3. Cape of Good Hope	33 56 S.	1 14 E.	,, *
4. St. Helena	15 57 S.	0 23 W.	1764 *
5. Makerstoun	55 35 N.	0 10 W.	213 ‡
6. Singapore	1 19 N.	7 16 E.	Few §
7. Madras	13 4 N.	5 21 E.	30 §
8. Simla	31 6 N.	5 9 E.	7096 §
9. Catherinenburg	56 50 N.	4 2 E.	1000 ? †
10. Bogoslowsk	59 45 N.	4 0 E.	1400 ? †

The observations made at these stations during the week March 31 to April 5 were discussed in the same manner as before, so as to obtain the daily means corresponding to each hour or two hours ||.

The daily means thus obtained are projected, Plate 1. Since the daily movements were found much smaller within the tropics than in high latitudes, the curves are projected on different scales to make the variations equally distinct ¶. The following are the principal conclusions from these projections.

All the curves show a maximum near the beginning and another near the end of the week, with a minimum near the middle. The turning-points occur in the following order at the different stations :—

* 1, 3, 4. Observations made at the Mag. and Met. Obs. at Hobarton in Van Diemen Island, at the Cape of Good Hope, and at St. Helena, printed under the superintendence of Colonel E. Sabine.

† 2, 9, 10. Annuaire Mag. et Mét. publiées par A. T. Kupffer, Année 1845.

‡ 5. Observations in Mag. and Met. Edited by John Allan Broun. Trans. Roy. Soc. Edin. vol. xix.

§ Previously cited.

|| At Pekin the observations were made two-hourly from 5^h A.M. to 9^h P.M., and the barometric heights for 11^h P.M., and 1^h and 3^h A.M. were found by interpolation for this discussion: similarly for Bogoslowsk, where observations were made from 8^h A.M. to 10^h P.M., the heights for the even hours from midnight to 6^h A.M. were obtained by interpolation.

¶ It should again be remarked that the hours at the head of the vertical lines are the local hours for each station: the vertical line corresponding to 12^h April 2^d, Greenwich mean time, is marked with an asterisk for each curve.

Stations.	Max. local time.	Stations.	Min. local time.	Stations.	Max. local time.
	d h		d h		d h
Madras	Mar. 30 20	Hobarton .	Apr. 2 10	Hobarton .	Apr. 4 7
St. Helena.........	„ 31 4½	Cape	„ 2 12	Pekin	„ 4 8?
Singapore	„ 31 8	Simla	„ 2 12	Cape	„ 4 11
Cape of G. Hope .	„ 31 9	Cather. ...	„ 2 12	Makers. ...	„ 4 16
Makerstoun	„ 31 11	Pekin	„ 2 14	Madras ...	„ 4 18½
Hobarton	„ 31 15	Singapore .	„ 2 19	Singapore .	„ 4 19
Pekin	„ 31 16	Madras ...	„ 2 19	Bogos. ...	„ 4 23
Simla	„ 31 23	Bogos. ...	„ 2 19	Simla	„ 5 0
Catherinenburg...	Apr. 1 3	St. Helena.	„ 3 1	Cather. ...	„ 5 3
Bogoslowsk	„ 1 9	Makers. ...	„ 3 8	St. Helena.	„ 5 12?

At Madras a secondary maximum appears at $31^d\ 19^h$, agreeing nearly with the mean of the epochs for Pekin and Simla. At Pekin a marked maximum occurs at $5^d\ 18^h$; the time given above refers to the inflection corresponding with the secondary maximum at Hobarton: the principal maximum at the latter station occurs nearly 24 hours later.

It will be seen that the succession is different for the different turning-points, so that no general law of precedence can be deduced relatively either to latitude or longitude.

The movements for the two most easterly stations, Hobarton and Pekin, have been projected first; and as the difference of latitudes is nearly 83°, the agreement of the two curves will appear very remarkable. The first maximum and the following minimum occur nearly simultaneously at the two places; while even the secondary maximum and minimum which follow at Hobarton are seen at Pekin in a distinct inflection, the mercury rising thereafter to a maximum at both stations.

At the Cape of Good Hope the curve is very regular with two equal branches, having the maxima and the minimum within a few hours of those for Hobarton and Pekin.

The curves for St. Helena and Makerstoun, the two most westerly stations, have been projected together; at both the minimum occurs later than at the other stations*.

* Differences were expected to be the rule and not the exception in this investigation, and it has not been thought necessary to give curves for some stations merely to show that such differences exist; as, however, the movements have been examined by me, I shall note that at St. Petersburg the first maximum occurs at the same time as at Bogoslowsk, which is nearly in the same latitude, but the second maximum occurs 12 hours and the minimum 24 hours later at the former than the latter station. Also at Nertchinsk, 11° north of Pekin, the first maximum occurs 4 hours later than at Madras, and the second maximum (at $5^d\ 5^h$) 8 hours later than at Catherinenburg (7 hours earlier than the last maximum at Pekin); but the principal minimum occurs 24 hours *before* that at Pekin, and is followed by a secondary maximum and minimum not shown at the other stations. In general at European stations the minimum appears to be retarded as at Makerstoun. At Toronto there are three maxima and three minima during the week. The object of this note has been to show the general action of the same cause over the earth; the deviations from the same types must be the subject of other researches.

When we consider the ranges of the oscillations at the different stations we find them to be as follows:—

Stations.	1st Max. to Min.	Min to 2nd Max.
	in.	in. in.
Hobarton	0·397	0·146 or 0·600?*
Pekin	0·430	0·108?,, 0·244 *
Cape of Good Hope	0·305	0·294
Catherinenburg	0·343	0·475
Bogoslowsk	0·269	0·410
Makerstoun	0·189	0·145
St. Helena	0·070	0·050?
Singapore	0·083	0·054
Madras	0·069	0·059
Simla	0·066	0·103

Thus Simla, though in nearly as high a latitude as the Cape of Good Hope, belongs by the range to the tropical series.

It is of much importance to observe that we have here to deal with the great atmospheric movements experienced in high latitudes. Thus the change of observed barometric height from minimum to maximum at Catherinenburg was nearly 0·6 inch; while at Makerstoun, though the variation of the *daily mean* pressure was less than at the other

---

Postscript, received 1st July.—Sinee writing the preceding note I have examined the barometric observations made at Sitka (latitude 57° 3′ N., longitude $14^h\ 58^m$ east of Greenwich). I find that there are two minima and two maxima within the week under consideration; these are as follow:—

Local Mean Time.	Daily Mean.	Range.	Local Mean Time.	Observed Height.	Range.
d h	in.	in.	d h	in.	in.
Mar. 31 18	Min. 29·277		31 22	29·123	
		0·483			0·720
Apr. 1 21	Max. 29·760		1 22	29·843	
		0·269			0·531
,, 2 19	Min. 29·491		2 20	29·312	
		0·824			1·069
,, 4 12	Max. 30·315		4 11	30·381	

It will be seen that while the first minimum is not shown at any of the other stations *after* March $31^d\ 0^h$, and the first maximum occurs twelve hours later than at Bogoslowsk, yet the second minimum occurs nearly at the same local hour as at the Asiatic stations, and the second maximum at the same local hour as at the Cape of Good Hope. The movements of the mercury are still larger than in any of the other cases, the change from the minimum, April $2^d\ 20^h$, to the maximum observation, $4^d\ 11^h$, being upwards of one inch. The whole series of facts leaves, it appears to me, no doubt that this great movement is connected with the same cause which produces the comparatively small variations within the tropics; and it may be noted that as we approach the poles the amount and irregularity of the barometric oscillations seem to increase, as in the case of the magnetic variations.

* These ranges refer to the maximum after $5^d\ 0^h$.

northern stations, yet, to attain the maximum shown at $31^d\ 12^h$, the mercury rose 0·40 inch within twelve hours during the 31st of March. I need scarcely point out the weighty bearing which these facts must have on all investigations with reference to the great barometric oscillations within our latitudes as well as to those of lesser magnitude within the tropics.

The following Table contains the daily mean height of the barometer at each station at the hours of maximum and minimum previously given, together with the mean height for the year.

Stations.	1st Max.	Min.	2nd Max.	Mean of year.
	in.	in.	in.	in.
Hobarton	30·067	29·670	29·816	29·794
Pekin	30·131	29·701	29·809 ?	30·015
Cape of Good Hope	30·155	28·848	30·142	30·058
St. Helena	28·309	28·239	28·289 ?	28·296
Makerstoun	29·959	29·770	29·915	29·586
Singapore	29·947	29·864	29·918	29·895
Madras	29·854 ?	29·785	29·844	29·853
Simla	23·184	23·118	23·221	23·195
Catherinenburg	28·967	28·624	29·099	29·023
Bogoslowsk	28·567	28·298	28·708	28·746

It will be perceived that the minimum height was less at all the stations, with the exception of Makerstoun, than the mean for the year.

VI. "On Clairautian Functions and Equations." By Capt. Allan Cunningham, R.E., Hon. Fellow of King's Coll. Lond. (Roorkee, India). Communicated by Prof. Cayley. Received April 18, 1876.

(Abstract.)

Notation.—In this paper D stands for $\frac{d}{dx}$; $y', y'' \ldots\ldots y^{(n)}$ stand for the differential coefficients of y (and therefore y^0 is equivalent to y itself); X, X_1, X_2, &c. stand for known functions of x; $X', X'' \ldots\ldots X^{(m)}$, $X_1', X_1'' \ldots\ldots X_1^{(m)}$, &c. stand for the differential coefficients of X, X_1, &c.; y_m stands for a particular integral of a linear differential equation; y_0 stands for the complete arbitrary portion of the solution of a linear differential equation.

1. *Clairautian Functions.*—It is proposed to apply the term Clairautian function to the following expressions (which possess properties similar to that on which the solution of "Clairaut's equation" is founded), viz.

$$y^{(n)},\ ky^{(n-1)} - xy^{(n)},\ \frac{k(k+1)}{\lfloor 2}y^{(n-2)} + \frac{k}{\lfloor 1} \cdot \frac{(-x)}{\lfloor 1}y^{(n-1)} + \frac{(-x)^2}{\lfloor 2}y^{(n)}, \cdot \quad (1)$$

and to denote them by the symbols ${}^kU_{0,\,n}$, ${}^kU_{1,\,n}$, ${}^kU_{2,\,n}$, $\ldots\ldots {}^kU_{n,\,n}$, so that

$$
{}^k\mathrm{U}_{r,\,n}=\frac{k(k+1)\,(k+2)\ldots.(k+r-1)}{\underline{|r}}y^{(n-r)}+\ldots\ldots
$$

$$
+\frac{k(k+1)}{\underline{|2}}\frac{(-x)^{r-2}}{\underline{|r-2}}y^{(n-2)}+\frac{k}{\underline{|1}}\frac{(-x)^{r-1}}{\underline{|r-1}}\cdot y^{(n-1)}+\frac{(-x)^r}{\underline{|r}}y^{(n)}, \quad . \quad (1)
$$

$$
=\Sigma_{p=o}^{p=r}\frac{\Gamma(k+r-p)}{\Gamma(k)\,.\,\underline{|n-p}}\cdot\frac{(-x)^p}{\underline{|p}}y^{(n-r+p)}, \quad \ldots\ldots\ldots \quad (1a)
$$

$$
{}^k\mathrm{U}_{n,\,n}=\frac{k(k+1)\,(k+2)\ldots.(k+n-1)}{\underline{|n}}y+\ldots\ldots
$$

$$
+\frac{k(k+1)}{\underline{|2}}\frac{(-x)^{n-2}}{\underline{|n-2}}y^{(n-2)}+\frac{k}{\underline{|1}}\frac{(-x)^{n-1}}{\underline{|n-1}}y^{(n-1)}+\frac{(-x)^2}{\underline{|r}}y^{(n)} \quad . \quad . \quad (1)
$$

$$
=\Sigma_{p=o}^{p=n}\frac{\Gamma(k+n-p)}{\Gamma(k)\,.\,\underline{|n-p}}\frac{(-x)^p}{\underline{|p}}y^p. \quad \ldots\ldots\ldots \quad (1a)
$$

They will be distinguished as of nth *order*, rth *rank*, and kth *class*; r and n are supposed always *positive integers*, and r not $>n$; and k may be any quantity whatever. It is obvious that there are

$(n+1)$ Clairautians of nth order;
$(r+1)$ terms in a Clairautian of rth rank *in general*;
$(1-k)$ terms in a Clairautian of rth rank, when k is *zero* or a *negative* integer numerically $<r$. (2)

Thus a Clairautian is a differential expression, the order and rank of which determine the orders of its highest and lowest differential coefficients; it is also obvious that the difference of the exponent (p) of x and order $(n-r+p)$ of differential coefficient is the same in every term.

2. *Clairautian Equations.*—It is proposed to term a differential equation involving Clairautian functions a CLAIRAUTIAN EQUATION. Upon the important properties proved in arts. 4, 5, the solution of many such differential equations may be founded and effected with elegance. These will be developed in what follows.

In consequence of the limitation of r, n as positive integers (art. 1), the differential equations presented will all be of the ordinary type, that is, involving the differential symbol D *only in a rational integral form.* "General differentiation" will be freely used when necessary to the generality of a solution (so that the quantity k may have any value). Any difficulty that may be felt in the interpretation of the transcendent $D^k 0$ (when k is not an integer) will generally disappear in the final results, such transcendents, in fact, cancelling.

3. *Algebraic relations.*—It is easy to see that the Clairautians of zero rank $(r=0)$ are simple differential coefficients,

$$
{}^k\mathrm{U}_{0,\,0}=y,\ {}^k\mathrm{U}_{0,\,1}=y',\ {}^k\mathrm{U}_{0,\,2}=y'',\ \ldots\ldots\ {}^k\mathrm{U}_{0,\,n}=y^{(n)}; \quad \ldots\ldots \quad (3)
$$

also that those of zero class ($k=0$) are

$$^0U_{0,n}=y^{(n)},\quad {^0U_{1,n}}=\frac{-x}{\lfloor 1}y^{(n)},\quad {^0U_{2,n}}=\frac{(-x)^2}{\lfloor 2}y^{(n)},\ \ldots\ldots$$

$$\ldots\ {^0U_{n,n}}=\frac{(-x)^n}{\lfloor n}y^{(n)}. \quad (4)$$

It is easy also to establish by expansion and comparison of coefficients that

$$^kU_{r,n}=\Sigma_{p=0}^{p=r}\,{^{k-1}U_{p,n-r+p}};\quad {^kU_{n,n}}=\Sigma_{p=0}^{p=n}\,{^{k-1}U_{p,p}},\ \ldots\ldots \quad (5)$$

$$^kU_{r,n}-{^{k-1}U_{r,n}}={^kU_{r-1,n-1}};\quad {^kU_{n,n}}-{^{k-1}U_{n,n}}={^kU_{n-1,n-1}}.\ \ldots \quad (6)$$

4. *Differential properties.*—It is easily established by actual differentiation that

$$\begin{aligned}&D\ .\,{^kU_{r,n}}={^{k-1}U_{r,n+1}};\quad D^2.\,{^kU_{r,n}}={^{k-2}U_{r,n+2}};\ \ldots\ldots\\ &D^p.\,{^kU_{r,n}}={^{k-p}U_{r,n+p}}.\ \ldots\ldots\ldots\ldots\ldots\end{aligned} \quad (7a)$$

Hence also, by the theory of "general differentiation,"

$$\left.\begin{aligned}&D^{k-p}.\,{^kU_{r,n}}={^pU_{r,k+n-p}},\ \ldots\ldots\ldots\ldots\\ &D^{k-1}.\,{^kU_{r,n}}={^1U_{r,k+n-1}},\ D^k.\,{^kU_{r,n}}=\frac{(-x)^r}{\lfloor r}y^{(k+n)},\ \ldots\ldots\end{aligned}\right\}(7b)$$

whatever be the value of k (omitting all arbitrary terms).

These results may be thus expressed in words:—

1°. Simple differentiation *depresses the class*, and *raises the order* of a Clairautian, without affecting its rank.

2°. All Clairautians of same class (k) and order (n), after k differentiations (in the general sense), contain, omitting arbitrary terms, a common factor $y^{(k+n)}$, and are therefore kth integrals of $y^{(k+n)}$.

3°. Hence also all the above quantities may be expressed as integrals of the last of them, $^0U_{r,k+n}$, or of $\dfrac{(-x)^r}{\lfloor r}.\,y^{(k+n)}$. (8)

5. *Symbolic forms.*—It is obvious from results (7), (8), that (omitting arbitrary terms)

$$x^kD^k.\,{^kU_{r,n}}=\frac{(-1)^r}{\lfloor r}.\,x^{k+r}D^{k+n}y=\frac{(-1)^r}{\lfloor r}.\,x^{k+r}D^{k+r}y^{(n-r)}.\ \ldots \quad (9)$$

Hence, by the known theorem—

$$x^mD^m=\frac{\lfloor xD}{\lfloor xD-m},\ \text{or}\ \frac{\Gamma(xD+1)}{\Gamma(xD-m+1)}\ \textit{in general},$$

it follows that

$$\left.\begin{aligned}{}^k\mathrm{U}_{r,\,n}&=\frac{(-1)^r}{\underline{|r}}\,.\,(x^k\mathrm{D}^k)^{-1}\,.\,(x^{k+r}\mathrm{D}^{k+r})\,.\,y^{(n-r)}, \quad \ldots\ldots \\ &=\frac{(-1)^r}{\underline{|r}}\,.\,\frac{\Gamma(x\mathrm{D}-k+1)}{\Gamma(x\mathrm{D}-k-r+1)}\,.\,y^{(n-r)}, \quad \ldots\ldots \\ &=\frac{(-1)^r}{\underline{|r}}\,.\,x^k\,\frac{\underline{|x\mathrm{D}}}{\underline{|x\mathrm{D}-r}}\,.\,x^{-k}x^{(n-r)}, \quad \ldots\ldots \\ &=\frac{(-1)^r}{\underline{|r}}\,.\,x^{k+r}\mathrm{D}^r\,.\,x^{-k}y^{(n-r)}. \quad \ldots\ldots\end{aligned}\right\}\quad(10a)$$

$$\left.\begin{aligned}{}^k\mathrm{U}_{n,\,n}&=\frac{(-1)^n}{\underline{|n}}\,.\,\frac{\Gamma(x\mathrm{D}-k+1)}{\Gamma(x\mathrm{D}-k-r+1)}\,.\,y, \quad \ldots\ldots \\ &=\frac{(-1)^n}{\underline{|n}}\,.\,x^k\,\frac{\underline{|x\mathrm{D}}}{\underline{|x\mathrm{D}-n}}\,.\,x^{-k}y, \quad \ldots\ldots \\ &=\frac{(-1)^n}{\underline{|n}}\,.\,x^{k+n}\mathrm{D}^n\,.\,x^{-k}y. \quad \ldots\ldots\end{aligned}\right\}\quad(10b)$$

Observe that, since r, n are supposed positive integers, these symbolic ratios always consist of a finite number of factors (viz. r, n respectively), and are therefore always interpretable, whatever be the value of k.

May 18, 1876.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

1. " Absorption-Spectra of Iodine." By Sir JOHN CONROY, Bart., M.A. Communicated by A. G. VERNON HARCOURT, Lee's Reader in Chemistry in the University of Oxford. Received April 12, 1876.

Iodine, as is well known, when in very thin layers, appears red by transmitted light; and when in solution the colour of the liquid depends not only on the amount of iodine contained in it, but also on the nature of the liquid in which it is dissolved.

Schultz-Sellack has pointed out (Pogg. Ann. vol. cxl. p. 334) that the liquids in which iodine is soluble may be divided into two classes :—

first, those with which it gives reddish-brown solutions, like alcohol; and secondly, those with which it gives violet ones, as bisulphide of carbon; and also that the colours of these two solutions correspond respectively with the colour of solid iodine, when seen by transmitted light, and with that of iodine vapour.

Andrews (Brit. Assoc. Report, 1871) has also remarked that iodine vapour and the solution of iodine in bisulphide of carbon are dichroic, while such is not the case with its solution in alcohol.

As I am not aware of any other observations on the absorption of light by iodine in solution or in the solid state having been published, I have the honour of having an account of some experiments I have recently made on this subject communicated to the Royal Society.

For these observations I have used one of Browning's spectroscopes with a single dense-glass prism of 60°, as with a greater amount of dispersive power it became more difficult to observe the beginning and end of the absorption. The spectroscope was firmly screwed to the wall of the room, with the collimator pointing vertically downwards, the light from a paraffin-lamp being reflected along it by a mirror—the width of the slit and the position of the mirror and lamp remaining unaltered during the course of the experiments, in order that the different absorption-spectra should, as far as possible, be comparable with each other. The solution whose absorption was to be observed was contained in a small beaker, supported by the ring of a retort-stand between the mirror and the slit of the collimator.

This arrangement was adopted in order to be able to observe the absorption through various thicknesses of the same solution, without having to use a wedge-cell, as some of the liquids in which iodine is soluble act very quickly on the cement with which such cells are fastened together.

A vertical scale was attached to the beaker, so that by gradually pouring a solution into it, the absorption through different known thicknesses could be observed, the solutions of iodine in bisulphide and tetrachloride of carbon being covered with a thin layer of water to prevent their evaporating.

When the absorption-spectra of solid and liquid iodine were to be observed, the beaker was replaced by a large cork which fitted the ring of the retort-stand, and through which a hole had been bored in a line with the axis of the collimator, and the glass slips between which the iodine had been melted laid on this. In the case of the liquid iodine, the low conductive power for heat of the cork retarded the cooling of the glass, and facilitated the observation of the absorption.

The telescope of the spectroscope, the eyepiece of which was furnished with cross wires, was carried by an arm moving over a divided arc; and the position of 10 of the principal lines in the solar spectrum having been observed, from these measurements, and from the wave-

lengths of the same lines, as determined by Ångström, a curve was constructed, by means of which the readings of the spectroscope were reduced to wave-lengths.

Solid Iodine.

Layers of iodine sufficiently thin to be transparent can be readily obtained, as Schultz-Sellack has remarked, by squeezing melted iodine between two pieces of flat, well-polished glass: it is only necessary to place a small fragment of iodine between two pieces of glass which have been previously well cleaned with alcohol, and heat them over a spirit-lamp till the iodine melts, and then press them together. I have obtained the best results by heating the iodine till it just melts, placing the pieces of glass on a smooth block of wood and squeezing them together with a flat cork.

The layers of iodine thus obtained are not usually of uniform thickness; and, in addition to this, they contain so little iodine that I was unable to determine their thickness by ascertaining the weight and area of the film. When seen, however, by reflected and transmitted light, the iodine film usually appears surrounded by coloured rings; and as these alter their position and shape when the glass slips are pressed together, they must be due to a thin layer of air, and not to any substance adhering to the glass; and consequently the layers of iodine are probably less than ·00004 inch, or ·001016 mm., in thickness.

When seen by transmitted light, these layers of iodine vary in colour from a deep brownish red, through different shades of brown, to a more or less pure yellow, according to the thickness and nature of the film; for, as is shown in the paper "On the Polarization of Light by Crystals of Iodine" (*infrà*, p. 51), the colour of the transmitted light apparently does not depend solely on the thickness of the layer of iodine through which it passes. These films correspond in colour with alcoholic solutions of iodine of different strength, and the absorption-spectra are very similar—the whole of the blue end of the spectrum being cut off, and the absorption extending further and further towards the less refrangible end of the spectrum, as the thickness of the film increases, till at length only light having a wave-length of about 650 (in "tenth-metres"), or slightly more refrangible than the C line, passes through; and a very slight increase in the thickness of the film is sufficient to stop this also.

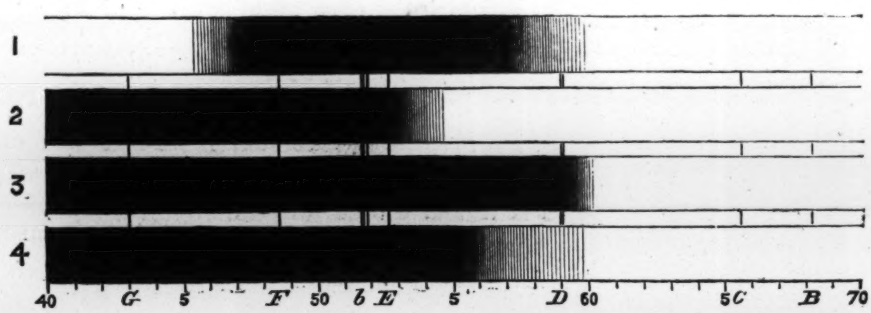
Fig. 4 shows the appearance with a film of medium thickness, the strong absorption ending at a point about one third of the distance between E and D, together with a certain amount of darkening extending to about D, and represents the mean results from measurements of the absorption-spectra of seven different films of iodine.

Liquid Iodine.

With a little careful management it is usually possible to melt one of these thin layers of iodine by heating it over the flame of a spirit-lamp,

without causing it either to be dissipated in vapour or to run together. The liquid iodine appears to be more transparent than the solid; for a layer which appears a deep red colour by transmitted light when hot, sometimes becomes perfectly opaque on cooling. When hot it is also of a deeper red than when cold; and the spectroscope shows that while the less-refrangible rays of the spectrum are freely transmitted, there is more absorption of those of mean refrangibility than is the case with solid iodine.

Fig. 3 shows the absorption-spectrum as deduced from five observations. Just as is the case with solid iodine, light of a wave-length of about 650 suffers the least amount of absorption during its passage through a layer of iodine.



Iodine in Solution.

As I have before mentioned, the liquids in which iodine is soluble may be divided into two classes: first, those with which it forms brownish-red solutions, as alcohol, ether, ethyl bromide, Dutch liquid, benzole, glycerine, potassium iodide in an aqueous solution, hydrogen chloride, &c.; second, those with which it forms violet solutions: this latter class is less numerous, and as far as I have hitherto been able to ascertain by actual experiment, only consists of the following substances—the bisulphide, tetrachloride, and monochloride of carbon, chloroform, phosphorous terchloride, tin tetrachloride, and under certain circumstances, as will be hereafter mentioned, hydrogen sulphate. In Watts's 'Dictionary of Chemistry,' vol. i. p. 881, it is stated that the solution of iodine in chloral has a purple colour.

In order to obtain solutions of iodine of a known strength, some of the liquids in which it is soluble being very volatile, a small glass weighing-tube closed with a cork was nearly filled with iodine which had been crushed small with the edge of a platinum spatula. A stoppered flask holding 25 c. c. was filled with the liquid in which the iodine was to be dissolved; and the tube having been weighed, a small quantity of iodine was shaken out of it, and the tube again weighed, and so on, till the desired amount had been added to the liquid.

In this way solutions of iodine in bisulphide and tetrachloride of carbon,

chloroform, alcohol, and in aqueous solutions of potassium iodide were prepared, and the absorption produced by different thicknesses of these solutions observed.

Solutions of iodine in alcohol, and of iodine in an aqueous solution of potassium iodide, of different strengths were then prepared, and the absorption produced by layers of these solutions, varying in thickness from 5 mm. to 25 mm., was observed.

Solutions of iodine in both liquids were, when of equal strength, of the same colour and the absorption-spectra were similar, and also similar to that of the solid iodine, except that the absorption did not appear to extend quite so far down the spectrum ; but as the transition from strong absorption to none at all is a very gradual one, it is somewhat difficult to determine the exact point at which the absorption ends. Fig. 2 represents the mean results obtained from thirty observations through different thicknesses of the alcohol solutions.

Iodine dissolved in bisulphide of carbon absorbs most strongly light of mean refrangibility ; and the absorption-spectrum resembles that of the vapour of iodine as represented in the map to M. Thalen's memoir, 'Le spectre d'absorption de la vapeur d'Iode' (Upsal, 1869), except that the absorption is continuous, and does not extend quite so far towards the less-refrangible end of the spectrum.

Fig. 1 represents the mean result of eighty-one measurements of the position of the absorption produced by different thicknesses of these solutions. The rays whose wave-lengths lie between 450 and 560 (approximately) are first absorbed ; and as the thickness of the layer or the concentration of the solution is increased, the absorption extends towards both ends of the spectrum, though more rapidly towards the less-refrangible one.

Just as was the case with the solution of iodine in alcohol, and with solid and liquid iodine, light of a wave-length of about 650 passes unabsorbed through a considerable thickness of the solution ; but a thickness which is sufficient to stop the whole of the red rays still allows the blue and violet ones to pass ; hence, whilst dilute solutions of iodine in bisulphide of carbon and other liquids of that class appear of a kind of red, when the light passes through a greater thickness, or the solution is more concentrated, they appear blue or violet.

Iodine is insoluble in cold hydrogen sulphate ; but when some fragments of iodine are placed in a test-tube, partially filled with strong hydrogen sulphate, and the tube heated, the iodine first melts, and then gradually colours the liquid, till it becomes about the same tint as a very dilute solution of iodine in bisulphide of carbon.

·0100 grm. of iodine was placed in a test-tube containing 25 c. c. of strong hydrogen sulphate, and the test-tube carefully heated over a spirit-lamp until the whole of the liquid iodine had disappeared ; the acid appeared of the same colour as a solution of iodine in chloroform, containing about the same amount of iodine. The colour of the acid did not

alter on cooling; after standing 24 hours the upper layer of acid for a depth of about one centimetre from the surface had become colourless; but at the end of five months the acid in the lower part of the tube was still pink, the upper half having become colourless, and a small quantity of a black powder having settled at the bottom.

Only a very small quantity of iodine can be held in solution by the hydrogen sulphate when cold, as any excess separates out in minute crystals.

It does not appear probable that the difference in the colour of the solutions which iodine forms with liquids of these two classes depends on any chemical fact, as both classes contain substances of very dissimilar chemical composition. I have not, however, as yet been able to ascertain any common property possessed by all the liquids of either class, beyond (as, indeed, is obvious) that all those in which iodine forms violet solutions are volatile liquids of high specific gravity.

It has been shown by various observers (H. Morton, Pogg. Ann. vol. clv. p. 573; Hagenbach, Pogg. Ann. vol. cxlvi. p. 533; Kraus, 'Chlorophyllfarbstoffe,' p. 53) that the position of the absorption-bands of substances in solution vary to a certain extent with the liquid in which they are dissolved; but this would appear to depend on some other cause; for, in addition to the displacement being small, it differs in amount with different liquids; whilst in the case of iodine, as far as I have been able to observe, the position of the absorption is the same for all the liquids belonging to one of the two classes. The action on light of iodine dissolved in alcohol greatly resembles the effect it produces when in the solid state; whilst the absorption of its solution in carbon bisulphide, and in other liquids of that class, bears, as has been pointed out to me by Professor Stokes, the same relation to the absorption-spectrum of the vapour as the spectrum of the solution of a coloured gas (nitrogen peroxide for example) does to that of the gas.

II. "On the Polarization of Light by Crystals of Iodine." By Sir John Conroy, Bart., M.A. Communicated by A. G. Vernon Harcourt, Lee's Reader in Chemistry in the University of Oxford. Received April 12, 1876.

On examining by means of a Nicol prism the light reflected from the surface of a layer of iodine, obtained by heating a fragment of that substance and then squeezing it between two plates of glass, as described in the preceding paper, I found that the film did not appear of uniform brightness, and that when the Nicol was rotated the relative brilliancy of different parts of the film changed—a portion that had appeared dark when the principal section of the Nicol was vertical, became bright when it was horizontal, and *vice versâ*; and, also, if instead of altering the

position of the Nicol, the film of iodine was rotated horizontally, the Nicol remaining at rest, the same changes in brilliancy occurred.

Removing the upper glass made no difference, except that the surface of the film of iodine tarnished rapidly, and then the amount of light reflected by it became considerably less.

The light incident upon the surface of the iodine was either ordinary diffused daylight or the light of a paraffine-lamp; and in neither case did it show more than the merest trace of polarization, and generally not even that, when examined by means of a double-image prism and a plate of selenite.

It therefore appears that the light reflected from the surface of a layer of iodine is polarized, and that the position of the plane of polarization is not, of necessity, either parallel or perpendicular to the plane of incidence, but bears a definite relation to some direction within the crystals composing the film.

I also found that when these films were sufficiently thin to be transparent the light they transmitted was polarized, and that the plane of polarization of the transmitted light was perpendicular to the plane of the light which was polarized by reflection from the same portion of the film.

After making these observations I ascertained that W. Haidinger had announced, upwards of twenty years ago (Pogg. Ann. clxxi. p. 321, 1847), that the surface-colours which certain substances show by reflected light, in the case of some of the platino-cyanides, namely, those of potassium, barium, and magnesium, consist partially of light polarized in a plane which bears a definite relation to the axis of the crystal; and in a subsequent paper (Sitzungsberichte der kaiserlichen Akademie der Wissenschaften, viii. p. 97, 1852) he states that certain other substances have the same property. He mentions iodine in this latter paper as showing these "surface-colours," but does not appear to have noticed that the plane of polarization of the light reflected from its surface bore any relation to some fixed direction within the substance.

I arranged a form of polariscope by means of which these observations could be repeated with a greater degree of accuracy. The instrument used consisted of a divided brass circle fixed vertically to a firm support; a Nicol furnished with a graduated circle was carried by an arm moving round the centre of the circle, and the slip of glass with the layer of iodine rested horizontally on a stage at the top of a tube, the height of which could be adjusted so that the surface of the iodine was level with the centre of the graduated brass circle. Both the stage and the tube revolved horizontally, and could be rotated independently of each other; and the latter had an index moving over a divided circle attached to it, and a diaphragm with an opening about 6 mm. wide fixed in it. By altering the position of the arm moving over the vertical circle the light reflected from the surface of the iodine at different angles could be ex-

amined; and by reflecting light along the axis of the tube, by a mirror placed below it, and clamping the arm in such a position that the axis of the tube and Nicol were in the same straight line, the polarization of the light transmitted by the iodine could also be observed. A second or polarizing Nicol was so arranged that it could either be brought below the tube or placed between the surface of the iodine and the source of light, so that the behaviour of the film, when the incident light was polarized, could be studied.

On repeating the before-mentioned observations with reflected light I found that occasionally portions of the film of iodine appeared quite black in certain positions of the film and Nicol, and that these same portions, when examined by transmitted light, did not merely alter in colour as the film or Nicol was rotated, as the remainder did, but in certain positions transmitted no light at all—or, in other words, that they behaved in a similar manner to what a plate of tourmaline would have done; and when seen by ordinary light and the naked eye, although they appeared to be of the same thickness as the remainder of the film, by which they were wholly or partially surrounded, they were of a much paler colour; usually, moreover, they reflected rather less light than the rest.

The change in the appearance of these portions of the film when seen through a Nicol was very striking in the case of some of the larger ones, as in certain positions they appeared perfectly transparent and of a pale yellow colour, and objects situated behind them could be clearly seen; but on turning either the film or the Nicol they became perfectly opaque, and resembled highly polished metallic surfaces.

On examining one of these films of iodine with a microscope with a $\frac{2}{3}$ object-glass I found that those portions of the film which polarized light most strongly differed considerably in appearance from the remainder, and that they appeared to consist of long crystals about ·003 inch wide adhering together side by side, whilst the rest of the film seemed to consist of thin plates of iodine overlaying one another, these, also, being long in proportion to their width.

A Nicol prism was placed over the eyepiece, and I then found that when the principal section of the Nicol was perpendicular to the long axes of the crystals, the maximum amount of light was transmitted, and when the principal section was parallel with the long axes of the crystals, they either appeared perfectly opaque or transmitted the minimum amount of light, according as the crystals in the field of view belonged to one or other class.

Other specimens of iodine showed this crystalline arrangement with different degrees of distinctness; but in all, or nearly all, some trace of it could be seen. In some cases, however, the minimum amount of light was transmitted when the principal section of the Nicol was not parallel with what appeared at first sight to be the long axes of the crystals; but

a more careful examination usually showed some traces of crystalline structure in a direction parallel with the principal section of the Nicol.

I shall refer to these two forms of iodine as iodine α and iodine β, calling those portions of the film α which are of a darker colour and polarize the light imperfectly, and the light-coloured strongly polarizing parts β.

From the appearance of the film, when seen under the microscope, it appears probable that this difference depends merely on the arrangement of the crystals, and that when they are regularly disposed in a single layer the film is one of those which I have called iodine β; whilst iodine α consists of several layers of thin crystals lying in various directions, or it may be due to different faces of the crystals of iodine being in contact with the glass, and to the light passing through the crystals in a different direction.

The difference between the action of the crystalline film on the transmitted light is one of degree only; for I obtained two specimens of iodine β in which the film was of unequal thickness; and in this case, when the principal section of the Nicol was parallel with the long axes of these crystals, the thick portion of them appeared opaque, but a considerable amount of light was transmitted by the thinner portions of the very same crystals. Moreover several specimens of iodine β which appeared perfectly opaque in certain positions of the Nicol when seen by ordinary daylight, were of a deep red colour when examined by direct sunlight; and I have recently succeeded in preparing several films of iodine β so thin that they were only opaque when seen through a Nicol, whose principal section was parallel with the length of the crystals, with very weak light; by ordinary daylight they appeared of a deep red colour under these circumstances.

Iodine α.—Films of iodine α between two slips of glass were laid on the stage of the polariscope, and the light they transmitted examined with the analyzing Nicol; on turning either the stage or the Nicol, the colours of the film varied, according to their thickness and the relative positions of the film of iodine and the Nicol, from a kind of brownish yellow to a deep red, the colours being similar to those of solutions of iodine in alcohol of various strengths.

When the incident light was polarized, and the film of iodine placed so as to transmit the minimum amount of light, or at right angles to this position, the field was dark when the Nicols were crossed and light when they were parallel. When, however, the film of iodine was in an intermediate position, the field was no longer dark in any position of the analyzer, the colour and intensity of the light varying slightly as it was turned.

Iodine β.—A film of iodine β of a yellowish brown colour was examined by transmitted light; the field appeared perfectly dark in two positions of the film and Nicol. With polarized light, when the film was

placed at right angles to the position in which it transmitted no light, the field was dark when the Nicols were crossed and light when they were parallel. When, however, the iodine film was placed in an intermediate position, the field was no longer dark when the Nicols were crossed, though it was so in two positions of the analyzer 180° apart.

In order to simplify the description of the experiments, I shall speak of the direction in the film which, when placed parallel with the principal section of the Nicol, caused the field to appear dark, or in the case of iodine α to be least bright, as the axis of the crystal.

Very thin films of iodine β, as I have mentioned before, are not opaque when the principal section of the Nicol is parallel with the length of the crystals, and, when examined with the polariscope, appear of a deep red colour, when, under similar circumstances, a thicker film would transmit no light at all.

Hence it would appear that iodine belongs to the class of double refracting substances in which the coefficient of absorption differs according to the direction in which the light passes through the crystal, and, further, that the ray whose plane of polarization is perpendicular to the axis of the crystal is most energetically absorbed.

This is the case with both forms of the crystalline layer of iodine; but the two rays are much more unequally absorbed by iodine β than by iodine α—so much so that whilst the latter only appears absolutely opaque when the principal section of the Nicol and the axis of the crystal are parallel, when the film is so thick that but little light can pass through under any circumstances, the former absorb the one ray so energetically that a layer which appears light yellow when the Nicol is in one position is absolutely opaque when it is turned through an angle of 90°.

When a thin film of iodine β is seen through a Nicol whose principal section is so placed that the minimum amount of light is transmitted, the light usually appears of the same colour and brightness as that which has passed through the adjacent portions of the film consisting of iodine α; and it is impossible to see where one form of the film ends and the other begins. From this it would appear as if both forms of the crystalline layer absorbed light polarized in a plane perpendicular to the axis of the crystals with equal intensity, but that they differ greatly in their absorptive powers for light polarized in a plane at right angles to this.

I have shown, in the preceding paper, that solutions of iodine in alcohol, when seen by transmitted light, vary in colour, from a pale yellow to a deep red, according to the strength of the solution and the thickness of the layer through which the light has to pass. In a similar manner, in proportion as the thickness of the films of iodine increases, the light becomes more and more red; and four films of iodine β, which when seen separately were of a pale yellow, appeared of a deep red when super-

imposed, and so placed with respect to each other that they transmitted the maximum amount of light.

The light of a paraffine-lamp reflected from the surface of a film, consisting partly of iodine α and partly of iodine β between two slips of glass, was examined by means of the Nicol, the angle of incidence being about 60°.

When the principal section of the Nicol was in the plane of incidence, and when consequently but little of the light reflected from the surface of the glass was transmitted, portions of the film of iodine appeared of different degrees of brightness; and on rotating either the Nicol or the stand, the relative brilliancy of different portions of the film changed, those portions which consisted of iodine β appearing perfectly black in certain relative positions of the film and Nicol, whilst the remainder of the film merely became more or less bright.

The film of iodine was then placed so that the portion consisting of iodine β appeared perfectly black when the principal section of the Nicol was in the plane of incidence. On rotating the stand, light reflected from the surface of the iodine was transmitted by the Nicol, and increased in quantity till the stand had been turned through 90°, when the surface of the iodine had a brilliant metallic lustre. On continuing the rotation, the surface gradually lost its brilliancy, and when the stage had been turned through 180° appeared perfectly black again.

On rotating the Nicol the same changes took place; but the light reflected from the surface of the glass marked the effect to a considerable extent when the principal section of the Nicol was no longer in the plane of incidence.

The light incident upon the surface of the glass showed no signs of polarization when examined by a double-image prism and plate of selenite, and only the faintest trace of it after passing through, at an angle of about 60°, a slip of glass similar to those used for covering the layers of iodine; consequently the polarization of the light must be due to the film of iodine.

From this it appears that the light reflected from the surface of a film of iodine β is polarized; and by examining the light transmitted by the same portion of the film, it was ascertained that the plane of polarization of the reflected light is perpendicular to that of the ray which is most freely transmitted, and consequently that the reflected light is polarized in a plane at right angles to the axis of the crystals.

When the incident light was polarized, it was found that it was reflected from the surface of the iodine when the plane of polarization of the light was perpendicular to the axis of the crystals, and extinguished when parallel.

As has been stated before, when a film of iodine α is seen through a Nicol, it does not appear black in any position; but the brilliancy of the surface alters as the Nicol or iodine is rotated.

Experiments similar to those just described show that when a ray of plane polarized light is incident upon such a surface of iodine, it is never completely extinguished, as is the case with iodine β; but the intensity of the reflected light depends on the relative position of the plane of polarization and the axis of the crystals, being least when they are parallel.

When the slips of glass between which the iodine has been melted are carefully separated, the film usually remains attached to one of them in a sufficiently perfect condition to be examined. At first it is extremely brilliant, and shows exactly the same appearances as have already been described as occurring with film of iodine under glass. The surface, however, not only tarnishes rapidly, but even at a low temperature (10°) the film quickly evaporates; and consequently the uncovered films are somewhat difficult to examine.

They, however, permit some additional facts to be observed, which either are seen with difficulty or not seen at all when the iodine is covered with a plate of glass: and chief amongst these is the "surface-colour" which iodine shows when light is incident upon it at a high angle.

When a film of either iodine α or β is placed on the stage of the polariscope with its axis parallel with the plane of incidence and the principal section of the Nicol in the same plane, the surface of the iodine appears bright and metallic when light is incident on it at an angle of about 60°. As the angle of incidence increases, the colour of the reflected light changes; at about 70° the surface appears blue, and is still bright, but has lost its metallic appearance to a considerable extent, and at about 72° the colour is most intense; but as, in addition to the difficulties which are inseparable from determinations of this kind, the instrument which I have used for these experiments does not allow of any very accurate measurements being made with it, the value of these angles can only be regarded as approximate.

On rotating the stand the amount of reflected light diminishes rapidly, and the iodine appears dark or nearly so when the axes of the crystals are perpendicular to the plane of incidence.

On rotating the Nicol, the axes of the crystals of iodine remaining parallel with the plane of incidence, the surface of the iodine becomes bright and metallic, the maximum amount of light being transmitted when the principal section of the Nicol is perpendicular to the plane of incidence.

When the incident light is polarized in the plane of incidence the surface of the iodine appears brilliant and metallic in all positions, and when seen through the analyzer the amount of light reflected by the film alters as the former is rotated, but there is no trace of colour.

When, however, the light is polarized perpendicularly to the plane of incidence the reflection from the surface of the iodine is a coloured one

when the axes of the crystals are parallel with the plane of incidence; consequently the appearance of the film is exactly the same when unpolarized light falls on its surface and it is seen through a Nicol whose principal section is vertical, and when the incident light is polarized perpendicularly to the plane of incidence and it is seen directly.

These experiments show that when light falls upon the surface of a film of iodine at an angle of about 72° a portion of the light is polarized by reflection in the plane of incidence, and this independently of the position of the crystals composing the film, and that another portion of the light, which is coloured by reflection, and to which the surface-colour is due, is polarized in a plane whose direction depends on that of the crystals composing the film, and, further, that this light is polarized perpendicularly to the axis of the crystals.

The surface-colour can only be seen when the angle of incidence which the light makes with the surface of the iodine is a large one; and the reason that in the case of iodine covered with glass it is not visible, apparently is, that with a large angle of incidence nearly the whole of the light is reflected from the surface of the glass. I succeeded in seeing the blue colour in the case of a fragment of iodine which had been melted between a slip of glass and one of the sides of a small crown-glass prism of an equilateral section, and also when such a prism was placed on the surface of one of the glass slips covering the iodine, a drop of carbon tetrachloride (the index of refraction of this liquid being nearly the same as that of the glass) being placed between the slip and the prism, as under these circumstances light can reach the surface of the iodine at a greater angle than is possible when it is covered by a flat piece of glass; but in neither case was the surface-colour so well seen as when the iodine was uncovered.

Haidinger has remarked (Sitzungsberichte der kaiserlichen Akademie der Wissenschaften, Band viii. p. 97) that the surface-colours are complementary to the colour of the light transmitted by the same substance; and this also appears to be the case with iodine, as in the solid and liquid condition, and also when dissolved in certain liquids, it absorbs most readily the blue rays; but at the same time, as Professor Stokes has pointed out, the surface-colour and the colour of the transmitted light can only be said to be complementary within very narrow limits, as the colour of the transmitted light varies with the thickness of the layer of substance through which it passes.

Films of iodine α and β, when the light was incident on their surface at a considerable angle, were found to polarize the light elliptically in certain positions of the film, as was shown by the black cross being distorted when a plate of Iceland spar, cut perpendicularly to the axis of the crystal, and a plano-convex lens of about 40 mm. focal length were placed between the surface of the iodine and the analyzing Nicol. The amount of distortion, which was never very considerable, increased

with the angle of incidence, and appeared to attain its maximum when the angle was about 72°.

When a film of either iodine α or β is placed on the stage with its axis perpendicular to the plane of incidence, and the principal section of the analyzer parallel with the latter plane, the black cross is perfect; but on turning the film till its axis is parallel with the plane of incidence, the cross becomes slightly distorted, and the centre appears bluish. When the light falling on the surface of the film is polarized in a plane forming an angle of 45° with the plane of incidence, the principal section of the analyzing Nicol still remaining in that plane, the black cross is perfect as long as the axis of the iodine is perpendicular to the plane of incidence, and distorted when it is parallel.

Iodine β only shows the black cross very faintly with unpolarized light when its axis is perpendicular to the plane of incidence.

The distortion when the incident light is polarized is far greater than that produced by the reflection of a ray of light, polarized at an angle of 45 degrees with the plane of incidence, from the surface of a piece of glass, and quite comparable in amount with the effect produced when the light falls on a metallic surface.

I selected from a large quantity of freshly sublimed iodine a few pieces bearing a more regular crystalline form than the rest, and amongst these there were two nearly triangular plates, about 10 mm. long and 8 mm. broad.

One of these was arranged on the stage of the polariscope, and the light reflected from the surface observed in the way that has already been described in the case of the films of iodine.

The light was most completely polarized when the angle of incidence was about 72°; when the length of the crystal was perpendicular to the principal section of the analyzer it appeared darkest, and when parallel with it lightest.

The surface-colour and the distortion of the black cross were as clearly seen as with the film of iodine.

After several unsuccessful attempts I succeeded in preparing some well-defined crystals of iodine by carefully heating on a sand bath a small quantity of that substance in a wide-mouthed stoppered bottle, when some perfect rhomboidal plates of iodine about 1 mm. long were deposited on the cool part of the bottle. When these were examined with the polariscope, the principal section of the Nicol being in the plane of incidence, they appeared brightest when their long axis was parallel with, and darkest when it was perpendicular to, the plane of incidence.

Hence it appears that when the long axes of these crystals are parallel with or perpendicular to the plane of incidence, part of the light reflected from their surface is polarized in the plane of incidence, and part in a plane at right angles to their long axes, and consequently that the long

axes of these rhomboidal plates correspond with that direction within the film of iodine which has been spoken of as the axis of the crystal: and as iodine belongs to the trimetric system, this may be considered the principal axis, as being the one in the direction of which the crystals are prismatically developed to the greatest extent; and it also appears that when a ray of light passes normally through such a crystal it is divided into two rays polarized respectively parallel with and perpendicular to the same axis; and the one whose flame of polarization is parallel with the principal axis suffers the least absorption.

III. "Picrorocellin." By John Stenhouse, LL.D., F.R.S., and Charles Edward Groves. Received April 27, 1876.

Through the kindness of Mr. C. Lavers Smith, the eminent orchil manufacturer of Spitalfields, we were furnished with a quantity of a lichen which he had observed to have a very bitter taste, and which came into the market through a Portuguese house. It is believed to have been brought from the West Coast of Africa; but our endeavours to ascertain the exact locality have hitherto been unsuccessful. From the appearance of the lichen it seems to grow on limestone rocks; and Mr. W. Carruthers, of the British Museum, and the Rev. J. Y. Crombie, to whom we submitted it, pronounced it to be a variety of *Rocella fuciformis*, the ordinary *Rocella* usually growing on trees. This lichen is remarkable for its intensely bitter taste; and the preliminary experiments showed that this is due to the presence of a crystalline compound which is but slightly soluble in water.

Picrorocellin.

The lichen was accordingly first treated with water and hydrate of lime, in the usual manner, to extract the erythrin which it contains in common with other varieties of *Rocella*, and the residue, after being dried at the ordinary temperature, was extracted by boiling spirit. The alcoholic solution, which contained the bitter substance together with chlorophyl and various fatty and resinous impurities, was concentrated by distillation until almost the whole of the alcohol was removed. When cold, the dark-coloured pasty mass was pressed in a cloth, boiled up with a small quantity of strong spirit, and allowed to cool, pressed, and again treated in the same manner. By this means much of the chlorophyl and almost the whole of the oily matters were dissolved out, leaving a dark green-coloured crystalline product. In order to remove the last traces of chlorophyl from this, it was boiled up twice or thrice with benzine, in which the crystals are only slightly soluble.

The spent "weed" from this variety of lichen which has been exhausted with ammonia in the ordinary process of the orchil manufacture also yields the same crystalline substance, but it is much more difficult to purify

than that obtained from the lichen which has been exhausted with milk of lime. The erythrin in this lichen is identical with that from the ordinary kinds of *R. fuciformis*, yielding orcin and erythrite when boiled with lime or other alkalies.

The nearly colourless substance was now boiled for some time with about ten times its weight of spirit, and filtered whilst hot through a vacuum filter. In this operation it is necessary to use a hot-water funnel, as, otherwise, the crystals which separate soon choke up the filter. The filtered solution, which should be boiled until clear, on cooling, deposits a large amount of crystals, which evidently consist of a mixture of two substances, one forming lustrous prisms, the other feathery tufts of minute flattened needles. These two substances can be easily separated mechanically by elutriation, the comparatively large prisms of picrorocellin, from their size and weight, rapidly sinking to the bottom of the solution, whilst the light feathery tufts of the other compound remain suspended. One or two recrystallizations from spirit generally suffice to purify the former; but if they have any green tinge, it is necessary to treat them previously with boiling benzine in order to remove the trace of chlorophyl to which the colour is due.

The new compound crystallizes in long prismatic crystals of considerable lustre, which are moderately soluble in boiling spirit, slightly soluble in ether and benzine, but almost insoluble in water, petroleum, and carbon bisulphide. It melts at 192°–194° C., and when more strongly heated boils and gives off vapours of an oily substance of a pleasant aromatic odour, leaving a small amount of carbonaceous residue. Cold concentrated sulphuric acid colours it of a deep brown; but when it is warmed it dissolves, forming a pale yellow solution, from which water precipitates a yellow compound. If the sulphuric acid solution be heated nearly to its boiling-point, it darkens and gives off sulphurous anhydride; the addition of water then gives no precipitate. It is also soluble in warm nitric acid, and the addition of water causes a yellow precipitate similar in appearance to that produced in the sulphuric acid solution; the nitric acid solution, when heated, gives off nitrous fumes. If boiled with dilute sulphuric or hydrochloric acid for a short time, the picrorocellin is decomposed, a compound, *xanthorocellin*, being formed which crystallizes from alcohol in long silky needles of a pale yellow colour. When picrorocellin is distilled with a mixture of dilute sulphuric acid and potassium dichromate, it is decomposed, an oil passing over having the odour of benzoic aldehyde, accompanied by a white crystalline substance. The latter, after being purified by recrystallization, melted at 121° and had the general appearance and properties of benzoic acid, with which it is identical. It was also found to be oxidized by an acid solution of potassium permanganate, but no odour of bitter-almond oil was observable.

The substance dried at 100° was submitted to analysis, with the following results:—

I. ·263 gram substance gave ·657 gram carbonic anhydride and ·151 gram water.

II. ·178 gram substance gave ·444 gram carbonic anhydride and ·100 gram water.

III. ·218 gram substance gave ·02285 gram ammonia.

IV. ·215 gram substance gave ·02219 gram ammonia.

		Theory.	I.	II.	III.	IV.	Mean.
C_{27} ..	324	68·21	68·13	68·03	..	..	68·08
H_{29} ..	29	6·11	6·38	6·24	..	..	6·31
N_3 ..	42	8·84	..	..	8·63	8·50	8·56
O_5 ..	80	16·84					
	475	100·00					

The numbers obtained from the results of these analyses agree very closely with those required by the formula $C_{27}H_{29}N_3O_5$. As this compound is obtained from a species of *Rocella* and possesses an exceedingly bitter taste, we purpose calling it *picrorocellin.* It is remarkable as being the first crystalline organic substance containing nitrogen which has been found in the lichens.

Occasionally specimens of *Rocella* are met with which have a comparatively feeble bitter taste; but hitherto we have been unable to isolate any crystalline compound from them to which this property might be ascribed.

We have not, as yet, examined the second substance crystallizing in minute, difficulty soluble needles, and which accompanies picrorocellin in this lichen, but hope to be able to do so ere long. We have ascertained, however, that it is not merely a fatty substance of a nature similar to rocellic acid, since on boiling it with a dilute solution of sodic hydrate, benzoic aldehyde appears to be produced.

Xanthorocellin.

When picrorocellin was heated above its fusing-point it decomposed, the products obtained varying with the temperature. When strongly heated, water and ammonia were given off, and a brown oily body distilled, which on being put aside for some time deposited crystals. These, after being separated from the fluid portion and recrystallized two or three times from spirit, formed colourless plates which are moderately soluble in alcohol but insoluble in water. This substance has been reserved for further investigation.

If, however, instead of subjecting the picrorocellin to destructive distillation it was merely heated for about ten minutes to 220°, it gave off water, and the fluid product if poured out and allowed to cool solidified to a resinous-looking mass. This, when finely powdered and dissolved in about three times its weight of boiling spirit, solidified on cooling to a pulp consisting of long, slender, yellow needles of xanthorocellin. This sub-

stance may be more conveniently prepared, however, by the action of dilute acids on picrorocellin, the proportions which gave the most satisfactory results being 1 part of picrorocellin to 4 of strong hydrochloric acid and 4 of water. The mixture was boiled for about 8 hours in a flask furnished with a return condenser, and the product, which still contained some unaltered picrorocellin, was collected, washed, and dissolved in boiling spirit. On cooling, the xanthorocellin was deposited in slender needles of a pale yellow colour, which amounted to about 70 per cent. of the picrorocellin originally employed. They were easily purified by two or three crystallizations from boiling spirit. When dilute sulphuric acid was substituted for the hydrochloric acid, the results obtained were not so good, oily impurities seeming to be formed at the same time. The compound as prepared by this process is identical with that obtained when picrorocellin is subjected to a temperature of 220° in the manner previously described, both substances melting at 183°.

Although xanthorocellin could be prepared with tolerable facility by the action of dilute hydrochloric acid on picrorocellin, yet it had the disadvantage that a portion of the latter always remained unattacked; this was due, no doubt, to its insolubility in the dilute acid. It seemed possible that this inconvenience might be obviated by employing an alcoholic solution of picrorocellin. On making the experiment, it was found that when an alcoholic solution of the substance was acidulated with hydrochloric acid and boiled, although xanthorocellin was readily formed, yet in order to recover it, it was necessary to partly distil off the spirit and then precipitate with water; the product was in this case accompanied with more or less of a tarry matter. These difficulties, however, were overcome by the use of glacial acetic acid. Picrorocellin dissolved readily in the boiling acid, and if sufficiently concentrated crystallized out again unchanged on cooling. On adding a drop of hydrochloric acid to the hot colourless solution and again boiling it, it almost instantly became of a yellow colour, and now no longer deposited crystals of picrorocellin when cooled. The addition of water to this solution immediately produced a precipitate of xanthorocellin.

After numerous trials, the following was found to be the best method of preparing xanthorocellin:—10 grams of picrorocellin are dissolved in 15 grams of boiling glacial acetic acid, 6 drops of concentrated hydrochloric acid are added and the whole boiled for 15 minutes in a flask furnished with an inverted condenser. On allowing it to stand for some time after it has become cold, it solidifies to a mass of crystals of the xanthorocellin. These are stirred up with water, thoroughly washed to remove adhering acid, and then recrystallized from spirit. The yield in this case was found to be 76 per cent. of the picrorocellin employed, and the alcoholic mother liquors when evaporated left neither tarry residue nor undecomposed picrorocellin. When dilute sulphuric acid was substituted for the hydrochloric acid in this experiment, the result was very

similar to that observed with the aqueous acid; xanthorocellin was formed, but the yield was much smaller, only 56 per cent., owing, doubtless, to secondary decomposition; this supposition was corroborated by the fact that the addition of water to the glacial acetic acid solution precipitated the crude xanthorocellin as a yellow oil, which only crystallized after it had been standing for some short time. This substance is insoluble in petroleum, and but slightly soluble in hot carbon bisulphide or in ether. It is moderately soluble in hot benzine, and readily soluble in boiling spirit.

The substance dried at 100° gave the following results:—

I. ·247 gram substance gave ·692 gram carbonic anhydride and ·115 gram of water.

II. ·350 gram substance gave ·979 gram carbonic anhydride and ·171 gram of water.

III. ·160 gram substance gave ·450 gram carbonic anhydride and ·079 gram of water.

IV. ·261 gram substance gave ·02742 gram ammonia.

V. ·240 gram substance gave ·02415 gram ammonia.

		Theory.	I.	II.	III.	IV.	V.
C_{21} ..	252	76·60	76·44	76·28	76·71		
H_{17} ..	17	5·17	5·17	5·43	5·49		
N_2 ..	28	8·51	..	..	..	8·65	8·29
O_2 ..	32	9·72					
	329	100·00					

These numbers correspond pretty closely with those required by the formula $C_{21}H_{17}N_2O_2$.

When xanthorocellin was boiled for some time with a moderately strong aqueous solution of sodic hydrate, it acquired a yellow colour, but did not appear to dissolve to any great extent. On collecting this insoluble compound and washing it, first with strong caustic soda and then with a saturated solution of sodium carbonate, a bright yellow powder was left. This yellow compound, when treated with a small quantity of spirit, to which a few drops of sodium hydrate solution had been added, dissolved only partially, leaving a residue consisting of colourless crystals. These were readily soluble in water; and on adding an acid to the solution, a white precipitate was obtained. The addition of a concentrated solution of sodium hydrate to the clear yellow alcoholic solution immediately produced a yellow precipitate. It was observed that xanthorocellin dissolved in boiling water to which a *few drops* of a solution of sodium hydrate had been added, and on cooling it crystallized out again apparently unaltered. If, however, instead of allowing it to cool, strong soda was added to the hot clear solution, a bright yellow precipitate was produced. A similar precipitate was obtained on heating xanthorocellin with spirit containing a little sodium hydrate in solution,

pouring off the clear liquid from the colourless crystals which were formed, and adding excess of soda.

Xanthorocellin is soluble in warm concentrated sulphuric acid with a brilliant orange-colour; but on adding water, the substance is precipitated unaltered. If the acid solution be strongly heated, however, it effervesces slightly, and becomes somewhat darker in colour: the addition of water to this no longer produces a precipitate.

Xanthorocellin dissolves in cold nitric acid, and if water be at once added the substance is precipitated apparently unaltered. On allowing the nitric acid solution to stand for some time, however, or on gently heating the mixture, decomposition takes place with the formation of new compounds. These may be more conveniently obtained, however, by the action of nitric acid on the substance dissolved in glacial acetic acid, in the following manner:—5 grams of xanthorocellin were dissolved in 10 c. c. of boiling acetic acid, and the solution rapidly cooled, but without agitation, so as to avoid as much as possible causing the substance to crystallize out; 5 c. c. of nitric acid (specific gravity 1·45) were then added, the whole thoroughly mixed and gently heated in a water bath until the action had set in. When this took place, the source of heat was withdrawn, the heat developed by the reaction being sufficient to cause the liquid to boil; large quantities of nitrous fumes were given off, and lustrous scales soon began to appear in the liquid. If, as sometimes happened, the action became too violent and threatened to eject the contents from the flask, it was easily moderated by plunging the latter into cold water for a few seconds. As soon as the reaction was complete, the contents of the flask were poured into a beaker, and put aside for a few hours; the solid crystalline mass thus obtained was then thoroughly incorporated with 30 c. c. of spirit, which dissolved nearly every thing except the scales. The latter were collected on a Bunsen filter, and washed thoroughly with cold spirit, in which they are almost insoluble. The yield of the new substance is about 35 per cent. of the xanthorocellin originally taken; it crystallizes in beautifully white hexagonal plates, which do not melt at 275°, but begin to undergo decomposition below that temperature. The spirituous washings from these crystals, obtained when the crude product was treated with alcohol, were allowed to evaporate spontaneously, until the latter had disappeared. On distilling the residue in a current of steam, some benzoic aldehyde passed over with the aqueous vapour, but no hydrocyanic acid could be detected in the distillate. The crystalline substance left in the retort appears to be a nitro-acid of high melting-point, totally different in its properties, however, from nitrobenzoic acid. We hope to be able, at some future time, to examine the products of this interesting reaction more fully.

Action of sodium hydrate on picrorocellin.

When picrorocellin was boiled with three times its weight of spirit, and

an aqueous solution of sodium hydrate was gradually added until all the crystals had disappeared, a solution was obtained which, when filtered and rendered acid with acetic acid, deposited crystals of very pure unaltered picrorocellin equal in weight to about half those originally taken.

The alcoholic mother liquors on being evaporated left an oily residue insoluble in water. It was exceedingly soluble in hot spirit, however; and the solution, if sufficiently concentrated, deposited colourless crystals of a new substance mixed with more or less picrorocellin.

As from this experiment picrorocellin seemed to be altered by the action of alkalis, a quantity of it was boiled with a concentrated solution of sodium hydrate, when it became evident that it was rapidly being changed, and in a few minutes it became red, and fused to an oily mass enclosing crystals of the unaltered substance. The whole was then allowed to cool; and after the soda solution had been poured off, the red mass was gently heated with water, which readily dissolved most of it, leaving merely the unattacked picrorocellin. The addition of sodium hydrate to the deep yellow solution thus obtained threw down a yellowish-red resinous mass, whilst acids caused the formation of a voluminous, almost white precipitate. This readily aggregated to a plastic mass, which was washed by kneading it in tepid water. During the action of caustic soda on picrorocellin, a volatile substance is produced having a pleasant aromatic odour.

It was found, however, that the principal product of this reaction could be obtained far more readily, and in a purer state, by operating with dilute solutions of sodium hydrate. For this purpose 3 parts of solid sodium hydrate were dissolved in 180 to 200 of boiling water, and then 10 parts of finely powdered picrorocellin were added; the crystals dissolved rapidly in the boiling liquid, ammonia was given off in small quantity, and after about an hour's digestion the reaction was considered to be complete. When nearly cold, acetic acid was added to the solution in slight excess; this produced a glutinous kind of precipitate, which could be moulded under the warm liquid into a stick closely resembling bleached shellac in appearance, being lustrous and silky. The product was found, however, to be far from pure; and in order to obtain it in that state, it had to be crystallized successively from alcohol and from carbon bisulphide. The crude product was therefore digested with two thirds of its weight of boiling spirit until dissolved, and then set aside to cool. After standing a considerable time, it deposited the new substance in colourless crystals, which were collected on cotton wool on a vacuum filter, washed with *a very small quantity* of cold spirit, and dried. These crystals were then boiled with 30 times their weight of carbon bisulphide until dissolved, the solution filtered, and then concentrated to about half its bulk by distillation. On cooling, the substance crystallized out in large, brilliant, colourless prisms, amounting to from 40 to 45 per cent. of the weight of the picrorocellin originally taken. These may be rendered

quite pure by crystallizing them first from a mixture of equal weights of alcohol and water (10 parts) and finally from strong spirit.

Tho crystals, whether prepared by boiling picrorocellin with an aqueous or with an alcoholic solution of sodium hydrate, are identical, both melting at 154° C. The fused compound remains quite liquid even when cold; but on adding a crystalline fragment of the substance it instantly solidifies. Dried at 100° C. and submitted to analysis it gave the following results:—

I. ·287 gram of substance gave ·778 gram carbonic anhydride and ·175 gram of water.

II. ·193 gram of substance gave ·523 gram carbonic anhydride and ·113 gram of water.

III. ·280 gram of substance gave ·760 gram carbonic anhydride and ·169 gram of water.

IV. ·368 gram of substance gave ·03264 gram ammonia.

V. ·297 gram of substance gave ·02611 gram ammonia.

VI. ·243 gram of substance gave ·02089 gram ammonia.

		Theory.	I.	II.	III.	IV.	V.	VI.
C_{24} ..	288	74·04	73·93	73·91	74·03			
H_{25} ..	25	6·43	6·78	6·51	6·71			
N_2 ..	28	7·20	..	..	..	7·30	7·24	7·08
O_3 ..	48	12·33						
	389	100·00						

These numbers correspond very closely with the formula $C_{24}H_{25}N_2O_3$. This compound is almost insoluble in petroleum, and only very slightly soluble in ether, moderately so in boiling benzine. When strongly heated, it fuses and becomes deep yellow, being converted into xanthorocellin. Nitric acid oxidizes it, benzoic aldehyde being first produced, which, by a continuance of the action, is converted into benzoic acid. A similar result is obtained when it is treated with chromic-acid mixture. The crystals dissolve in concentrated sulphuric acid by the aid of a gentle heat, and on adding water a precipitate of xanthorocellin is obtained.

As picrorocellin possessed such an extremely bitter taste, and was, moreover, a nitrogenous compound, it seemed not improbable that it might possess medicinal properties. It was therefore submitted to our friend Dr. T. Lauder Brunton, F.R.S., who kindly undertook its examination with especial reference to the possible resemblance between its actions and those of quinine. He says, "The results may be briefly stated as follows:—

"1. The substance when injected under the skin of an animal exerted no perceptible action whatever.

"2. When injected under the skin of a frog it diminished reflex action. This diminution appeared to be caused in the same way as that

effected by quinine, namely by irritation of the inhibitory centres within the head. As the whole subject of the action of quinine on the functions of the spinal cord is at present unsettled, too much stress must not be laid on this action of picrorocellin.

"3. When an ethereal solution of picrorocellin is added to an alkaline solution of sulphate of indigo, with blood and ozonized turpentine, in the manner recommended by Binz and employed by him in his researches on quinine, the production of isatin is not in the least retarded, whilst quinine retards it very greatly. Picrorocellin thus differs from quinine in not arresting oxidation, a most remarkable characteristic of the latter.

"I regret that I have been unable to test it chemically in a case of ague, as the patients I see come and go so irregularly that little or no information would be gained by administering it to them. The sparing solubility of picrorocellin is a serious objection to its use in medicine, even supposing it to have the same properties as quinine; and as it does not possess one of the most important of these properties, there is no probability that it can ever be used as a substitute for quinine."

IV. "On the Organization of the Fossil Plants of the Coal-measures.—Part VIII. Ferns (continued) and Gymnospermous Stems and Seeds." By Prof. W. C. Williamson, F.R.S., Professor of Natural History, Owens College, Manchester. Received May 2, 1876.

(Abstract.)

Ferns (continued).—Under the name of *Rachiopteris corrugata* a small *stem* of a fern is described, the outer surface of the bark of which is corrugated with innumerable transverse ridges and furrows. It has a vascular axis in its centre composed of several clusters of barred vessels filled with tylose, which clusters are blended together at their periphery, forming a cylinder; its centre is occupied by a cellular medulla, mingled with small vessels, which sends off radiating prolongations into the vascular cylinder, partially separating the bundles of the latter. Besides this cylinder is a second, smaller, isolated oval bundle, which soon escapes from the stem as the centre of a petiole; but before it does so a new one is detached from the opposite side of the central cylinder, which, in turn, imitates its predecessor. Besides these primary bundles, numerous secondary smaller ones are detached, sometimes from the central cylinder, sometimes from near the bases of the petiolar bundles; these probably supplied rootlets. The author points out that this fern, along with the *Anachoropteris Decaisnii* and the *Zygopteris Brongniarti* of M. Renault, constitute a group of ferns having a very distinct type of stem-structure different from what is found in the rhizomes of recent ferns, and which approximates to the lower Lepidodendroid stems as represented by *L. Harcourtii*.

Two kinds of sporangia of ferns are described. One of these has a perfectly vertical annulus, such as is common amongst the Polypodiaceæ. A second has a large, horizontal, subterminal annulus, approaching closely to the form seen in the recent Gleicheniaceæ and Schizæaceæ, especially resembling the latter type. Both these sporangia contained spores; in the first mentioned these were numerous and small; in the latter they are fewer in number, but of larger dimensions. The Gymnospermous stems of the Coal-measures are next examined. The small branch of *Dadoxylon* from Coalbrook Dale, described by the author many years ago in the Transactions of the Manchester Literary and Philosophical Society, is first restudied. Its pith is Sternbergian; its ligneous zone has a medullary sheath of barred vessels, whilst its woody zone is composed of wedges of discigerous fibres arranged exogenously and separated by mural medullary rays. The disks of the fibres lack the central perforations seen in those of recent conifers. The bark is exactly like that of a young shoot of a *Taxus*, consisting of an inner liber, the tissues of which are arranged compactly in lines running parallel to each other and to the surface of the wood; whilst the outer layer consists of large parenchymatous cells, which in the living plant doubtless contained chlorophyl. It appears to correspond to the phelloderm, no true phellein layer being present. Other branches, especially from the Ganister beds near Oldham and Halifax, are also described. Many of these are of much larger size, but all have Sternbergian piths, with the exception of one in which the parenchymatous medulla is not disciform, but like that of living conifers. The chief peculiarity in the majority of these latter fossil branches and twigs is that they give off small twin vascular bundles from the innermost surface of the ligneous cylinder. These pass outwards side by side through the smaller branches, but can only be traced in the innermost portions of the larger ones; hence it is probable that they either supplied leaves arranged in pairs (not distichously), or that they went to a binerved leaf, the latter being most likely to have been their real destination. The bark is rarely preserved in these larger specimens from the Ganister ironstones, in which they are associated with myriads of *Goniatites*, an indication that they have been drifted from a distance and long exposed to water—conditions very different from those characterizing the origin of the coal in which most of the Oldham plants have been obtained.

The author discusses the claim set up by M. Brongniart and Professor Newberry for the admission of *Sigillaria* amongst the Gymnospermous exogens, as well as Dr. Dawson's opinion that some of them, at least, have decided Gymnospermous affinities; but still believes that this determination is not justified by the facts. All the additional observations which he has made since the publication of his second and third memoirs confirm his original conclusion that no true distinction can be demonstrated to exist between the *Sigillariæ* and the higher forms of *Lepidodendra*, in

which the vascular cylinder assumes the exogenous Diploxyloid organization. All the plants of which stems and branches have been found displaying an organization corresponding to that of living Gymnosperms are still comprehended within Endlicher's genus *Dadoxylon*. On the other hand, recognizing in *Trigonocarpum* all the external features of a true seed, the author cannot admit the probability of its having belonged to the Lycopodiaceous *Sigillariæ*.

Gymnospermous Seeds.—Attention is next directed to the curious seeds discovered in America, and published in Professor Newberry's 'Geological Survey of Ohio.' These, however, merely display external forms. Still more remarkable is the collection of such seeds found by M. Grand-Eury at St. Etienne in France. These exhibit their internal structure in a wonderful manner, as is shown by M. Brongniart's brief memoir published in the 'Annales des Sciences Naturelles.' M. Brongniart called attention, in that memoir, to a remarkable organization of the micropylar extremity of many of these seeds, where a peculiar cavity existed, between the micropyle and the apex of the nucleus, into which the pollen-grains obtained entrance through the micropyle, and were thus brought into contact with the nucleus. In a more recent memoir on the fertilization of the ovules of some species of recent Cycads (*Ceratozamiæ*), M. Brongniart showed that a mammillar prolongation of the apex of the nucleus projected into the micropyle, which it filled; but that during fertilization the cells of this prolongation became disorganized, and a cavity was produced into which the pollen-grains found their way, the apex of the nucleus below this cavity becoming covered over by true perispermic membrane. These structural peculiarities so far accord with what he observed in M. Grand-Eury's seeds, as to lead him to surmise that the latter had Cycadean rather than Coniferous affinities.

The author has found a number of remarkable seeds of a similar type to those from St. Etienne in the Oldham nodules, and he has been indebted to his friends Mr. Butterworth and Mr. Nield, of Oldham, and to Captain Aitken, of Bacup, for a few others.

The first of these is a very small, nearly spherical seed, which the author names *Lagenostoma ovoides*, about ·16 of an inch in length and ·1 in breadth. It has a solid testa, within which can be recognized two distinct membranes—an inner or "perispermic" one, which has enclosed the endosperm, and an outer or "nucular" one, which has been in close contact with the perispermic one throughout the greater part of the seed, but which splits up at its apex into two portions, the inner one of which forms a remarkable flask-shaped cavity, which the author designates the lagenostome. Its base has rested upon the apex of the perisperm, and its upper extremity has been continuous with the micropyle. Within this lagenostome is a little delicate parenchyma, which has shrunk up towards the centre of the cavity, leaving a surrounding space in which, in some examples, the author has found the objects regarded by

M. Brongniart as pollen-grains—an opinion in which the author concurs. External to the lagenostome the second or outer division of the nucular membrane forms a remarkable "canopy," which hangs down from the micropyle, enclosing the lagenostome within ten sharply defined and regular crescentic folds, the concavities of which are directed outwards. The walls of this lagenostome and of the "canopy" correspond with the nucular membrane in consisting of flattened prosenchymatous cells. The perispermic membrane, on the other hand, looks structureless, save that it appears to have had imbedded in it an innumerable multitude of minute crystals, like those observed by Dr. Hooker on the spicular cells of *Welwitschia.*

A second species the author designates *Lagenostoma physoides.* In this the apex of the endospermic sac contracts into a mammilliform prolongation, overlapped by the base of the lagenostome, which overhangs it as a bladder half-full of water might be made to overhang the neck of a soda-water bottle upon which it rested. This species has other distinctive structural peculiarities.

For a second genus of new seeds the author proposes the name of *Conostoma. C. oblonga* from Oldham is about ·18 of an inch in length. Here, again, we have an endosperm enclosed in a perispermic membrane, and this in turn is encased within a nucular one, the whole being invested by a dense testa. The lagenostome is again formed out of divisions of the apical part of the nucular membrane; but it assumes a funnel-shape at its base, whilst its upper extremity is continuous with the micropyle. A second species, named *C. ovalis*, is from the Burntisland deposit, and is more ovate than *C. oblonga.* In it the lagenostome assumes a remarkably funnel-shaped contour. The same deposit has furnished a third species, *C. intermedia.* To another remarkable seed from Oldham the author gives the name of *Malacotesta oblonga*, of which the maximum length, exclusive of its funiculus, has been about ·25. Its exotesta has been soft and parenchymatous, with a prosenchymatous inner (nucular?) membrane. The micropyle has been remarkably wide with incurved margins at the exostome, and enclosing a mass of delicate parenchyma through which a canal passed.

The author has obtained a fine series both of longitudinal and transverse sections of *Trigonocarpum olivæforme*, the seed long ago made the subject of a valuable memoir by Dr. Hooker and Mr. Binney. So far as the longitudinal sections are concerned, the results obtained correspond closely with those already arrived at by these two authors, except that a modified form of lagenostome is shown to have existed at the apex of the nucleus. The transverse sections show that the two layers of the testa, an outer soft parenchymatous exotesta and an inner sclerotesta, present some striking features. The exterior of the latter has exhibited three principal, acute, prominent, longitudinal ridges, between each two of which are three intermediate ones, the centre of these three being

rounded, and the two flanking ones acute. The internal cavity of the endotesta is prolonged like a narrow fissure only into each of the three principal ridges. The ordinary sandstone specimens of *Trigonocarpum olivæforme* commonly seen in cabinets do *not* represent, as has hitherto been supposed, the *exterior* of these seeds, but are casts of the *interior* of the sclerenchymatous endotesta, the three thin, longitudinal, wing-like appendages being merely casts of the three slit-like extensions of that interior just referred to. These slits extend upwards into the prolonged micropyle, the interior of which displays a triangular section, each of the sides of which is convex, the convexity projecting inwards.

The nomenclature of this type of seed is in great confusion, owing to specific differences being based on mere differences of size, many of which are probably nothing more than varieties due to age and development.

Casts of seeds with six longitudinal wings are described, corresponding with Brongniart's genus *Hexapterospermum.* They are more oblong than *Trigonocarpum olivæforme*, but apparently identical with the *T. Nöggerathi* of the 'Fossil Flora.' The author doubts the wisdom of Brongniart's establishment of a separate genus for these seeds.

Several species of the important genus *Cardiocarpum* have been obtained displaying the internal organization of these remarkable seeds. They all agree in possessing a central endosperm which is remarkable for the very large size of its conspicuous parenchymatous cells. This is invested by a perispermic membrane, the whole being enclosed within a testa composed of two very distinct and separate layers. A thin inner one, which may be identical with the nucular membrane of other seeds, is entirely composed of delicate prosenchymatous cells, and is prolonged into an elongated micropyle, into which the endosperm is not prolonged. Externally to this is an exotesta composed of a denser parenchyma. In some species this latter tissue is uniform throughout, in others it is separable into a dense endotesta and a more lax parenchymatous exotesta. The first species described is apparently identical with the *C. anomalum* of Carruthers, and has a trigonous endosperm invested by the two layers of testa(?), both of which are prolonged into a slender tapering beak, half the entire length of the seed, and which contains the elongated micropyle. Another species, designated *C. compressum*, has its apparent testa composed (as just described) of two continuous layers. In it the micropyle is comparatively short, and its apical extremity is patulous or trumpet-shaped. To a third very beautiful little cordato-lanceolate species with a peduncle or funiculus equal in length to the seed, the author gives the name of *Cardiocarpum Butterworthii*, after its discoverer. These seeds exhibit no specialized organ corresponding to the lagenostome of *Lagenostoma* and other seeds described. The pollen has passed down the long narrow micropyle into the triangular space at its inner extremity, where it came into direct contact with the endospermic membrane. It thus appears that the seeds known by the name of *Cardiocarpum* have a very

simple organization, approximating somewhat closely to that of the ovules of *Juniperus*, *Callitris*, and *Welwitschia.*

Some small seeds, which appear to be identical with the *Cardiocarpum tenellum* of Dawson, found in great numbers on slabs of shale by Mr. John Smith, of Kilwinning, in Ayrshire, are described. They were found in the upper Coal-measures near Stonehouse in Lanarkshire.

The last form noticed is a very curious winged seed from the uppermost Coal-measures of Ardwick, at Manchester, and which appears to have been a double seed, resembling in general form the samara of an ash. It belongs to Brongniart's genus *Polypterospermum.*

The fact that large numbers of seeds of unmistakable flowering plants exhibit very close resemblance to the *ovules* of Gymnospermous seeds is a very important one. Prof. Newberry has obtained such seeds in America; M. Grand-Eury has done the same thing in France; and it now appears that, though attention has but very recently been drawn to the existence of the smaller forms now described in the British Coal-measures, the discovery of a considerable variety has already rewarded the researches of the author and his auxiliary friends. There is no doubt that further research will materially increase that number. The question naturally arises, where are the Gymnospermous plants to which these seeds belonged? Finding the latter in the thin "upper-foot" coal-seam suggests that other remains of their parent stems should also be found there. The Dadoxylons are the only ones which exhibit any probability of such relationship. But these have chiefly been found in the marine Ganister bed, which underlies the upper-foot coal from which the majority of the seeds have been derived, indicating that the Dadoxylons grew apart from the Calamites and Lycopods abounding in the coal side by side with the seeds. Time alone can solve these problems, as well as others relating to the true homologies of some of the structures contained within these seeds.

V. "On Stratified Discharges.—II. Observations with a Revolving Mirror." By William Spottiswoode, M.A., Treas. R.S. Received May 3, 1876.

In a paper published in Poggendorff's 'Annalen,' Jubelband, p. 32, A. Wüllner has described a series of observations made, by means of a revolving mirror, upon the discharge of a large induction-coil through tubes containing ordinary atmospheric air at various degrees of pressure. When, as is generally the case with an induction-spark, the discharge occupies an appreciable interval of time, the image in the mirror appears spread out to a breadth proportional to the duration and to the velocity of rotation. The successive phases of the phenomena then appear, as usual, arranged in successive positions, and may be studied separately, even when too rapid to be disentangled by the unassisted eye.

Wüllner's observations appear to have been directed rather to the nature of the coil-discharge than to that of the stratifications; and some of his principal conclusions are accordingly of the following kind, viz. that at low pressures, *i. e.* down to 1 millim., when the discharge was stratified, the striæ showed an intermittence of intensity indicating either a pulsation within the duration of the main discharge or a breaking up of the main into a series of partial discharges. At greater pressures, *e. g.* at 26 millims., when almost all trace of stratification was lost, this breaking up into partial discharges (especially at the commencement) was distinctly perceptible. At yet greater pressures, *i. e.* from 40 millims. to 75 millims., a cloudy kind of stratification showed itself; but, excepting a bright flash at the outset, no appearance of partial discharge was visible. The observations, which were at first directed to capillary tubes, were extended to tubes of various diameters, and also included the effect of a magnet on the discharge.

For some time prior to the publication of the volume in question I had been engaged upon a series of experiments very similar in their general disposition, but with a somewhat different object in view, viz. the character and behaviour of the striæ; and of these, together with some recent additions, I now propose to offer a short account to the Society.

My general instrumental arrangements appear to have been similar to those of Wüllner; in fact they could hardly have been very different. The tubes were attached to the coil in the usual way, and a contact-breaker of the ordinary form with its own electromagnet was in the first instance used. By suitably adjusting the velocity of the mirror to the rapidity of the contact-breaker the image could be kept tolerably steady in the field of view. In order to obtain greater steadiness a special contact-breaker was next devised. This was mechanically connected with the spindle of the mirror, and so arranged as to break the current when the image was in the centre of the field of view. The only point in this part of the apparatus which requires special notice is the fact that this contact-breaker, like all others, should be placed in close proximity to the condenser of the coil, otherwise a great loss of light is sustained. For the last-mentioned form there was finally substituted a mercurial break (successfully arranged by my assistant, Mr. Ward), the plunger of which works on a cam attached to the axle of the mirror; so that the action of the contact-breaker is regulated by that of the mirror, instead of the reverse as in the former arrangement. With the broader tubes a slit was used; with the narrower this adjunct was less necessary; while with capillary tubes, such as are used for spectrum-analysis, it could be dispensed with altogether.

In experiments for comparing the unstratified statical discharge with the stratified at the same pressure of gas within the tube, and for observing the transition from one to the other, a Leyden jar and a spark

of air, the length of which could be regulated at pleasure, were introduced into the secondary circuit.

Striæ as observed by the eye have been divided into two classes, viz. the flake-like and the flocculent or cloudy. Of the former those produced in hydrogen-tubes may be taken as a type; of the latter those produced in carbonic-acid tubes. But upon examining some tubes especially selected for the purpose, it was found that, while to this apparent difference a real difference corresponds, a fundamental feature of the striæ underlying both was brought out.

The feature in question was this: that the striæ, at whatever points produced, appear to have generally during the period of their existence a motion along the tube in a direction from the negative towards the positive terminal. This motion, which I have called, for convenience, the proper motion of the striæ, is for given circumstances of tube and current generally uniform; and its variations in velocity are at all times confined within very narrow limits. The proper motion in this sense appertains, strictly speaking, to the flake-like striæ only. The apparent proper motion of the flocculent striæ is, on the contrary, variable not only in velocity, but also in direction; and on further examination it turns out that the flocculent striæ are themselves compounded of the flake-like, which latter I have on that account called elementary striæ.

Elementary striæ are in general produced at regular intervals along the tube. The series extends from the positive terminal in the direction of the negative to a distance depending upon the actual circumstances of the tube and current. The length of the column, and consequently the number of the striæ, depends mainly upon the resistance of the tube, the duration of the entire current, and, to a certain extent, upon the amount of the battery-surface exposed; and in that sense upon the strength of the current. The velocity of the proper motion, other circumstances being the same, depends upon the number of cells employed; in other words, upon the electromotive force.

The appearances of the striæ, however, their essential features, and the conclusions which may be drawn from them will be better apprehended by means of sketches, even though imperfect, than by mere description; and I therefore subjoin a few examples.

Fig. 1 represents the appearance of (in the mirror) a carbonic-acid tube with the slit attached. This tube, viewed by the eye, shows flake-like fluttering striæ, with a slight tendency to flocculency near the head of the column. The commencement of the discharge is at the right hand, and the negative terminal at the top. The drawing fairly represents the appearance of the upper part or head of the column of striæ during one complete coil-discharge. When the battery-surface exposed is small, the whole consists of, first, three or four columns of striæ of decreasing length, and afterwards of an almost unbroken field of striæ. Each of the initial columns is perfectly stratified; and the same disposi-

tion of striæ prevails throughout the entire discharge. The striæ which fill the main part of the field present a proper motion nearly uniform, but slightly diminishing towards the end. These striæ are for the most part unbroken, but are occasionally interrupted at apparently irregular intervals. When the battery-surface is increased the elementary striæ are more broken, and near the head of the column the interruptions occur as in the figure. The separation of the earlier part of the discharged into striated columns divided by intervening rifts does not, with the exception of the first, extend far towards the positive terminal. Nevertheless, even as far as the positive terminal itself, there seems at times to be a fuller development of discharge than is subsequently maintained.

Fig. 1.

The first rift in the discharge, following the first outburst, is sometimes distinguishable even as far as the positive terminal; and perhaps in those cases indicates a real cessation of the discharge. This is corroborated by the fact that a similar interruption is then perceptible in the glow surrounding the negative terminal; but after this the negative

glow retains its unbroken character throughout the entire period of the discharge.

The stratified columns with their intervening rifts are sometimes reproduced towards the close of the discharge; but this appears to take place only when the battery is in an unusual condition of energy, and disappears when, as in the bichromate battery, polarization of the plates rapidly takes place. On these occasions especially, but also at other times, traces may be seen of the faint lines of light connecting the positive with the negative parts of the discharge mentioned by Wüllner in the paper quoted above.

Other tubes, when viewed by the eye, show flaky striæ more or less difficult to distinguish from one another. Observed in the mirror, they show much the same phenomena as the tube figured above, except that the striæ are rather more crowded together and slightly more broken. This is the case especially with ammonia-tubes, in some of which the striæ are undistinguishable by the eye, and which accordingly give the impression of an unstratified column of light.

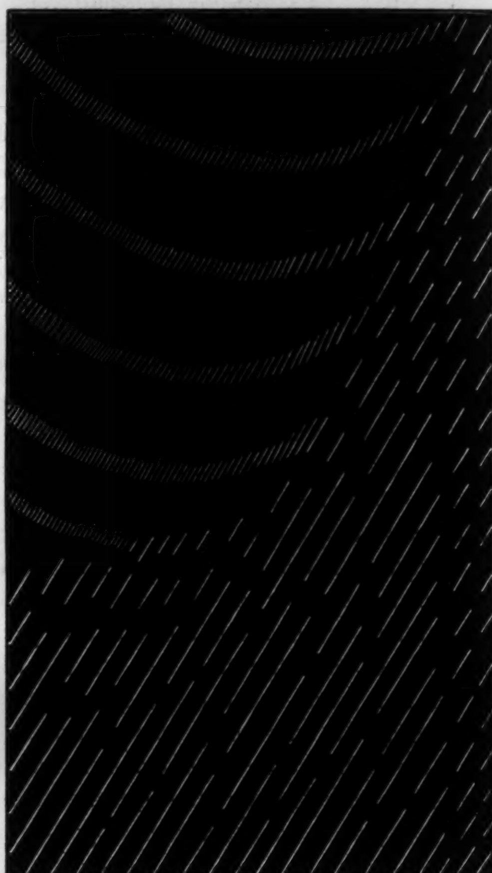
Fig. 2 represents the appearance in the mirror of another carbonic-acid tube with a current similar to that used in the former case. Viewed by the eye it shows flocculent striæ, each having a contour sharply defined towards the negative terminal, loosely defined towards the positive. The following description of the phenomena, taken from my earlier notes, may now be regarded as a description of the apparent proper motion of the flocculent as distinguished from the elementary striæ:—"The discharge opens with a considerable rush, indicated by the bright line at the commencement. There is no other indication of partial discharges. Proper motion at first towards the negative, afterwards towards the positive terminal. In this, as in other tubes giving striæ of this kind, ripples may be observed on the curve of proper motion."

So far my older notes; but on closer examination, and when the battery-surface exposed is sufficiently reduced, the entire field is seen to be traversed by elementary striæ having a normal proper motion. When the battery-surface is gradually increased, the elementary striæ, especially near the head of the column, have their duration shortened so as to leave dark intervals at regular stages in the column. These successive short-lived elementary striæ form a series of diagonal lines, each series of which traces a sketch of a flocculent stria. As the surface is still further increased these diagonal lines appear more and more crowded together, until at last they blend into unbroken flocculent striæ.

This compound nature and mode of formation may be taken as a general characteristic of the flocculent striæ. In some tubes it is more easily brought out, in others only with greater difficulty. In some it can hardly be verified experimentally without a loss of light so great as to mask the phenomenon. The apparent proper motion of the flocculent striæ depends, as is easily seen, upon the position at which the elemen-

tary striæ are replaced. If they are replaced in the positions which their predecessors held, the flocculent striæ will appear straight in the mirror; if they are replaced successively nearer the positive terminal, the apparent proper motion will be in the normal direction; if nearer the negative, it will be reversed.

Fig. 2.



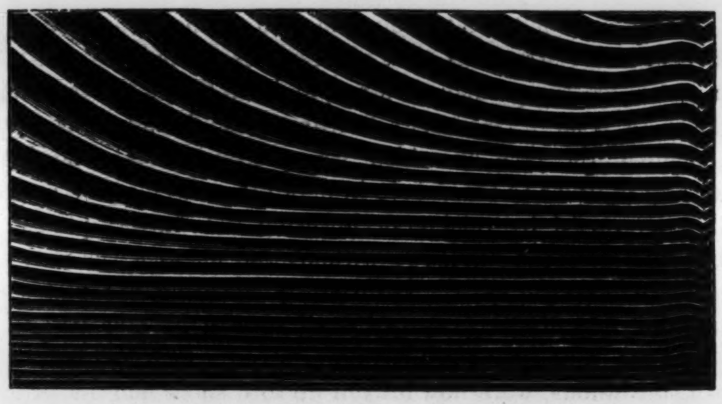
An ether-tube examined in the same way showed nearly the same features as the last. The elementary striæ were, however, not so easily separable; and the flocculent striæ were formed as usual at an earlier stage near the head of the column than near the foot of it.

In another carbonic-acid tube the proper motion of the flocculent striæ was coincident in direction with that of the elementary; and the latter were consequently more difficult to disentangle. One point in this tube was particularly noticeable, viz. that as the column of flocculent striæ retreated, so did the negative glow advance. The two remained throughout the entire discharge the same distance apart.

Fig. 3 represents the discharge in a hydrogen-tube of conical form,

the diameter of which varied from capillary size to $\frac{1}{2}$ inch, the capillary end being at the bottom. The positive terminal is at the top. The principal interest of this tube consists in showing the influence of diameter upon the velocity of proper motion. The wider the tube the

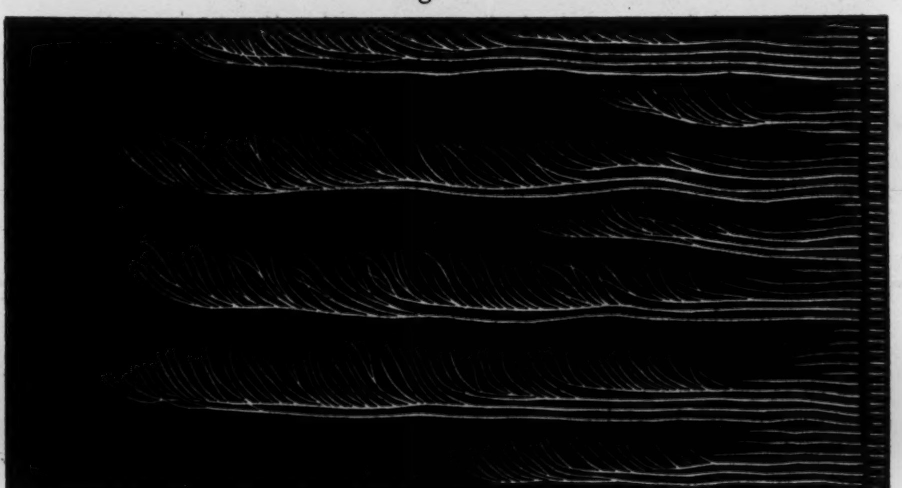
Fig. 3.



freer, it seems, the striæ are to move. The same fact may be observed by comparing tubes differing in diameter, but in other respects the same; but the conical tube brings out the fact in the most striking manner.

Fig. 4 represents a chloroform-tube, in which a piece of cotton-wool had been inserted with a view of ascertaining whether any motion would be communicated to it by the current. This proved to be the case; but I do not attempt here to describe the phenomenon. To the unassisted eye the discharge was extremely brilliant; it passed in a column not quite straight, but in a writhing snake-like curve, with flaky striæ at

Fig. 4.



intervals through its length. When viewed in the mirror the striæ were seen to spread themselves out with slight, but irregular, proper motion. With an increased battery-surface, or with a greater number of cells, but more notably with the latter, not only were the striæ lengthened, but from several of the long elementary striæ shorter ones were thrown out nearly at right angles to the former. These were of short duration, and had great proper motion. The general appearance of these compound striæ was that of branches of fir trees, the twigs of which represented the permanent striæ, and the leaves the secondary.

Beside these, a large (Geissler's "hydrocarbon") tube was examined with a magnet the pole of which was placed near the head of the column; and in order to trace more in detail the effect of the magnet, its strength was varied by raising or lowering the battery-plates. The general character of the discharge without the magnet was very similar to that represented in fig. 1. On slightly lowering the plates of the magnet-battery the discharge spread itself over a greater breadth than before. At the same time the elementary striæ, which had for the most part been continuous, were now broken up into short lengths, presenting the first features of flocculent striæ. On further lowering the plates these flocculent striæ became more and more developed until the whole field in the neighbourhood of the magnetic pole became filled with such striæ. It is well known that one effect of the magnetic field is to bring out striæ in portions of tubes where no striæ were visible before, and also that the striæ so brought out present a flocculent appearance; but the revolving mirror shows this fact in a more decisive manner. Another effect of the magnetic field is to drive the discharge to one side of the tube in accordance with Ampère's law—in other words, to constrict the discharge. In narrower tubes than the one here described, the constriction goes so far as to imitate the appearance of a capillary tube; and this effect is borne out by the revolving mirror. The intensification of the discharge and its concomitant phenomena within the range of the magnetic field are in accordance with the experiment of Faraday, wherein he showed the increased loudness of the report perceptible on breaking a current in between the poles of a magnet.

In a carbonic-acid tube (Gassiot's No. 454) I have succeeded in starting with a very weak current, capable of producing only elementary striæ, and thence passing to the production of flocculent striæ, either by strengthening the battery-current, or by inducing upon the existing current the action of a magnetic field. The identification of the results of these two independent processes, especially when combined with the comparison made above of the effect of magnetism with that of narrowing the tube, can hardly fail to have some important signification in the ultimate theory of the striæ.

Besides the tubes above mentioned many others were tried; but these will probably suffice for the present purpose.

The following are some of the general conclusions to which the foregoing experiments seem to lead:—

I. The thin flake-like striæ, when sharp and distinct in their appearance, either are short-lived or have very slow proper motion, or both.*

II. The apparent irregularity in the distribution of such striæ, during even a single discharge of the coil, is due, not to any actual irregularity in their arrangement, but to their unequal duration and to the various periods at which they are renewed. These striæ are, in fact, arranged at regular intervals throughout the entire column. The fluttering appearance usually noticeable is occasioned by slight variations in position of the elementary striæ at successive discharges of the coil. With a view to divesting the coil-discharge of this irregular character, as well as for other purposes, I devised two different forms of contact-breakers (one of which is described in the Royal Society's 'Proceedings,' vol. xxiii. p. 455); but I postpone a description of the second, as well as of the experiments arising from its use, to another occasion.

III. The proper motion of the elementary striæ is that which appertains to them during a single discharge of the coil. This appears to be generally directed from the positive towards the negative terminal. Its velocity varies generally within very narrow limits. It is greater the greater the number of coils employed, or the greater the electromotive force of the current. In some tubes it may be seen to diminish towards the close of the discharge; and even in rare instances alternately to increase and to diminish during a single discharge.

IV. Flocculent striæ, such as are usually seen in carbonic-acid tubes, are a compound phenomenon. They are due to a succession of short-lived elementary striæ, which are regularly renewed. The positions at which they are renewed determine the apparent proper motion of the elementary striæ. If they are constantly renewed at the same positions in the tube, the flocculent striæ will appear to have no proper motion and to remain steady. If they are renewed at positions nearer and nearer to the positive terminal, the proper motion will be the same as that of the elementary striæ; if they are renewed at positions further and further from the positive terminal, the proper motion will be reversed.

V. The velocity of proper motion varies, other circumstances being the same, with the diameter of the tube. This was notably exemplified in the conical tube. In tubes constructed for spectrum-analysis the capillary part shows very slight, while the more open parts often show considerable proper motion.

VI. Speaking generally, the discharge lasts longer in narrow than in wide tubes. In spectrum-tubes the capillary part gives in the mirror an image extending far beyond that due to the wider parts.

VII. The coil-discharge appears, in the earlier part of its development at least, to be subject to great fluctuations in extent. In all cases there

is a strong outburst at first. This, although sometimes appearing as a bright line, is always, I believe, really stratified. Immediately after this there follows a very rapid shortening of the column. The extent of this shortening varies with circumstances; but when, as is often the case, it reaches far down towards the positive terminal, a corresponding diminution of intensity is perceptible in the negative glow. The column of striæ, after rising again, is often subject to similar fluctuations. These, which are sometimes four or five in number, are successively of less and less extent, and reach only a short distance down the column or striæ. The rifts due to these fluctuations then disappear, and the striæ either continue without interruption, or follow broken at irregular intervals, until the close of the discharge.

VIII. The effect of the proper motion, taken by itself, is to shorten the column of striæ. But, as we have seen, the striæ are in many cases renewed from time to time. In regard to this point, the head of the column presents the most instructive features. After the cessation of these rifts, the general appearance of the field is that of a series of diagonal lines commencing at successive points which form the bounding limit of the column at successive instants of time. If the points are situated in a horizontal line, the striæ are renewed at regular intervals at the same place; and the length of the column is maintained by a periodic renewal of striæ, a new one appearing at the head of the column as soon as its predecessor has passed over one dark interval. If the boundary of the illuminated field rises, the length of the column increases; if it descends, the column shortens. In every case, however, the growth of the column takes place by regular and successive steps, and not irregularly. The intervals of the new striæ from one another and from the old ones are the same as those of the old ones from one another.

IX. The principal influence of a change in the electromotive force appears to consist in altering the velocity of proper motion. A change in the amount of battery-surface exposed produces a corresponding change in the duration of the entire discharge, as well as apparently in the development of some of the minor details of the striæ.

X. When the proper motion of the elementary striæ exceeds a certain amount, the striæ appear to the eye to be blended into one solid column of light, and all trace of stratification is lost. When this is the case the mirror will often disentangle the individual striæ. But there are, as might well be expected, cases in which even the mirror is of no avail, but in which we may still suppose that stratification exists. A variety of experiments have led me to think that the separation of the discharge into two parts, viz. the column of light extending from the positive terminal, and the glow around the negative, with a dark space intervening, may be a test of stratified discharge; but I cannot affirm any thing certainly on this point.

VI. "The Calculus of Chemical Operations.—Part II. On the Analysis of Chemical Events." By Sir B. C. BRODIE, Bart., F.R.S., late Professor of Chemistry in the University of Oxford. Received January 13, 1876.

(Abstract.)

Introduction.—An account is here given of the origin of our views of the constitution of ponderable matter, regarded as constituted of units compounded of "simple weights." These considerations lead to two systems, and two only, in which the unit of hydrogen is respectively expressed by the symbols α and α^2. Between these systems we have no absolute means of selection; but a preference is here given to the system α as immediately leading to the law of even numbers.

The exception presented by the binoxide of nitrogen is then considered, and an hypothesis suggested to account for this anomaly.

The object of the work is then defined—namely, given a chemical event, how are we to determine the events of which it is compounded?

Section I.—The *question of the Multiplication and Division of chemical equations* is here considered. It is shown that we may multiply and divide a chemical equation of the form $u=0$ by any chemical function, if the sum of the numerical coefficients in that equation is equal to zero, but otherwise not. A method is given by which every chemical equation may be brought under this form. Such an equation is termed a "normal" chemical equation, for it is an equation on which we may operate by the rules of elementary algebra.

It is then shown that every chemical expression of the form $A\,(x-a)\,(y-b)$, and also $A\,(x-a)\,(y-b)\,(z-c)\ldots$ (that is, the continued product of any number of such factors more than one), necessarily $=0$.

As regards the interpretation of normal chemical equations. Normal equations express the identity of the two members of the equation, not only as regards matter, but as regards matter and space also. Thus the equation $1+2\alpha\xi=2\alpha+\xi^2$ asserts not only that the matter of two units of water is identical with the matter of two units of hydrogen and a unit of oxygen, but also that an empty unit of space and the space occupied by two units of water are identical with the space occupied by two units of hydrogen and a unit of oxygen.

It is further shown that in any chemical equation any one of the prime factors of the equation may be substituted for another, and the equation will still be true.

Section II.—Our knowledge of the identity of matter is derived from chemical transmutations or events; and every chemical equation may be regarded as the record of such an event or some number of such events. Chemical events may be regarded as compound or simple. A compound event is defined as an event which is regarded in the system of events under our consideration as constituted of two or more events. A simple

event is an event which is not so regarded. Thus, for example, take the system of the four events :—

$$\begin{array}{ll} (1) & a^2\nu\ +a^3\kappa^2\omega\ =a\omega\ \ +a^4\kappa^2\nu, \\ (2) & a^4\kappa^2\nu+a^3\kappa^2\omega\ =a\omega\ \ +a^6\kappa^4\nu, \\ (3) & a^6\kappa^4\nu+a^3\kappa^2\omega\ =a\omega\ \ +a^8\kappa^4\nu, \\ (4) & a^2\nu\ +3\,a^3\kappa^2\omega=3\,a\omega+a^8\kappa^6\nu. \end{array}$$

The event 4 is a compound event, being the aggregate of the events 1, 2, 3; whereas the events 1, 2, 3 are in that system simple events, being incapable of such a construction.

Section III. *On the causes of events.*—The cause of an event is given when the operations are defined by the agency of which the event occurs. *Def.* If in any chemical event the change in the arrangement of the symbols, by which the composition of the units of matter before and after the event respectively is symbolized, be of such a nature that where in the arrangement before the event the symbol x appears the symbol a appears after the event, and where a appears before x appears after, so that the two arrangements differ in this respect and this respect alone, then the event occurs by the substitution of a for x, which is the "cause" of the event. Hence the same event may arise from more than one cause. Thus, for example, the event

$$\mathrm{A}xy+\mathrm{A}ab=\mathrm{A}ya+\mathrm{A}xb$$

occurs by the substitution of a for x and of b for y, for these symbols satisfy the condition given in the above definition.

It is similarly shown that the event

$$\mathrm{A}xyz+\mathrm{A}abz+\mathrm{A}ayc+\mathrm{A}xbc=\mathrm{A}xyc+\mathrm{A}abc+\mathrm{A}ayz+\mathrm{A}xbz$$

occurs by the substitution of a for x, b for y, z for c; and, further, that if the equation to any chemical event be of the form $\mathrm{A}\,(x-a)\,(y-b)\,(z-c)\,(v-d)\,(w-e)\ldots=0$, that event occurs by the substitutions of a for x, b for y, c for z, d for v, e for w . . . .

If in these substitutions any symbol, say 'a'$=1$, the event occurs by the transference of the simple weight thus symbolized.

The following event occurs in three ways by the substitution of ξ for χ, the hydride of propyl, $a^4\kappa^3$, being constant,

$$a^4\kappa^3\chi^3+3a^4\kappa^3\xi^2\chi=a^4\kappa^3\xi^3+3a^4\kappa^3\xi^2\chi,$$

the equation being of the form

$$a^4\kappa^3(\chi-\xi)^3=0.$$

Similarly the event

$$a^2\kappa\chi^3+3a^2\kappa\chi=3a^2\kappa\chi^2+a^2\kappa$$

is an event occurring in three ways by the transference of χ, the equation being of the form

$$a^2\kappa(\chi-1)^3=0.$$

I submit the following equation to the consideration of the reader,

$$a^4\kappa^3\xi\,(\beta-\xi)\,(\chi-\xi)\,(a\kappa^2\xi-1)=0.$$

Section IV. *Elementary analysis of events.*—If the equation to a chemical event be capable of expression as the continued product of rational factors of the form previously given $(x-a)$, x and a being prime factors of the equation, the event is a simple event incapable of further resolution; but occasionally the equations to events may be expressed by rational factors, although not of this form. In this case they admit of an easy analysis into other events of which they are the aggregates. Take, for example, the equation

$$\alpha\chi^2+2\alpha\omega=2\alpha\chi+\alpha\omega^2,$$

or

$$\alpha(\chi+\omega-2)(\chi-\omega)=0,$$

which may be written thus,

$$\alpha(\chi-1)(\chi-\omega)+\alpha(\omega-1)(\chi-\omega)=0,$$

whence

$$\alpha(\chi-1)(\chi-\omega)=0,$$
$$\alpha(\omega-1)(\chi-\omega)=0,$$

the constituents being

$$\alpha\chi^2+\alpha\omega=\alpha\chi+\alpha\omega\chi,$$
$$\alpha\omega\chi+\alpha\omega=\alpha\omega^2+\alpha\chi.$$

Again, the following event is the action of chlorosulphuric acid upon water:—

$$\alpha\theta\xi^2\chi^2+2\alpha\xi=\alpha\theta\xi^4+2\alpha\chi.$$

This equation is of the form

$$\alpha(\theta\xi^2+\theta\xi^3-2)(\chi-\xi),$$

whence

$$\alpha(\theta\xi^2\chi-1)(\chi-\xi)+\alpha(\theta\xi^3-1)(\chi-\xi),$$

the constituents being

$$\alpha\theta\xi^2\chi^2+\alpha\xi=\alpha\theta\xi^3\chi+\alpha\chi,$$
$$\alpha\theta\xi^3\chi+\alpha\xi=\alpha\theta\xi^4+\alpha\chi.$$

The analysis of these two phenomena here indicated has actually been effected by experiment.

Section V.—In this section the doctrine of *Chemical Congruity* is discussed, two chemical functions being said to be congruous to one another in reference to a special substitution if they assume the same value when that substitution is respectively effected in them.

Further, a method is given for the *Development of chemical functions*, and for the complete theoretical analysis of any chemical event whatsoever—the theoretical analysis of a chemical event occurring by any number of specified substitutions, namely, of a for x, b for y, c for $z, \ldots,$ being here said to be effected when all the different chemical events occurring in any way whatever by these substitutions are enumerated, the aggregate of which constitutes the event in question.

I shall not attempt any further abstract of this section, which is given in my memoir with as much brevity as is consistent with the comprehension of the subject.

Presents received, May 4, 1876.

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"Note on a Simultaneous Disturbance of the Barometer and of the Magnetic Needle." By the Rev. S. J. PERRY, F.R.S. Received April 27, 1876.

As any question of the connexion between the different phenomena of nature may be of interest, I will venture to call attention to an observation made, in February last, in China, which tends to establish a fresh link between terrestrial magnetism and meteorology.

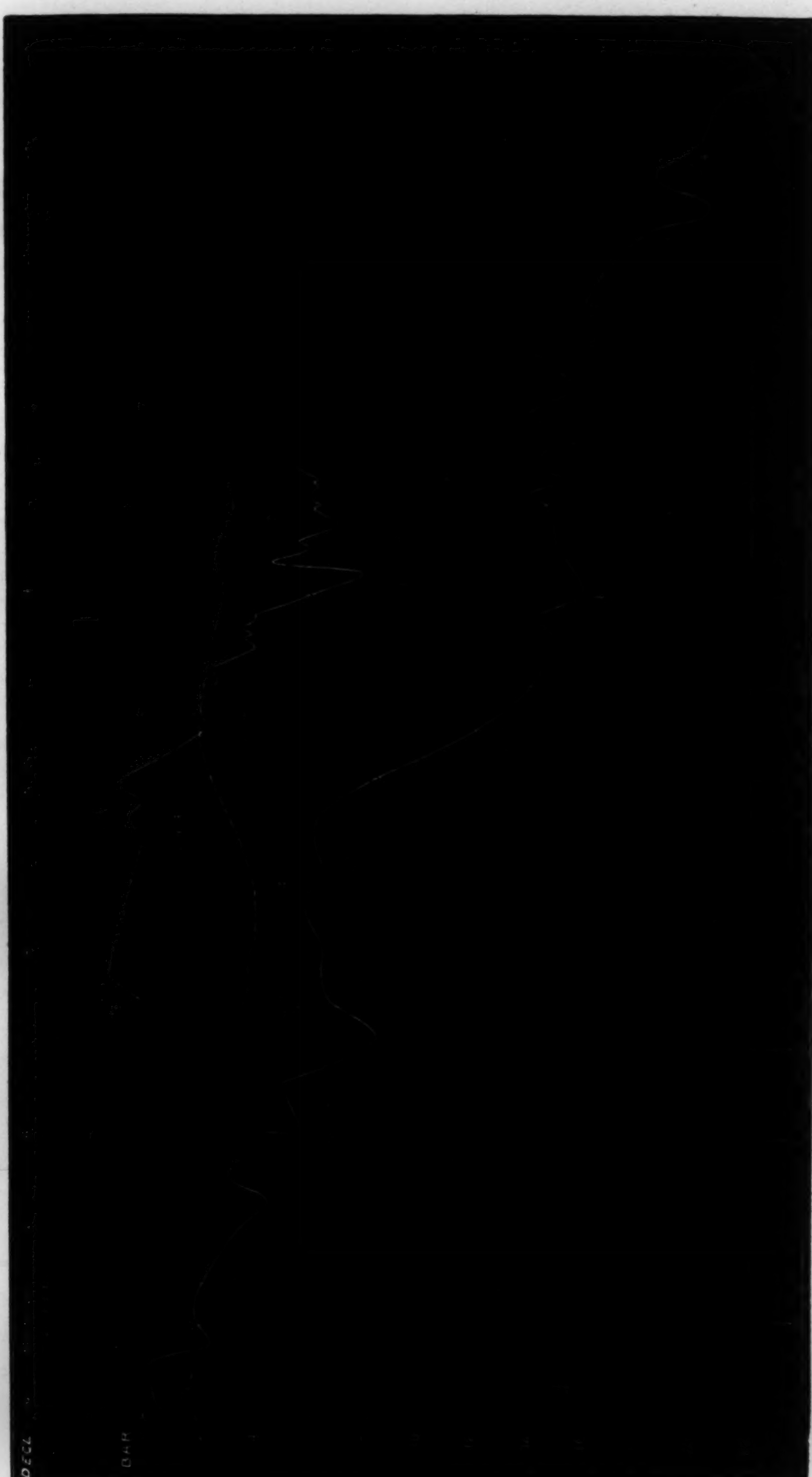
From the curves that accompany this note (p. 91), it would seem that the same cause may be capable of producing a simultaneous perturbation of the barometer and of the suspended magnets.

At the newly established observatory of Zi-Ka-Wei, near Shanghai, no very considerable disturbances of the magnetic elements have been observed in 1874 and 1875 ; but on February 20, 1876, the director, the Rev. M. Dechevrens, S.J., had the good fortune to notice the first irregularities of any great extent that had ever been observed in China ; and these movements were found afterwards to be almost identical in all their inflexions with those traced by the mercury in the barometric curve. The first observations on the morning of the 20th showed an increase of westerly declination and of dip, with a diminution of the horizontal component of the intensity, the observed value of H.F. being the least ever recorded at this station. From $10^h\ 32^m$ A.M. Mr. Dechevrens was able to give his whole attention to the declination-magnet, which was then moving westward with a rising barometer. The barometer attained a maximum about $10^h\ 48^m$ A.M., followed by two westerly maxima of declination at 11 A.M. and 11.40. From $10^h\ 48^m$ until shortly after 4 P.M. the barometer fell rapidly, the magnet in the mean time moving almost constantly in an easterly direction, attaining its least declination of $1°\ 55'{\cdot}07$ W. at $4^h\ 24^m$ P.M. The amplitude of the declination disturbance was $10'{\cdot}33$, a large amount if we consider that the mean diurnal range scarcely exceeds $3'{\cdot}5$ in the winter months. On the previous evening the passing clouds appeared to be lit up on their borders, possibly by an aurora. A N.E. wind blew very constantly throughout the day, and the rain fell from 3 P.M. until the following morning.

The dotted line represents the range of the declination-magnet on February 21.

S. J. PERRY.

Stonyhurst Observatory,
April 26, 1876.



Comparison of movements of the Declination-magnet and of the Barometer, February 20, 1876, at Zi-Ka-Wei, near Shanghai. The dotted line represents the normal variation of the magnet as observed the next day, February 21.

June 1, 1876.

The Annual Meeting for the election of Fellows was held his day.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Statutes relating to the election of Fellows having been read, Mr. Warren De La Rue and Prof. P. M. Duncan were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present having been collected, the following candidates were declared duly elected into the Society :—

Capt. William de Wiveleslie Abney, R.E.
Prof. Henry Edward Armstrong, Ph.D.
Rev. William B. Clarke, M.A., F.G.S.
James Croll, F.R.S.E.
Edwin Dunkin, Sec. R.A.S.
Prof. John Eric Erichsen, F.R.C.S.
David Ferrier, M.A., M.D.
Col. Augustus H. Lane Fox.
Prof. Alfred Henry Garrod, M.A.
Robert Baldwin Hayward, M.A.
Charles Meldrum, M.A., F.R.A.S.
Edward James Reed, C.B.
Prof. William Rutherford, M.D.
Robert Swinhoe, F.R.G.S.
Prof. Thomas Edward Thorpe, Ph.D.

Thanks were given to the Scrutators.

The Right Hon. B. Disraeli was admitted into the Society.

June 15, 1876.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

Mr. William Archer, Capt. William de Wiveleslie Abney, Mr. Edwin Dunkin, Prof. John Eric Erichsen, Dr. David Ferrier, Col. Augustus H. Lane Fox, Prof. Alfred Henry Garrod, Mr. Robert Baldwin Hayward, Mr. Edward James Reed, Prof. William Rutherford, and Mr. Robert Swinhoe were admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Report to the Hydrographer of the Admiralty on the Voyage of the 'Challenger' from the Falkland Islands to Monte Video, and a Position in lat. 32° 24′ S., long. 13° 5′ W." By Prof. Wyville Thomson, F.R.S., Director of the Civilian Scientific Staff on board. Received May 5, 1876.

(See Proc. Roy. Soc. vol. xxiv. p. 623.)

II. "Preliminary Note on the Structure of the Stylasteridæ, a group of Stony Corals which, like the Milleporidæ, are Hydroids, and not Anthozoans." By H. N. Moseley, Naturalist on board H.M.S. 'Challenger.' Communicated by Professor Wyville Thomson, F.R.S., Director of the Civilian Scientific Staff. Received May 5, 1876.

On 14th February, 1876, in lat. 37° 17′ S., long. 53° 52′ W., off the mouth of the Rio de la Plata, the trawl brought up from 600 fathoms a number of specimens of corals of the family Stylasteridæ (Gray *). The specimens included six genera of the family and seven species. They were all in most excellent preservation, notwithstanding the fact that they had been slowly raised from 600 fathoms, and all had their generative organs in full development. An opportunity which had long been desired was thus afforded for making a detailed examination of the structure of the soft parts of this family, which, in the structure of its coralla, shows so many points of variance from that of Zoantharian coralla. From observations made on a species of *Stylaster* obtained from 500 fathoms off the Meangis Islands, and on a *Cryptohelia*, a short account of which is given in the Royal Society's 'Proceedings,' vol. xxiv. p. 63, I had already been led to suspect that the Stylasteridæ might prove to be Hydroids, although I did not venture to express this opinion because the evidence was then insufficient. The examination of the series of forms obtained off the Rio de la Plata at once showed that the Stylasteridæ are true Hydroids.

Unfortunately the trawl came up rather late in the day, and hence a very short period of daylight was available for the examination of the animals in the fresh condition; but it sufficed for the sketching of the male gonophores of a new genus of Stylasteridæ (*Polypora*), with the stages of development of the spermatozoa, and of the female gonophores of *Cryptohelia*.

Portions of the corals were preserved by means of chromic acid, osmic acid, absolute alcohol, and glycerine; and they were subsequently examined in the usual manner by means of sections. In cutting the sections, a new method, described by Mihakowics, 'Arch. für mikroskopische Anatomie,' ii. Bd. 3 Hft. p. 386, was adopted, and found to yield most astonishingly successful results. The method seems to supply a want long felt of a means of cutting fine sections of structures the parts of which are very loosely held together, and where it is desirable to maintain the exact relations in position of parts which in the sections often become entirely disconnected from one another. Mihakowics used his method for sections of vertebrate embryos; it is certainly the best possible method for the investigation of decalcified tissues, such as those of Corals or Echinoderms. A strong jelly, composed of equal parts of glycerine and gelatine, is used as an imbedding substance; it permeates

* Ann. and Mag. Nat. Hist. vol. xix. (1847).

the tissues, and takes the place of the hard calcareous supporting structures which have been removed by the acid. The sections are mounted in glycerine, and the imbedding substance, which is left *in situ* in the sections, becomes perfectly transparent, in fact almost invisible in this fluid. I stain the decalcified corals with carmine, then soak them in glycerine, and then transfer them directly to the warm fluid jelly, instead of treating them first with absolute alcohol after staining, as does Mihakowics. A teaspoon heated in hot water is a most convenient instrument for transferring the small masses of tissue, with the fluid jelly, to the cavities in the hardened liver used as an imbedding base. I have dwelt upon this method because it seems to me likely to be one which will prove of the greatest service in all kinds of difficult histological problems, such as Corti's organ, early stages of embryos, retina, &c. It is quite possible by the method to obtain sections of a single hydroid sporosac or planula.

The Stylasteridæ obtained off the Rio de la Plata comprised six genera, viz.:—*Stylaster*; *Cryptohelia*; *Allopora*; *Errina*; a new genus, *Polypora*; and a further new genus allied to *Errina*, which I propose to term *Acanthopora*. There is much confusion as to the determination of even the genera of the Stylasteridæ, and I have found it impossible to determine species in the absence of specimens for comparison. The *Stylaster* appears probably to be *S. erubescens* of Pourtales *. The *Cryptohelia* is the same as that obtained all over the world by the 'Challenger' in deep water, and apparently not specifically distinct from *C. pudica* †. Of the *Allopora* I cannot determine the species. There is one coral which appears to belong to the genus *Errina*, Gray ‡, of which a further diagnosis is given from the type specimens by Saville Kent §, and one of the allied new genus *Acanthopora*. The whole of the classification of the Stylasteridæ will need revision on the more certain basis of the knowledge of the structure of the soft parts. In the older regions of its stem *Lepidopora* appears to assume the character of a *Stylaster*. The coral for the reception of which I form the new genus *Polypora* differs markedly from other members of the family; I at first took it to be a *Millepora* with unusually large zooids.

The genus may be thus characterized, as far as the hard parts are concerned:—

Genus Polypora.

Corallum pure white, composed of a finely reticular but compact cœnenchym. It forms single, stout, vertical stems, usually compressed from before backwards, so as to be oval in transverse section. The stem gives off a limited number of irregularly dichotomous branches, which

* Illustrated Catalogue of the Museum of Comparative Zoology at Harvard College. No. IV. Deep-Sea Corals. By L. F. de Pourtales. Cambridge, Mass. 1871, p. 34.

† Hist. Nat. des Coralliaires, par. MM. Milne-Edwards et J. Haime, t. ii. p. 127.

‡ Proc. Zool. Soc. 1835, p. 35.

§ Proc. Zool. Soc. 1871, p. 282.

are flattened like the stem from before backwards, and tend to coalesce by their lateral margins and assume a flabellate form, which is sometimes somewhat curved. The surface of the corallum is perfectly even and smooth, and pierced by deep calicular cavities, simply circular in outline, and of two kinds, large and small. The larger less numerous calicles are disposed at irregular intervals over the surface; they are very deep, reaching nearly to the centre of the axis of the branch or stem, and contain a deep-seated, very long, and slender style with a brush-like tip. The more numerous smaller calicles vary in size; they are thickly disposed between the larger ones; they have no style. Seated beneath the surface between the calicles are numerous ovoid cavities, the ampullæ, which in this genus do not project; at certain stages of development these communicate with the exterior by minute irregularly shaped pores, seated in small shallow pits on the surface of the corallum. The calicles are usually more abundant on one face of the corallum than on the other, especially in its older basal region.

Type of the genus *Polypora dichotoma.*

Dimensions of the specimen:—Height of the corallum from $1\frac{3}{4}$ to 1 inch; breadth of fan 6 inches; diameter of stem from $1\frac{3}{4}$ to 1 inch; diameter of the mouths of the larger calicles $\frac{1}{50}$ of an inch.

A further examination of the species of *Stylaster* obtained off the Meangis Islands was made in connexion with that of the corals referred to above. This *Stylaster* resembles *Cryptohelia* in every particular, excepting that it has not the peculiar lid in front of its calicles. It will have to be separated from the other *Stylasters*, and placed in the same genus as *Cryptohelia.*

Structure of the soft parts of the Stylasteridæ.

In all the Stylasteridæ examined there is present an abundant cœnosarc, made up, as in the Milleporidæ, of a network of anastomosing canals, composed of an endoderm and ectoderm, and ramifying in corresponding canals in the spongy trabecular calcareous cœnenchym. In *Polypora* the meshes of the network are comparatively close; in all the other genera examined far more widely open. In *Cryptohelia* and the *Stylaster* from off the Meangis Islands, in which the calicles appear as swellings seated upon slender connecting branches, bundles of larger canals traverse the axes of these branches, and connect the zooid groups of the several calicles with one another. A continuous layer of tissue, as far as has yet been seen without cellular structure, but containing thread-cells, covers the external surface of the cœnosarc in all the genera. In all the Stylasteridæ there are two kinds of zooids, as in *Millepora*; the larger and less numerous have mouths and a special layer of digestive cells lining their body-cavity. The more numerous smaller zooids have no mouths and no gastric cells. The alimentary zooids are short and cylindrical; the smaller or tentacular zooids long and tapering. The alimentary zooids in *Stylaster erubescens* have eight tentacles; in *Cryptohelia*, and in the

Stylaster so closely resembling it, they are devoid of tentacles. In *Allopora* they have twelve, in *Errina* four, in *Acanthopora* six, in *Polypora dichotoma* four. In *Polypora*, in which the tentacles of the alimentary zooid were examined in the fresh condition, the tentacles were seen to be clavate, the heads of the tentacles being somewhat elongate, not spherical as in *Millepora*. I am as yet uncertain whether these tentacles are clavate in the other genera. The point is difficult to determine in the extremely contracted condition of the organs in reagents. The tentacles of these alimentary zooids are very short; they are placed in a single whorl at the base of the broadly conical hypostome. In *Cryptohelia* and in the allied *Stylaster* the tentacleless alimentary zooids are flask-shaped, with a conical projecting hypostome, as seen by Sars *. The rounded bottoms of the zooids are blind and unconnected with the cœnosarcal canals; but a series of canals radiate upwards from the sides of the flask to branch and join the network above. The smaller zooids I have termed tentacular zooids, because, though invariably devoid of tentacles themselves, they have the form of the simple elongate tentacles, and evidently must perform a tentacular function. In *Polypora*, *Errina*, and *Acanthopora* these tentacular zooids are dispersed irregularly amongst the alimentary zooids; in *Cryptohelia*, *Stylaster erubescens*, and *Allopora* they are arranged in a circlet around a centrally placed alimentary zooid in each so-called calicle of the corallum. The bases of these zooids communicate by large vascular offsets with the general network of the cœnosarc. The cavities of the alimentary zooids are four-rayed in transverse section, and in *Polypora* they divide at their base into four large vascular trunks, which subdivide to join the cœnosarcal meshwork. The cavities of the tentacular zooids are circular in transverse section. Both kinds of zooids are provided with strong circular and longitudinal muscles, which form wide conspicuous bands beneath the ectoderm. The alimentary zooids are situate on the summits of the styles of the corallum, where these are present. In *Polypora*, in the retracted condition of the zooids, the styles traverse the axes of the zooids from below for at least two thirds of their length. In *Polypora*, *Errina*, and *Acanthopora* the zooids of both kinds are retracted within long sacs, the cavities of which communicate with the surrounding network of the cœnosarc by a series of radially disposed canals, which canals in transverse sections of the zooids have at first sight exactly the appearance of a system of mesenteries. In *Cryptohelia* and the *Stylaster* so closely resembling it the alimentary zooids, lying as they do deep in the calicles, are probably never far protruded. The tentacular zooids are partly retracted between the pseudo-septa, partly doubled down within the calicles when the colony is in the retracted condition. In the other *Stylasters* and in *Allopora* the conditions are much the same. Two kinds of thread-cells are present, large and small: the large are of the slightly curved cylindrical form, and emit a thread with an elongate enlargement upon it near the sac, beset with a spiral of spines; these

* Forh. Selsk. Chr. 1872, p. 115.

larger cells are mostly gathered together in nematophores, which are disposed irregularly amongst the zooids in *Polypora*, regularly in the intervals between the tentacular zooids at the margins of the calicles in *Cryptohelia* and the *Stylaster* resembling it. The smaller kind of thread-cells are of an ovoid form, slightly flattened on one side; they occur in the tentacles of the alimentary zooids, and form a closely set covering over the entire external surfaces of the tentacular zooids. No three-spined thread-cells, like those occurring in *Millepora*, exist in the Stylasteridæ. Reproduction takes place by means of adelocodonic gonophores, which are produced as buds from the cœnosarcal network without having any other connexion with the other zooids. They occupy in the corallum the ampullæ which in *Polypora* are concealed beneath the even external surface of the corallum, but in the other genera of Stylasteridæ show themselves as rounded prominences on the surface of the coralla, being specially prominent in *Errina* and *Distichopora*. The Stylasteridæ are all diœcious. Females only of *Errina* and *Cryptohelia** have been examined, and males only of the other genera. The generative elements of *Acanthopora* were not observed at all. In the males of *Polypora* the gonophores present the usual structures occurring in Hydroids; they are simple ovoid sacs, with an axially placed spadix, and resembling in all respects those, *e. g.*, figured by Allman from *Laomedea flexuosa*†. The gonophores are sometimes single in the ampullæ, sometimes in groups of two or three arising from a common base with their contents in various stages of development. The ripe spermatozoa are precisely similar in form to those of *Garveia nutans*‡. In *Allopora*, *Acanthopora*, and *Stylaster erubescens* the male gonophores have a similar structure. In the *Stylaster* allied to *Cryptohelia* the male elements are developed in a series of sacs, which encircle the calicle, often in a double row. The sacs spring from the cœnosarcal network; they contain numerous smaller globular cysts, attached to a common basal endodermal tissue. These cysts are some of them filled with ripe spermatozoa, others with spermatic cells in various stages. The female gonophores are, in *Errina*, simple, *i. e.* each ampulla contains only a simple ovum or embryo. In *Cryptohelia* large sacs are present at the sides of the calicles, which contain ova and embryos in all stages of development. Only a single sac of the kind is developed in relation with each calicle. In both genera the spadix in its earliest stage is cup-shaped, the cup having fitted into it an ovum with germinal vesicle and spot well marked. The ova early lose the germinal vesicle and spot, and develop into very large planulæ, in the same manner as, *e. g.*, those in *Laomedea flexuosa*§. In *Errina* the planulæ are more ovoid in form than in *Cryptohelia*, in which they are long

* Off Japan last year a small fragment of what, at the time, I determined to be a male *Cryptohelia* was obtained by the dredge. I unfortunately cannot now refer to the specimen.

† 'A Monograph of the Gymnoblastic or Tubularian Hydroids,' by G. J. Allman, M.D. &c., Ray Soc. part 1, p. 65.

‡ *Ibid.* pl. xii. fig. 9.

§ Allman, *l. c.* p. 86.

and worm-like, measuring $\frac{1}{8}$ of an inch in length. They have a thick transparent ectoderm, abundantly supplied with the larger form of thread-cells. The spadix in both genera, as the development of the ovum proceeds, becomes divided at its margin into a series of lobes, which lobes subdivide and encroach over the surface of the ovum until more than half the proximal surface of the ovum is thus embraced by the cup of the spadix. The lobes of the margin of the spadix appear just like developing tentacles; and the spadix of *Cryptohelia* was at first supposed to be a developing actinula. The outer, thin, perforated calcareous walls of the ampullæ in *Errina* appear to get thinner as development of the embryo advances, until they fall away or are absorbed altogether, and give free exit to the planula. In *Cryptohelia* the planulæ probably escape through the mouths of the calicles. The endoderm, spadices, &c. are coloured red by a colouring-matter, soluble in spirit, insoluble in glycerine, in *Polypora*, *Cryptohelia*, and *Errina*. In the *Stylaster* resembling *Cryptohelia* the coloration is dusky green. The green colouring-matter is soluble in spirit, and yields an absorption-band in the spectrum. In *Polypora* the living layer of cœnosarc set free by decalcification is very thick, not merely a thin superficial film as in *Millepora*; indeed all but the most central axial regions of the branches of the corals are in active life. In the other genera the whole of the coral appears to maintain its vitality, there being no dead region represented by a cavity after decalcification.

Conclusions.

Since the observations of Prof. Sars* on the polyps of *Allopora oculina* it has been to some extent suspected that the Stylasteridæ were not Anthozoa, but possibly allied to the Milleporidæ, although the fact was not in any way demonstrated. Milne-Edwards long ago expressed himself extremely uncertain as to the affinities of *Distichopora*, and suspected that it might be an Alcyonarian †. In consideration of the facts now ascertained, there can be no doubt as to the hydroid affinities of the family. The Stylasteridæ appear to form a very natural family. They all possess two kinds of zooids. The tentacular zooids are closely similar in form in all the genera; and in the variations in the forms of the alimentary zooids all gradations are present. The thread-cells appear to be alike in form in all the genera. In all the gonophores are developed within ampullæ. The corals all bear, as far as has yet been ascertained, fixed sporosacs, as do, according to Allman, all deep-sea Hydroids ‡. It is possible, however, that forms such as *Stylaster sanguineus* occurring in shallow water § may bear planoblasts. There can be no doubt that *Distichopora* will prove closely allied to the other six genera of Stylasteridæ : its well-marked ampullæ and two kinds of pores are decisive in

* Sars, Forh. Selsk. Chr. 1872, p. 115.

† MM. Milne-Edwards and Haime, *l. c.* t. iii., Appendice, p. 451.

‡ Allman, *l. c.* vol. ii. p. 155; also 'Nature,' Oct. 28th, 1875, p. 556.

§ Pourtales, *l. c.* p. 83.

the matter. *Pliobothrus* is said by Pourtales * to have "occasional round cavities in the centre of its branches filled with a yolk-like substance contained in a membrane." These cavities seem to be ampullæ; and if so, then *Pliobothrus* may prove to belong to the Stylasteridæ, and not to the Milleporidæ. In a specimen of *Pliobothrus* obtained by the 'Challenger' I have been able to detect neither ampullæ nor tabulæ. It will evidently be possible easily to form natural genera for the Stylasteridæ characterized by the number of tentacles of the alimentary zooids, grouping of the tentacular zooids around them, &c. This I propose to attempt when I have completed my study of the subject.

The Milleporidæ differ from the Stylasteridæ in having tabulæ, and in possessing neither styles nor ampullæ, as well as in having their mouthless zooids provided with numerous tentacles. The two families have, however, many points of alliance, and they should, provisionally at least, be referred to a special suborder of the Hydroidea, which may be termed the Hydrocorallinæ.

A most remarkable result of the present inquiry is the determination that the calicles of *Stylaster* and *Cryptohelia* are tenanted and formed by colonies of zooids, and not by single polyps, as was most naturally hitherto supposed to be the case. Prof. Verrill, in criticising Prof. Agassiz's relegation of the Rugosa to the Hydroidea †, dwells on the utter impossibility of Acalephs forming corals with distinct septa; yet in *Cryptohelia* and the *Stylasters* septa are present in the corallum, which in many cases so closely resemble those of Zoantharian corals that these corals were placed by Milne-Edwards in the Oculinidæ, and the septa were never suspected to be pseudo-septa until Sars ‡ observed that in *Allopora oculina* the tentacles (tentacular zooids) were situate between the septa, and not upon them. I should not have detected the compound nature of the calicular groups in *Stylaster* had I not been led up to the fact by the examination of other genera of the family, in which the tentacular zooids are widely separated from the alimentary ones. The determination of the compound nature of the calicular groups at once explains the otherwise very anomalous arrangement of the pseudo-septa in many Stylasteridæ. The condition existing has been described § as a "tendency of the septa to unite by their inner edges and enclose in the interseptal chamber thus formed the septa of a higher order." The real explanation of the matter is that the apparent interseptal chambers are the pores or calicles of the tentacular zooids. In those species in which the tentacles are removed from harm's way in the retracted condition of the coral by being bent inwards down into the wide cavity containing the alimentary zooid (calicular cavity), these pores have their walls incomplete on the side nearest to the calicle, and take the form at their mouths of elongate slits, in order to allow of this inward

* Pourtales, *l. c.* p. 57.

† Prof. A. E. Verrill, 'Ann. & Mag. Nat. Hist.' 1872, 4th ser. vol. ix. p. 358.

‡ Forh. Selsk. Chr. 1872, p. 115.

§ Pourtales, *l. c.* p. 33.

inclination of the contained tentacular zooid when at rest, or when feeding the deeply seated alimentary zooid. The supposed included septa of higher order are the styles of the tentacular zooids. In some forms of the family these styles are brush-like in shape, just like the central styles of the alimentary zooids; they have this form in *Allopora mineacea* *, and less markedly in *Stylaster complanatus*, Pourt. † In some Stylasteridæ, as *e. g.* in *Stylaster amphihelioides*, S. Kent ‡, there is no appearance at all of pseudo-septa. The pores of the tentacular zooids are simple circular-mouthed pits, arranged in a circle around the large pore of the alimentary zooid. In *Allopora subviolacea*, S. Kent §, the pores of the tentacular zooids are, in some zooid groups in the same specimen, mere pores; in others slits communicating with the cavity of the pore of the alimentary zooid. The irregularly scattered condition of the zooids existing in *Polypora* is to be regarded as the primitive one in genesis, from which that existing in *Stylaster amphihelioides* and that in *Allopora subviolacea* represent transitional stages towards the high specialization of the zooid groups found in *Cryptohelia* and other species at present termed *Stylaster*.

It has hitherto been a matter of regret that the Hydroidea were of such a structure as to be unsuitable || for preservation in the fossil state, and that thus we were almost, excepting as far as Graptolites are concerned, without direct evidence as to the forms which may have been presented by their remote ancestry. We have now two families excellently adapted for preservation as fossils, viz. the Milleporidæ and the Stylasteridæ. At present no members of these families appear to have been observed in rocks older than the tertiary deposits. A single species only, *Distichopora antiqua*, is known to occur in tertiary beds in France, at Chaumont and Valmondois ¶; but now that special attention will be directed to these corals, and their structure is better understood, no doubt allied fossil forms will be detected. It seems just possible that amongst Palæozoic corals such forms as *Cyathonaxia* may have been tenanted by a group of hydroid zooids with a large alimentary zooid situate upon the projecting style. *Cystiphyllum vesiculosum* has a crowd of small slit-like pits covering the inner surface of its calicle, which have all the appearance of having been tenanted by hydroid tentacular zooids. I cannot, however, now refer to specimens; indeed I have never seen any. Ampullæ seem to be absent in these corals; but in shallow-water forms, as in *Millepora*, they probably would be so. It is quite possible that the Millepores produce Medusæ.

Although the Milleporidæ take a very large part in the formation of coral reefs, the Stylasteridæ have very little share in the building up of these structures, being for the most part confined to the deep sea. A few

* Pourtales, *l. c.* pl. iii. fig. 15.

† Pourtales, *l. c.* pl. ii. fig. 17.

‡ Saville Kent, *l. c.* pl. xxiv. fig. 1 *c*.

§ *Ibid.* pl. xxv. fig. 2 *a*.

|| Allman, *l. c.* vol. ii. p. 231.

¶ MM. Milne-Edwards & Haime, *l. c.* t. iii., Appendice, p. 451.

species only occur in shallow water, and apparently not in great abundance. In deeper water, however, the Stylasteridæ are most luxuriant. Immense quantities of a large flabellate red *Distichopora*, brought from the Marquesas group, are sold to tourists at Honolulu. The corals are said to come from deep water. The results of the 'Challenger's' dredging off the Rio de la Plata in 600 fathoms showed that at that depth very considerable deposits of calcareous matter must be formed by these various genera of hydroid corals, growing associated as they do in masses and attached to one another. Large dead masses of *Polypora* brought up by the dredge were especially remarkable, weighing more than 1 lb., and forming bases of attachment for sponges and all kinds of other animals.

I am at present engaged in preparing a series of drawings illustrative of the anatomy of the Stylasteridæ, which I hope shortly to lay before the Royal Society, together with a more complete account of the structure of these corals.

South Atlantic,
March 24, 1876.

III. "On the Comparative Anatomy of the Auditory Ossicles of the Mammalia." By Alban H. G. Doran, F.R.C.S. Communicated by Professor Flower, F.R.S. Received May 5, 1876.

(Abstract.)

The following observations have been made during the preparation of a series of the small ear-bones of the higher Vertebrata for the Museum of the Royal College of Surgeons of England, an undertaking which was commenced in the autumn of 1874, and is in the course of rapid enlargement up to the present date.

The foundation of the entire series was a small collection of the osseous auditory apparatus of the domestic and common indigenous animals of Germany, purchased by the College of Dr. Max Hübrich, of Munich, a few years since. Following the suggestions of the Conservator, Professor W. H. Flower, F.R.S., the author succeeded in removing from the crania of mammals in the College Museum a sufficient number of auditory ossicula to illustrate the characters of those bones in most of the important subdivisions of that class of vertebrated animals. Numerous additions from rare specimens have been obtained through the kindness of Sir Victor Brooke, Bart., Professor Parker, Professor A. H. Garrod, and other gentlemen.

These observations are now brought forward with the object of demonstrating how far the characteristics of the auditory ossicles of the different orders of the Mammalia accord with those distinctions throughout the whole organization which have assisted anatomists up to the present day in giving a definite position to each member of the class. Dr. Hyrtl has already published a well-known work on the Comparative

Anatomy of the Internal Auditory Apparatus of the Mammalia *, but the subject of the ossicula themselves has not been considered in that able treatise quite fully and precisely enough for the present purpose.

All the Mammalia are known to possess three of these small bones, named respectively the *malleus*, the *incus*, and the *stapes*; the two former are occasionally fused. Their form and characters in our own species may be regarded as fairly typical, so that it is advisable to commence the subject by considering the ossicula of *Homo*.

Speaking from the stand-point of comparative anatomy, we may describe the malleus of man as having a well-developed head somewhat compressed antero-posteriorly and expanded laterally. It rises considerably above its articular region, and bulges markedly in an outward direction. The articular surface lies on the posterior aspect of the head very obliquely, so that its external extremity lies much higher than the internal. It is generally spoken of by human anatomists as forming one single saddle-shaped facet; but on comparing it with the same surface in the malleus of most of the lower mammalia, say in a cat or a pig, it will be seen that it is in reality made up of two facets, much less distinct than in those animals, but more marked than in many monkeys. A very faint groove divides them, and runs in the very oblique long axis of the whole surface. The more internal and upper facet above this groove represents that which is almost completely superior in the lower animals, the more external, below the groove, corresponding to their lower facet. Both rise into a high vertical convexity about the middle of the surface, where the latter is a little contracted; their planes slope towards the groove, so that the articular surface appears concave, especially when viewed sideways.

The neck of the human malleus is constricted and shorter than in most of the Mammalia, though longer than in most of the Primates. On its extero-superior aspect is a sharp sigmoid ridge, beginning near the anterior border of the articular surface, and losing itself on the root of the manubrium. This ridge is of the same form, and probably represents the sharply curved entire neck of the malleus of most Carnivora and Ruminants and many other Mammalia, where a bony lamella extends from that portion as far as the processus gracilis. A very faint trace of that "lamina," as it may briefly be called, may be seen extending, in a fully developed fœtal human malleus, from the neck to the root of the processus gracilis; but it seems reasonable to infer that the stout, compact portion of the neck in front of the sigmoid ridge represents in a more solid form the lamina referred to. Close to the root of the manubrium, and on the inner side, a very faint eminence, to which the tensor tympani is attached, represents the processus muscularis of some other mammals. The well-known processus gracilis of the malleus of *Homo* is known to be to a great extent absorbed in the process of extra-

* Vergleichend-anatomische Untersuchungen über das innere Gehörorgan des Menschen und der Säugethiere. Prague, 1845.

uterine growth, and hence is far less stable than in animals where it is held together, speaking roughly, by the manubrium.

The manubrium is rather short, and forms with the neck an angle of about 140°. It is broader at the base than in the Simiidæ, and flattened laterally; still the sides are slightly convex. The extremity is slightly recurved and spatulate, and the processus brevis is very well developed.

The body of the incus in *Homo* is well developed and rather longer than deep vertically; the crura are very divergent, and the "processus brevis," or posterior crus, very high in the natural position of the bone, is stout and rather long; the long, slender "processus longus" is gently curved, and bears a small os orbiculare or Sylvian apophysis rather firmly seated on a not very thin pedicle.

The stapes of man is noted for the great width of its aperture, although there is no canal between its crura as in many lower animals. The head is proportionally rather small, and the anterior of the two slender crura is the straightest. The footplate is necessarily wide horizontally, but rather narrow vertically; its outline is reniform, the upper border being convex or arched, the lower is slightly concave in the middle. Its posterior extremity is blunter than the anterior, and it is somewhat convex towards the vestibule.

Comparing the ossicula of *Homo* with those of the Simiidæ it appears :—

1st. That the ear-bones of *Homo*, *Troglodytes*, and *Simia* closely resemble one another.

2ndly. The malleus of *Hylobates* has greater affinities to the above genera than to the lower monkeys, but the incus and stapes are of a lower type.

3rdly. The ossicula of *Troglodytes niger* are altogether most like those of *Homo*; but in the form of the head and articular surface of the malleus *Simia* most approaches Man. The malleus of the gorilla is less human than the chimpanzee's, the outer segment of the articular surface being wide, whilst its manubrium more resembles that of *Simia*: but the incus and stapes of *T. gorilla* are very much like the same ossicula in *Homo*.

4thly. Taking the characters of these high animals into general consideration, we must conclude that they tend far more towards *Homo* than to the tailed Old-World monkeys.

The ossicula of the Cercopithecidæ possess several prominent characters, some of which are absent in certain genera; and they are most marked in *Macacus*. These peculiarities are principally :—in the malleus great shortness and great constriction of the neck, and a manubrium forming a very wide angle with the rest of the bone, possessing both a processus brevis and a processus muscularis, and well dilated at the extremity; in the incus a square or high and narrow body, and in the stapes extremely straight crura; this latter feature is constant.

Semnopithecus in its incus, and in the slight lateral compression of the well-developed head of the malleus, approaches the Simiidæ, but in the

characters of the neck and manubrium and of the whole stapes it resembles the other genera of this group. *Cercopithecus* comes next, the head of the malleus being well developed and prominent; but its incus is generally square-bodied or high and narrow. In *Colobus* and *Cercocebus* the head of the malleus is almost as flattened and comparatively ill-developed as in *Macacus*, and the incus is similar. In *Cynopithecus* and *Cynocephalus* the flattening of the head of the malleus and shortness of its neck and other characters already referred to are as marked as in *Macacus*, and there are no distinctions of the slightest importance between the ossicula of those three genera. In the shortness of the neck and form of the incus the Old-World monkeys resemble or tend more towards the Cebidæ than to Man.

The Platyrrhini differ considerably in their ear-bones from the Old-World monkeys, and chiefly in the complete or practical absence of the neck of the malleus in all genera excepting *Ateles*, and the peculiar shape of the neck in that genus. The Hapalidæ have mallei which approach in type the corresponding ossicle in the Lorises (Nycticebidæ), and the stapes, by the partial fusion of its crura, reverts to a condition frequent among the Edentata and Marsupialia. Such fusion may be seen both in *Hapale* and *Midas*, but is not constant.

Whilst *Ateles* differs from all the other Cebidæ in its malleus, *Cebus* closely resembles that genus in having a similar incus; but in the absence of neck to the malleus it rather resembles *Mycetes*. In the high narrow incus *Pithecia* agrees with *Mycetes*.

Among the Lemuridæ the ossicula of the Galagos, Nycticebidæ, and *Propithecus* differ hardly at all from the type of the smaller Cebidæ. In *Lemur* the neck of the malleus, and often a trace of the processus brevis mallei, reappears; and in that genus and the Indrisinæ there is a bony canal between the crura of the stapes not observed in the Galagos and Lorises or in *Tarsius*. Whilst retaining certain points of resemblance to *Lemur*, the ossicula of *Chiromys* decidedly remind the observer of the same bones in certain Rodents, especially the Castoridæ and Sciuridæ.

Among the Carnivora the auditory ossicles of the Fissipedia differ completely in character from those of the Pinnipedia.

The ossicula of the terrestrial flesh-eaters bear on the whole a strong general resemblance to one another; still they present some interesting points of distinction in the more typical families. These distinctions are mostly to be found in the malleus. The presence of a lamina of thin bone between the neck of the malleus and the processus gracilis is a constant character, except in *Herpestes* and its allies; and there is always a processus muscularis for the tendon of the tensor tympani, except in the Bears, as Hyrtl has observed.

Putting aside the Ursidæ, which are at once readily distinguished from all other families by the absence of that process, the more typical divisions present certain salient distinctions in the malleus. In the cats and dogs the muscular process is long, slender, and curved; but in the

Canidæ the manubrium forms a bold curve with the concavity forwards, and its outer surface, towards the membrana tympani, is broad throughout; in the Felidæ this curve hardly exists, and the outer aspect is very narrow. In *Hyæna* and *Proteles* the processus muscularis of the malleus is very stout, blunt-pointed, and almost straight, and the manubrium is curved as in the dogs; its outer aspect is broader near the tip than at the base in the *Hyæna*, but broad throughout in *Proteles*. Hence the mallei of these animals are more canine than feline, particularly that of the Aard-wolf. Among the Canidæ themselves *Lycaon* most approaches the Hyænidæ in the stoutness of its processus muscularis. In all the above families, as well as in the Civets and in the *Cryptoprocta*, the incus is small with slender crura, the posterior almost as long as the processus longus, and the stapes is small and triangular.

The Procyonidæ, Æluridæ, Viverridæ, and Cryptoproctidæ resemble one another in the ill-development of the processus muscularis of the malleus, which, however, is never quite absent. The outer surface of the manubrium in those families is narrow, as in *Felis* and *Ursus*. In the *Ælurus* and the Procyonidæ, including *Bassaris astuta*, the incus has a very short processus brevis, as in the bear; in the Civets and *Cryptoprocta* that crus is well developed, as in the cats and dogs. From the above observations it follows that *Cryptoprocta* is more Feline than Canine, and more Viverrine than Feline, in the character of its malleus.

The weasels and the other Mustelidæ are known from the rest of the Fissipedia by the extreme narrowness of the lamina of the malleus, and the very wide angle which the rather short manubrium forms with the neck. The processus muscularis of the malleus is as well developed as in the cats and dogs; but the Mustelidæ exceed the bears and the Procyonidæ in the extreme ill-development of the posterior crus of the incus. In the smaller weasels the base of the stapes is generally bullate.

In the genera *Herpestes* and *Suricata* the malleus differs in form from the type existing in the other Viverridæ and the rest of this suborder. The head of that ossicle is more developed, but there is hardly a trace of a lamina. The processus muscularis is not situated on the neck close to the root of the handle as in the other Carnivora, but on the inner edge of the manubrium itself. The incus is of the form seen in *Felis*, *Viverra*, and *Canis*, not of the ursine type.

The ossicula of the Pinnipedia are large, well marked, and readily distinguishable in the different genera.

The Otariidæ are exceptional in having very small ossicula, but they are of dense consistence as in the true seals. The malleus has a head which is concave anteriorly, and the articular surface is of the same prominent character as in *Phoca*. The neck is constricted, and the manubrium rather longer than in the other Pinnipedia. The incus differs from that of all other members of this suborder; in its non-divergent posterior crus and its long, far-reaching slender processus longus it is

almost arctoid. On the other hand, the stapes has oftener fused crura without any aperture (as in the dolphins) than in the Phocina.

The walrus possesses a malleus with a head much like that of *Phoca*; but the articular surface is less abnormal in character, and above all the manubrium is extremely short. The incus is phocine; but in the stapes *Trichechus* approaches *Macrorhinus*.

The ossicula of *Macrorhinus* differ very markedly from those of *Cystophora*; in the malleus the former resembles *Stenorhynchus*, whilst in the stapes it is more like *Trichechus*. The stapedes of *Stenorhynchus* and *Cystophora* are more of the *Phoca* type. In the Phocinæ alone is seen a second articulation between the malleus and incus, and this feature is not constant in every species.

Among the UNGULATA the laminated type of malleus prevails; the processus muscularis is rarely quite obsolete, but seldom very long. The incus is very variable in form, and bears important distinguishing features in certain families. A quadrilateral form of stapes, due to great breadth of the head of that ossicle, occurs very frequently.

Among the Perissodactyla *Equus* in its malleus least resembles the remainder of the whole order, the head of that bone being well developed above the level of the articular surface as well as anteriorly, and the lamina is almost obsolete. In the Rhinocerotidæ (where all the ossicula are proportionally very small) and in the Tapirs the malleus has a narrow lamina, not simply extending between the head and the processus gracilis, but running forwards to the very extremity of the latter. All these animals have perfectly triangular stapedes, differing from the form almost constant in the larger Artiodactyla.

In the Tylopoda the malleus resembles that of the pig and its allies in the great anterior development of its head; but unlike those animals the articular surface of that ossicle is wide and shallow, as in the Rhinoceros on the one hand and the larger Ruminants on the other; but it most resembles *Rhinoceros* in the ill-development of the processus muscularis mallei and in the triangular form of the stapes.

In the Suidæ, in *Hippopotamus* and *Phacochœrus*, the head of the malleus is greatly produced forwards with a rather deep articular surface; the lamina and the processus muscularis are well developed. The incus has a typical character; the body is very square in form, with the crura short, especially the posterior. The form of the articular surface in *Hippopotamus* and in large specimens of *Phacochœrus* differs from the same in the pigs. *Hippopotamus*, though so large, has a triangular stapes, whilst that bone is quadrilateral in the Suidæ, as in the ox and adult sheep.

In *Tragulus* the malleus is indistinguishable from those of many small antelopes and deer, the head not being produced forwards as in the pigs; whilst the incus retains to perfection the square body and short posterior crus of the Suidæ.

The Bovidæ, Antilocapridæ, Camelopardalidæ, and Cervidæ are re-

markable for the strong resemblance which the ossicula of the adults of the smaller species bear to those of the mature fœtus or young of the larger members of those families. The head of the malleus is always ill-developed, the processus muscularis always present, and the manubrium is frequently very long. In the adult *Bos* the great shallowness of the articular facet of the malleus, the bold curve of the manubrium, the very high and well-developed body of the incus with its long and divergent processus brevis, and the quadrilateral form of the stapes are all very distinctive; on the other hand, in the adult *Ovis* the articular surface is deeper and the facets less level than in *Bos*, and the manubrium is almost straight; nor is the body of the incus so developed, although the posterior crus is very long. The stapes is nearly as quadrilateral in a large adult *Ovis aries* as in *Bos*, but it often remains triangular in small sheep.

In the lamb or fully developed fœtal sheep the articular surface of the malleus is still deeper cut and the facets more prominent than in the adult; the incus has a shallow body and the stapes is quite triangular. Most of the remaining Bovidæ imitate, in the ossicula of the adults, the types of the ox, the sheep, or the lamb. Thus in the wild sheep, the goats, and many small antelopes, as *Nemorhœdus*, *Oreotragus*, and *Saiga*, the ossicula much resemble those of the young *Ovis aries*; *Gazella* and its allies are more bovine in the type of the malleus, whilst the stapes is generally triangular, even in the adult; the incus appears quite transitional between *Ovis* and *Bos*, the body being almost square; indeed as the posterior crus is sometimes rather short in these antelopes, that ossicle approaches the pig and chevrotain type. *Nanotragus*, *Cephalophus*, *Neotragus*, *Tetraceros*, and *Nanohœdus* are also balanced in characters between *Bos* and *Ovis*.

Kobus, *Tragelaphus*, *Alcelaphus*, and *Catoblepas* lean more, in the general characters of the auditory ossicles, towards *Bos*; the gnus are remarkable for the great length of the manubrium, which is straighter than in the ox. In the adult *Ovibos* and *Anoa* the ossicula are slightly more calf-like than ox-like.

The ossicula of *Camelopardalis* are remarkably like those of *Bos*, and undergo similar changes during the growth of the animal. In *Antilocapra* the ossicula have not strongly defined characters, although perhaps they most resemble those of the medium-sized antelopes. The quadrilateral form of stapes allies it to *Ovis* or *Bos*; the incus, as in *Gazella*, may either be considered intermediate between the form in those two domesticated ruminants, or (as the posterior crus is not long) to relate the prong-horn antelope to the Chevrotains.

In the Cervidæ the malleus always retains in the adult the characters seen in the fully developed fœtal ox, the articular surface having well-marked facets, and the manubrium being almost straight. The processus muscularis is large in the genus *Cervus*. The body of the incus is always

shallow, with an even, stouter, and more divergent posterior crus than in other ruminants. The stapes remains triangular in full-grown large deer. *Moschus* is quite cervine in its ossicula, the shallow body and thick, long, divergent posterior crus of the incus being very different from the square-bodied incus with short crura seen in the Tragulidæ.

Among the RODENTIA we find great variety in the form of the auditory ossicula in different families, as is also the case among the Insectivora. Nearly every type of malleus may be observed among the various subdivisions of the order, such as the large-headed, distinctly necked form of the higher Primates, the neckless variety of the lower monkeys, the laminated type of the ruminants and terrestrial carnivora, and the fused condition of the malleus and incus of the guinea-pig and its allies. The stapes, too, varies, being sometimes large in proportion to the size of the animal, in other cases very small in large species.

The most constant character in the rodent's malleus is the broad, laterally flattened manubrium, with a processus muscularis on its inner edge far from the neck of the ossicle, which may be said to present three prevailing types—the neckless form in the squirrels, the laminated variety of the rats, and the malleo-incudal fusion of the Hystricidæ. The incus varies little, and its processus brevis is always shorter than the anterior crus, and but little divergent. A bony canal between the crura of the stapes is frequent in several families.

Classifying the animals intermediate in the character of the malleus between the genus *Sciurus*, where it is neckless without a trace of any processus brevis, and *Castor*, where both neck and process exist, the genera will be found to run as nearly as possible in the following order:—*Sciurus*, *Anomalurus*, *Marmotta*, *Tamias* and *Spermophilus*, *Pteromys*, *Myoxus* and *Castor*, the separation of the head from the manubrium becoming more and more apparent in each of these genera towards the last; but, taking other points into consideration, *Anomalurus* should be placed after the ground-squirrels, having a small stapes with crura not very divergent, as in the Hystricidæ; and *Marmotta* separates itself from other sciuroid rodents by the peculiar form of its head, which is extremely flattened laterally and projects above the articular surface. In all the above rodents, except *Anomalurus*, the stapes is large, with wide, thin, divergent crura; and an intercrural bony canal exists in most species.

In the Muridæ the malleus has a well-formed lamina and a manubrium rather broad at the base. The former peculiarity is most marked in *Mus*, *Hapalotis*, *Hydromys*, and their allies, where an orbicular process standing out from the front of the neck is a frequent feature, and appears identical with a similar projection in the shrews, and is probably an extreme development of the sharp angular protuberance seen in the malleus of the badger, and in that from a *Bassaris* in the College collection. In *Fiber* the lamina is smaller and the manubrium broader than

in *Mus*, so that the malleus more resembles that of *Lepus*. The stapes of this family has generally long, slender, and not very divergent crura, and the intercrural canal is wanting.

In the Hystricidæ the great feature is the ankylosis of the malleus to the incus, already well known to zoologists. It is almost invariable in the adults of that family. The manubrium is very broad, and the inner edge above the processus muscularis is very thick. The varieties among the different genera are trifling: the head of the malleus is produced forwards to an extreme degree in *Aulacodus*, *Capromys*, and particularly in *Chinchilla*, but less so in the porcupines and the agouti. The stapes is always proportionally small, with stout and not very divergent crura; a bony intercrural canal occurs in many genera, but is an inconstant feature in individual specimens of the same species.

As the ankylosis of the two outer ossicles occurs in *Dipus*, whilst the head of the malleus remains small and unproduced, and that bone possesses a wide lamina, it must be considered intermediate, as far as those little bones are concerned, between the Muridæ and Hystricidæ.

As occurs in other orders, the fossorial members of the Rodentia present great peculiarities in their ear-bones. In *Geomys* the malleus somewhat resembles that of *Marmotta*; the stapes is remarkable for the large bulla on its base. *Rhizomys* and *Ellobius* approximate most to the rats, *Bathyergus* to the Hystricidæ, which it exceeds in the degree of fusion of the malleus to the incus, which latter bone, however, differs in form from the same in that family. In *Spalax* the malleus approaches the more central type of *Castor* or *Lepus*; but the stapes is of a very unique type, somewhat similar to that of *Chrysochloris*, except that one crus is quite straight and very divergent.

In both species of ELEPHANT the large ossicula appear rather like the modified ear-bones of certain rodents than like any ungulate. There is neither the lamina or long manubrium mallei, nor the thick and divergent processus brevis incudis, nor the quadrilateral stapes frequent among the large Ungulata; on the other hand the short, broad-based manubrium, the thin, short, and hardly divergent processus brevis of the incus, and the wide intercrural aperture of the stapes are characteristic in *Elephas* and common among the Rodentia.

In the HYRACES the ear-bones bear a slight affinity to those of the horse, but none of any importance to the common types among the Ungulata or Rodentia.

The ossicles of the remaining groups of the Mammalia will be described in a subsequent communication.

IV. "On two new Vanadium Minerals." By H. E. ROSCOE, F.R.S. Received May 10, 1876.

No. 1.—The first of these remarkable minerals contains 28 per cent. of vanadium pentoxide. It was forwarded to me by Dr. James Blake,

of San Francisco, with the following statement:—"I discovered it in a gold-mine, and it serves as the matrix for the gold. It occurs in small bunches, filling cavities in a schistose porphyry."

The mineral occurs in radiating and foliated talc-like masses, greenish grey in bulk, and light greenish yellow when seen in fragments, forming a grey powder. Its lustre is subvitreous to fatty. In mass it is opaque, but is translucent in thin splinters. Its hardness is about equal to that of talc; its specific gravity is 2·902.

Blowpipe reactions.—When heated in thin splinters the mineral fuses easily to a black non-magnetic bead. In a closed tube a little water is given off. With borax and microcosmic salt the reactions of vanadium and silica are obtained. It is only partially attacked by acids.

Composition.—Two complete analyses of the mineral have been made with the following results:—

	I.	II.	Mean.
Silica	41·25	..	41·25
Vanadium pentoxide	28·85	28·36	28·60
Alumina	14·34	13·94	14·14
Iron sesquioxide	1·04	1·23	1·13
Manganese sesquioxide	1·45	·85	1·15
Lime	·61	·62	·61
Magnesia	1·96	2·06	2·01
Potash	8·25	8·87	8·56
Soda	·72	·92	·82
Water	·94	1·22	1·08
Moisture	2·12	2·42	2·27
	101·53		101·62

The following formula approximately represents the composition of this mineral, a portion of the alumina being replaced by the sesquioxides of iron and manganese, and a portion of the potassium oxide by lime, magnesia, and soda:—

$$2(Al_2O_3 \cdot V_2O_5) + 2K_2O \cdot 9SiO_2 + H_2O,$$

or $4AlVO_4 + K_4Si_9O_{20} + H_2O$.

Thus we have:—

	Calculated.	Found.
Silica	41·18	40·38
Vanadium pentoxide	27·63	28·00
Alumina	15·59	15·32
Potassium oxide	14·24	15·24
Water	1·36	1·06
	100·00	100·00

Vanadium and aluminium salts cannot be separated by fusion with sodium carbonate and subsequent precipitation by ammonium chloride, as some of the alumina is thrown down as aluminium vanadate together with the ammonium vanadate.

The vanadate of aluminium and the ferric oxide precipitated together by acetate of ammonium are separated by fusion with sodium carbonate and subsequent boiling with water. The vanadic acid in the acidified solution is then reduced by sulphur dioxide, the liquid well boiled, and the vanadium estimated volumetrically by means of potassium permanganate. The larger portion of the vanadium remains in solution after the addition of ammonium acetate, and this is precipitated as lead metavanadate by the addition of lead acetate solution. The lead salt is dissolved in nitric acid, and the lead thrown down by sulphuretted hydrogen, the filtrate on evaporation yielding pure vanadium pentoxide, which after ignition is weighed. This method was likewise made use of for the separation of vanadium in the portion of mineral which served for the estimation of the alkalies.

A direct assay of the vanadium was made by heating 0·5105 grm. of the mineral with strong sulphuric acid, diluting with water, reducing with a current of sulphur dioxide, and titrating with a standard solution of permanganate of 1 cub. centim. = 0·00534 grm. of metallic iron. Of this solution 17·3 cub. centims. were needed; this corresponds to 29·5 per cent. of vanadium pentoxide, the change of oxidation being from V_2O_4 to V_2O_5. From this must, however, be deducted the amount of vanadium pentoxide equivalent to 1·13 per cent. of ferric oxide; this leaves 28·21 per cent. of V_2O_5, an amount closely approximating to that (28·6) obtained by analysis.

Dr. James Blake, in his communication referred to above, states:—"At the suggestion of my friend Professor Gibbs, I propose to name the mineral Roscoelite, should the name not be already appropriated."

No. 2.—The second vanadium mineral, to which I propose to give the name of Mottramite, occurs as a crystalline incrustation on Keuper sandstone found at Alderley Edge and at Mottram St. Andrew's, in Cheshire, and at other localities.

The incrustation is usually very thin; but occasionally it becomes 3 or 4 millimetres in thickness, and in one or two cases masses almost the size of a walnut have been found. Sometimes the incrustation has a black velvety appearance, and consists of numerous extremely small crystals, so minute that their form has not yet been ascertained. More commonly, however, the mineral possesses a compact character. The crystals are black by reflected light, but very thin particles are subtranslucent and transmit yellow light. The compact mineral is purplish brown and opaque. The lustre of the crystals is resinous. Streak yellow. Hardness of compact portion about that of calc-spar. Specific gravity 5·894.

Behaviour before the blowpipe.—In the closed tube a little water is evolved on heating, and the mineral fuses very easily. On asbestos it fuses easily and slightly tinges the flame green. Heated on charcoal with sodium carbonate in reducing flame, a yellow incrustation and grey malleable bead are obtained. On dissolving the bead in nitric acid and

adding ammonia in excess, a blue solution is obtained. Heated with borax on platinnm wire in the oxidizing flame the bead is yellow when hot, then becomes green, and when cold is blue. In the reducing flame the borax bead is bluish green, with a red skeleton of reduced copper.

Analysis.—Two complete analyses of this mineral, freed as much as possible from the matrix, gave the following results:—

	I.	II.	Mean.
Vanadium pentoxide	16·78	17·49	17·14
Lead oxide	50·49	51·45	50·97
Copper oxide............	19·72	18·48	19·10
Oxides of Fe, Zn, Mn	2·52	2·52	2·52
Lime	2·61	1·64	2·13
Magnesia	0·37	0·16	·26
Water	3·63	3·63	3·63
Moisture	0·22	0·22	0·22
Silica....................	0·87	1·25	1·06
	97·21	96·84	97·03

Taking the oxides of vanadium, copper, and lead, together with the water, and omitting the small quantities of the other constituents, the proportions are those required by the following formula:—

$$(Pb\,Cu)_3\,V_2\,O_8 + 2\,(Pb\,Cu)\,(OH)_2.$$

Thus:—

	Calculated.	Found.
Vanadium pentoxide	18·74	18·87
Copper oxide	20·39	21·02
Lead oxide................	57·18	56·12
Water	3·69	3·99
	100·00	100·00

As this mineral contains no aluminium, the determination of vanadium is made by fusing the mass, after separation of copper and lead, with sodium carbonate, treating the fused mass with water, and precipitating the soluble sodium vanadate as ammonium salt.

In addition to the above constituents the mineral contains traces of arsenic, cobalt, and sodium.

Mottramite is interesting as forming the third term in a second (doubtless) isomorphous group of phosphates, arsenates, and vanadates corresponding to the well-known pyromorphite, mimetesite, and vanadinite group. The new group is:—

Dihydrite .... $Cu_3\,P_2O_8 + 2\,Cu\,(OH)_2$.
Erinite ...... $Cu_3As_2O_8 + 2\,Cu\,(OH)_2$.
Mottramite.... $(Cu\,Pb)_3V_2O_8+2\,(Cu\,Pb)\,(OH)_2$.

The crystalline form of these three minerals remains yet to be determined.

V. "The Action of Light on Selenium." By Prof. W. G. Adams, F.R.S., and Mr. R. E. Day. Received May 18, 1876.

(Abstract.)

The paper contains an account of a series of experiments which have been carried on during the past year, and which have had for their object the investigation of the electrical behaviour of selenium, especially as regards its sensitiveness to light. The first part contains a short summary of the results obtained by Professor Adams, which have been communicated to the Society *.

It has been already shown that the action is due principally, if not entirely, to those rays of the spectrum which are luminous, and that the ultra-red or the ultra-violet rays have little or no effect; also that the intensity of the action depends on the illuminating power of the light, being directly as the square root of that illuminating power.

It was also observed that with the same piece of selenium at the same temperature, the resistance diminished as the battery-power was increased. Also it was found that the electrical resistance of the rod of selenium was different for currents going through it in opposite directions. Thus if two platinum wires be melted into the selenium at two points, A and B, and the resistance of the selenium be balanced by the Wheatstone's bridge arrangement, the positive pole of the battery being connected to the electrode A, then on reversing the current so that the negative pole of the battery was now connected to the electrode A, the numerical value of the balancing resistance was always found to be different from that previously obtained.

If the electrical conductivity of selenium followed the ordinary law of metallic conduction this would not be the case; and hence it seemed probable that a careful investigation of these points would lead to some important results.

In the experiments recorded in this paper, the objects we have had especially in view have been:—

(i) To examine the character of the electrical conductivity of selenium when kept in the dark.

(ii) To determine whether light could actually generate an electric current in the selenium.

Several pieces of selenium were prepared as follows:—A small piece varying from a quarter of an inch to an inch in length was broken off a stick of vitreous selenium. A platinum wire was then taken and bent round into a small ring at one end, and the remainder of the wire turned up at right angles to the plane of this ring. The rings of two such wires were then heated in the flame of a spirit-lamp, and pressed into the ends of the little cylinder of selenium,

* See 'Proceedings,' vol. xxiii. p. 535, and vol. xxiv. p. 163.

thus forming platinum electrodes. The whole was then annealed. After annealing copper wires were soldered on to the platinum electrodes, and the selenium was then inclosed in a piece of glass tube, the electrodes being passed through corks fixed at the ends of the tube. A numbered label was then attached to one of the electrodes, and this was then always described as the "marked" electrode.

The method of annealing which we have found to give the best results is very simple. A large iron ball is heated to a bright red heat, and then placed in a large iron bowl of sand; the sand is then heaped up all over the ball, and left for an hour. The ball is then taken out, and the selenium, wrapped up in paper, is put into the hot sand and left there for twenty-four hours. On removing it from the sand its appearance has generally changed from a bright glassy character to a dull slate-coloured one; and when this is the case its conductivity is generally very good.

In most of our experiments it was important to know what was the *direction* of the current in any particular case, and we therefore decided to call those currents direct or positive currents when the positive electrode of the battery was connected with the *marked* electrode of the selenium plate under examination. In order to be able to reverse the current with respect to the selenium without affecting any other portion of the circuit, the ends of the wire electrodes of the selenium were made to dip into two little mercury cups fixed on a plate of ebonite, and then were connected to the binding-screws of the Wheatstone bridge arrangement. Thus by reversing the position of the electrodes the direction of the current through the selenium was reversed. The positive direction of the current was always determined at the commencement of each series of experiments by means of a delicately suspended magnetic needle.

A few preliminary experiments were made to determine whether the change of resistance with change of direction of the current had any connexion with the position of the selenium or the direction of the current with respect to the magnetic meridian. No such connexion was found to exist.

From the results obtained from a great many experiments made to determine the diminution of resistance with increased battery-power, and the change of resistance with a change of the direction of the current, the following conclusions were drawn:—

(1) That on the whole there is a general diminution of resistance in the selenium as the battery-power is increased.

(2) The first current through the selenium, if a strong one, causes a permanent *set* of the molecules, in consequence of which the passage of the current through the selenium during the remainder of the experiments is more resisted in that direction than it is when passing in the opposite direction.

(3) The passage of the current in any direction produces a *set* of the

molecules which facilitates the subsequent passage of a current in the opposite, but obstructs one in the same direction. Hence when two currents are sent through successively, after a very small interval, in the same direction, the resistance observed in the second case, even with the higher battery-power, is often equal to or greater than it was before.

The results of these experiments seeming to indicate that the conductivity of selenium is electrolytic, a number of experiments were undertaken in order to discover whether after the passage of an electric current through a piece of selenium any distinct evidence of polarization could be detected. It was then found that, after passing the current from a voltaic battery for some time through the selenium, and after having disengaged the electrodes from the battery and connected them with a galvanometer, a current, in some cases of considerable intensity, in the opposite direction to that of the original battery-current, passed through the galvanometer. This proved that the passage of the battery-current sets up polarization in the selenium.

All the results hitherto described were obtained with the selenium kept in the dark.

We then tried to discover whether on exposing the selenium to light during the passage of the polarization-current any change in the intensity of that current would be produced: we found that in several cases there was a distinct change; in most instances the action of the light assisted the passage of the current; but in one case we found that the effect of light was not only to bring the deflection of the galvanometer-needle down to zero, but also to send it up considerably on the other side.

Here there seemed to be a case of light actually producing an electromotive force within the selenium, which in this case was opposed to and could overbalance the electromotive force due to polarization.

The question at once presented itself as to whether it would be possible to *start a current in the selenium merely by the action of light.* Accordingly the same piece of selenium was connected directly with the galvanometer. While unexposed there was no action whatever. On exposing the tube to the light of a candle, there was at once a strong deflection of the galvanometer-needle. On screening off the light the deflection came back at once to zero.

This experiment was repeated in various ways and with light from different sources, the results clearly proving that by the action of light alone we could start and maintain an electrical current in the selenium.

All the pieces of selenium hitherto used had repeatedly had electrical currents passing through them, and it therefore seemed desirable to examine the effect of exposure to light on pieces of selenium which had never before had an electrical current sent through them.

Accordingly three pieces were prepared, as nearly alike as possible, and were annealed. Two of them were found on trial to be sensitive to light—that is to say, light impinging on them produced an electrical current. The third piece, however, showed no signs of sensitiveness. Hence it appears that three pieces which were made up from the same stick, which are of the same length and were annealed at the same time, may, owing to some slight difference in their molecular condition, be very different as to their relative sensitiveness to the action of light.

In the experiments by which the above results were obtained, the piece of selenium under examination had always been exposed as a whole to the influence of the light, so that it was not possible to tell whether any one part of a piece was more sensitive than any other.

In order to examine into this point more fully, we used the lime-light, and then by means of a lens the light was brought to a focus on the particular portion of the selenium plate which was to be tested. A glass cell containing water, and having parallel sides, was interposed in the path of the beam, so as to assist in absorbing any obscure heat-rays.

The results of these experiments proved conclusively the following points:—

(1) That pieces of annealed selenium are in general sensitive to light, *i. e.* that under the action of light a difference of potential is developed between the molecules which under certain conditions can produce an electric current through the substance.

(2) That the sensitiveness is different at different parts of the same piece.

(3) That in general the direction of the current is from the less towards the more illuminated portion of the selenium, but that owing to accidental differences in molecular arrangement this direction is sometimes reversed.

The currents produced in the selenium by the action of light do not resemble the thermoelectric currents due to heating of the junctions between the platinum electrode and the selenium; for in many cases the current produced was most intense when the light was focused on points of the selenium not coinciding with the junctions; also the current was produced suddenly on exposure; and on shutting off the light the needle *at once* fell to zero: the gradual action due to gradual cooling was entirely wanting.

When the light fell upon a junction, the current passed from the selenium to the platinum through the junction, which is not in accordance with the place assigned to selenium in the thermoelectric series of metals.

Experiments were next undertaken in order to examine what effect would be produced on the strength of a current which was passing

through a piece of selenium in the dark when a beam of light was allowed to fall upon it.

The results obtained from these experiments were as follows:—

With pieces of selenium of low resistance and with a weak current passing through them—

(1) When light falls on the end of the selenium at which the current from the positive pole of the battery is entering the metal it *opposes* the passage of the current.

(2) When light falls on the end of the selenium at which the current is leaving the metal it *assists* the passage of the current.

With pieces of selenium of a high resistance we found that in all cases the action of light tended to facilitate the passage of the battery-current, whichever was its direction.

We also found that in those pieces which appeared so little sensitive to light that no independent current was developed in them by exposure, yet when a current due to an external electromotive force was passing through them, the exposure to light facilitated the passage of the current.

The results of the experiments described in this paper furnish a possible explanation of the character of the action which takes place when light falls upon a piece of selenium which is in a more or less perfect crystalline condition.

When a stick of vitreous selenium has been heated to its point of softening, if it were possible to cool the whole equally and very slowly, then the whole of the molecules throughout its mass would be able to take up their natural crystalline positions, and the whole would then be in a perfectly crystalline state, and would conduct electricity and heat equally well throughout its mass. But from the nature of the process it is evident that the outer layers will cool the most rapidly, and we shall have, in passing from the outside to the centre, a series of strata in a more and more perfect crystalline condition.

Light, as we know in the case of some bodies, tends to promote crystallization, and, when it falls on the surface of such a stick of selenium, probably tends to promote crystallization in the exterior layers, and therefore to produce a flow of energy from within outwards, which under certain circumstances appears, in the case of selenium, to produce an electric current.

The crystallization produced in selenium by light may also account for the diminution in the resistance of the selenium when a current from a battery is passing through it, for in changing to the crystalline state selenium becomes a better conductor of electricity.

VI. "On the Application of the Principle of Reciprocity to Acoustics." By Lord RAYLEIGH, F.R.S. Received May 27, 1876.

In a memoir published some years ago by Helmholtz (Crelle, Bd. lvii.) it was proved that if a uniform frictionless gaseous medium be thrown into vibration by a simple source of sound of given period and intensity, the variation of pressure is the same at any point B when the source of sound is at A as it would have been at A had the source of sound been situated at B, and that this law is not interfered with by the presence of any number of fixed solid obstacles on which the sound may impinge.

A simple source of sound is a point at which the condition of continuity of the fluid is broken by an alternate introduction and abstraction of fluid, given in amount and periodic according to the harmonic law.

The reciprocal property is capable of generalization so as to apply to all acoustical systems whatever capable of vibrating about a configuration of equilibrium, as I proved in the Proceedings of the Mathematical Society for June 1873, and is not lost even when the systems are subject to damping, provided that the frictional forces vary as the first power of the velocity, as must always be the case when the motion is small enough. Thus Helmholtz's theorem may be extended to the case when the medium is not uniform, and when the obstacles are of such a character that they share the vibration.

But although the principle of reciprocity appears to be firmly grounded on the theoretical side, instances are not uncommon in which a sound generated in the open air at a point A is heard at a distant point B, when an equal or even more powerful sound at B fails to make itself heard at A; and some phenomena of this kind are strongly insisted upon by Prof. Henry in opposition to Prof. Tyndall's views as to the importance of "acoustic clouds" in relation to the audibility of fog-signals. These observations were not, indeed, made with the simple sonorous sources of theory; but there is no reason to suppose that the result would have been different if simple sources could have been used.

In experiments having for their object the comparison of sounds heard under different circumstances there is one necessary precaution to which it may not be superfluous to allude, depending on the fact that the audibility of a particular sound depends not only upon the strength of that sound, but also upon the strength of other sounds which may be heard along with it. For example, a lady seated in a closed carriage and carrying on a conversation through an open window in a crowded thoroughfare will hear what is said to her far more easily than she can make herself heard in return; but this is no failure in the law of reciprocity.

The explanation of his observations given by Henry depends upon the peculiar action of wind, first explained by Prof. Stokes. According to

this view a sound is ordinarily heard better with the wind than against it, in consequence of a curvature of the rays. With the wind a ray will generally be bent downwards, since the velocity of the air is generally greater overhead than at the surface, and therefore the upper part of the wave-front tends to gain on the lower. The ray which ultimately reaches the observer is one which started in some degree upwards from the source, and has the advantage of being out of the way of obstacles for the greater part of its course. Against the wind, on the other hand, the curvature of the rays is upwards, so that a would-be observer at a considerable distance is in danger of being left in a sound-shadow.

It is very important to remark that this effect depends, not upon the mere existence of a wind, but upon the velocity of the wind being greater overhead than below. A uniform translation of the entire atmosphere would be almost without effect. In particular cases it may happen that the velocity of the wind diminishes with height, and then sound is best transmitted *against* the wind. Prof. Henry shows that several anomalous phenomena relating to the audibility of signals may be explained by various suppositions as to the velocity of the wind at different heights. When the distances concerned are great, comparatively small curvatures of the ray may produce considerable results.

There is a further possible consequence of the action of wind (or variable temperature), which, so far as I know, has not hitherto been remarked. By making the velocity a suitable function of height it would be possible to secure an actual convergence of rays in a vertical plane upon a particular station. The atmosphere would then act like the lens of a lighthouse, and the intensity of sound might be altogether abnormal. This may perhaps be the explanation of the extraordinary distances at which guns have sometimes been heard.

The difference in the propagation of sound against and with the wind is no exception to the general law referred to at the beginning of this communication, for that law applies only to the vibrations of a system about a configuration of equilibrium. A motion of the medium is thus excluded. But the bending of the sound-ray due to a variable temperature, to which attention has been drawn by Prof. Reynolds, does not interfere with the application of the law.

An experiment has, however, been brought forward by Prof. Tyndall, in which there is an apparent failure of reciprocity not referable to any motion of the medium*. The source of sound is a very high-pitched reed mounted in a short tube and blown from a small bellows with which it is connected by rubber tubing. The variation of pressure at the second point is made apparent by means of the sensitive flame, which has been used by Prof. Tyndall with so much success on other occasions. Although the flame itself, when unexcited, is 18 to 24 inches high, it was

* Proceedings of the Royal Institution, January 1875; also Prof. Tyndall's work on Sound, 3rd edition.

proved by a subsidiary experiment that the root of the flame, where it issues from the burner, is the seat of sensitiveness. With this arrangement the effect of a cardboard or glass screen interposed between the reed and the flame was found to be different, according as the screen was close to the flame or close to the reed. In the former case the flame indicated the action of sound, but in the latter remained uninfluenced. Since the motion of the screen is plainly equivalent to an interchange of the reed and flame, there is to all appearance a failure in the law of reciprocity.

At first sight this experiment is difficult to reconcile with theoretical conclusions. It is true that the conditions under which reciprocity is to be expected are not very perfectly realized, since the flame ought not to be moved from one position to the other. Although the seat of sensitiveness may be limited to the root of the flame, the tall column of highly heated gas might not be without effect; and in fact it appeared to me possible that the response of the flame, when close to the screen, might be due to the conduction of sound downwards along it. Not feeling satisfied, however, with this explanation, I determined to repeat the experiment, and wrote to Prof. Tyndall, asking to be allowed to see the apparatus. In reply he very kindly proposed to arrange a repetition of the experiment at the Royal Institution for my benefit, an offer which I gladly accepted.

The effect itself was perfectly distinct, and, as it soon appeared, was not to be explained in the manner just suggested, since the response of the flame when close to the screen continued, even when the upper part of the heated column was protected from the direct action of the source by additional screens interposed. I was more than ever puzzled until Mr. Cottrell showed me another experiment in which, I believe, the key of the difficulty is to be found.

When the axis of the tube containing the reed is directed towards the flame, situated at a moderate distance, there is a distinct and immediate response; but when the axis is turned away from the flame through a comparatively small angle, the effect ceases, although the distance is the same as before, and there are no obstacles interposed. If now a cardboard screen is held in the prolongation of the axis of the reed, and at such an angle as to reflect the vibrations in the direction of the flame, the effect is again produced with the same apparent force as at first.

These results prove conclusively that the reed does not behave as the simple source of theory, even approximately. When the screen is close (about 2 inches distant) the more powerful vibrations issuing along the axis of the instrument impinge directly upon the screen, are reflected back, and take no further part in the experiment. The only vibrations which have a chance of reaching the flame, after diffraction round the screen, are the comparatively feeble ones which issue nearly at right angles with the axis. On the other hand, when the screen is close to

the flame, the efficient vibrations are those which issue at a small angle with the axis, and are therefore much more powerful. Under these circumstances it is not surprising that the flame is affected in the latter case and not in the former.

The concentration of sound in the direction of the axis is greater than would have been anticipated, and is to be explained by the very short wave-length corresponding to the pitch of the reed. If, as is not improbable, the overtones of the note given by the reed are the most efficient part of the sound, the wave-length will be still shorter and the concentration more easy to understand*.

The reciprocal theorem in its generalized form is not restricted to simple sources, from which (in the absence of obstacles) sound would issue alike in all directions; and the statement for *double sources* will throw light on the subject of this note. A double source may be thus defined:—Conceive two equal and opposite simple sources, situated at a short distance apart, to be acting simultaneously. By calling the two sources opposite, it is meant that they are to be at any moment in opposite phases. At a moderate distance the effects of the two sources are antagonistic and may be made to neutralize one another to any extent by diminishing the distance between the sources. If, however, at the same time that we diminish the interval, we augment the intensity of the single sources, the effect may be kept constant. Pushing this idea to its limit, when the intensity becomes infinite and the interval vanishes, we arrive at the conception of a double source having an axis of symmetry coincident with the line joining the single sources of which it is composed. In an open space the effect of a double source is the same as that communicated to the air by the vibration of a solid sphere whose centre is situated at the double point and whose line of vibration coincides with the axis, and the intensity of sound in directions inclined to the axis varies as the square of the cosine of the obliquity.

The statement of the reciprocal theorem with respect to double sources is then as follows:—If there be equal double sources at two points A and B, having axes A P, B Q respectively, then the *velocity* of the medium at B resolved in the direction B Q due to the source at A is the same as the *velocity* at A resolved in the direction A P due to the source at B. If the waves observed at A and B are sensibly plane, and if the axes A P, B Q are equally inclined to the waves received, we may, in the above statement, replace "velocities" by "pressures," but not otherwise.

Suppose, now, that equal double sources face each other, so that the common axis is A B, and let us examine the effect of interposing a screen near to A. By the reciprocal theorem, whether there be a screen or not, the velocity at A in direction A B due to B is equal to the velocity at B

* July 13.—I have lately observed that the flame in question is extremely sensitive to one of Mr. F. Galton's whistles, which gives notes near the limits of ordinary hearing.

in direction A B due to A. The waves received at B are approximately plane and perpendicular to A B, so that the relation between the velocity and pressure at B is that proper to a plane wave; but it is otherwise in the case of the sound received at A. Accordingly the reciprocal theorem does not lead us to expect an equality between the pressures at A and B, on which quantities the behaviour of the sensitive flames depends. On the contrary, it would appear that the pressure at A corresponding to the given velocity along A B should be much greater than in the case of a plane wave, and then the relative advantage of the position A would be explained.

It will be seen that if the preceding arguments are correct, Prof. Tyndall's experiment does not bear out the conclusions that he has based upon it with respect to the observations of the French Commission at Villejuif and Montlhéry. No acoustic clouds could explain the failure of reciprocity then observed; and the more probable hypothesis that the effect was due to wind is not inconsistent with the observation that the air (at the surface) was moving in the direction against which the sound was best heard.

Further experiments on this subject are very desirable.

VII. "On Phosphorus Pentafluoride." By T. E. Thorpe, F.R.S., Professor of Chemistry in the Yorkshire College of Science. Received June 6, 1876.

Arsenic trifluoride acts violently upon phosphorus pentachloride with the formation of *arsenic trichloride* and the liberation of a heavy fuming gas, which analysis shows to be *phosphorus pentafluoride*.

The production of the new gas may be represented by the equation

$$5\,AsF_3 + 3\,PCl_5 = 5\,AsCl_3 + 3PF_5.$$

Direct experiments have shown that the amount of arsenic trichloride actually produced in the reactions agrees closely with that demanded by this equation.

The accuracy of the formula was further controlled by determinations of the density of the gas. Three experiments made by two independent methods gave the numbers—

I.	62·98
II.	63·33
III.	63·39

The number demanded by the formula PF_5 is

63·0,

hydrogen being the unit.

Phosphorus pentafluoride is a colourless gas: it is incombustible and extinguishes flame; it is absolutely irrespirable even when largely diluted with air; it fumes strongly in moist air, and is rapidly decomposed by water, forming hydrofluoric and phosphoric acids. As it is nearly 4½ times heavier than air, it may be collected by downward displacement, and may be poured from vessel to vessel. It may be preserved in glass vessels over dry mercury for some time without much alteration; but its volume very gradually diminishes, and the glass after prolonged contact with the gas is found to be slightly corroded.

An attempt was made to liquefy the gas by compressing it in an Oersted's apparatus as arranged to show the condensation of the more readily liquefiable gases. Under a pressure of 12 atmospheres (which was the highest pressure the apparatus would safely bear) it showed no signs of change. When compared with the same initial volume of air, no deviation from Boyle's law was observed sufficiently marked to warrant the belief that the gas under this pressure was anywhere near its point of condensation.

Phosphorus pentafluoride experiences no apparent change on the passage of induction-sparks, either when pure or when mixed with oxygen or hydrogen. The character of the light emitted during the discharge is under investigation; the spectrum which it affords is exceedingly complicated.

Phosphorus pentafluoride combines immediately with ammonia-gas, forming a white solid body of the composition $2PF_5.5NH_3$. The gas is readily absorbed by an aqueous solution of ammonia, and the liquid on concentration yields a crystalline deposit consisting of a mixture of ammonium dihydrogen phosphate, $NH_4H_2PO_4$, and acid ammonium fluoride, $NH_4F.HF$.

Wurtz has conclusively shown that phosphorus pentachloride can actually exist in the gaseous state under diminished pressure and between certain narrow limits of temperature. On the other hand, the author has given reasons, derived from considerations of specific volume, for the supposition that phosphorus oxychloride, $POCl_3$, and phosphorus thiochloride, $PSCl_3$ (bodies which are frequently adduced to show its pentadicity), are in reality derivatives of triad phosphorus*.

The existence of the gaseous pentafluoride, taken in conjunction with the fact that it is perfectly stable, even at very high temperatures, is of great interest theoretically, inasmuch as this body unequivocally indicates the pentadicity of phosphorus.

* "Researches upon the Specific Volumes of Liquids.—I On the Atomic Value of Phosphorus," Proc. Roy. Soc. xxiii. p. 364.

VIII. "On Supersaturated Saline Solutions." By J. G. Grenfell, B.A., F.G.S. Communicated by C. Tomlinson, F.R.S. Received June 6, 1876.

In making experiments on the sensitiveness of supersaturated solutions to air and greasy surfaces, I was much annoyed by the solutions so frequently crystallizing on the removal of the cotton-wool, as this necessitated boiling the flask again and waiting till it was cool. I noticed that frequently part of the cotton-wool adhered to the mouth of the flask; and it struck me that in removing this some fibres must get detached and fall in, carrying with them in all probability crystals of the salt. I soon convinced myself that this was the case, and that cotton-wool is perhaps the worst material that could be chosen for covering these solutions. I now always use paper or tinfoil; and I find that these can be removed many times from the same solution without inducing crystallization. I then found that even the most sensitive solutions could be taken up in a clean glass tube and dropped on a clean glass plate without crystallizing; and that they will remain liquid exposed to the air for a very long time, often, in fact, till they dry up by evaporation in modified forms. Twenty drops on a plate give twenty experiments on the effect of air, clean and unclean surfaces, and evaporation; then the plate is cleaned, and more drops are taken from the original solution till this is used up. The trouble of boiling is thus reduced to a minimum, and the drops can be put upon all kinds of surfaces to test their activity. The slow growth of the modified salts can be watched for hours; and their forms are sometimes peculiar. Thus sulphate of soda often gives a single, square, flat pyramid, or a broad well-shaped prism, or occasionally small octahedra round the edge of the drop. The pyramids and prisms change to opaque white when touched, and are apparently the 7-atom salt; the octahedra do not change, and are evidently the anhydrous salt. This fact is interesting, from its supporting the view that it is the anhydrous salt which is in solution.

Or, again, a plate with drops may be dried over calcium chloride; and this sometimes modifies the results, as in the case of ammonia alum. This salt, when allowed to evaporate in air, generally forms a shining semitransparent film of greenish colour with a depression at the top, in which is often a circular opening, while inside small globular concretions of a dull, opaque, milky white colour are formed; these will remain moist inside for a couple of days or more. When touched with the normal salt, the whole drop becomes brilliant opaque white, quite dry, and apparently increases in volume, as the crust often breaks up and curls outwards.

This modified salt is apparently new. I put some drops over calcium chloride: no film was formed, but the drops crystallized very slowly in

the globular forms mixed with little, clear, flat, very thin pointed plates which reminded me much of a particular form of aluminium sulphate. When dry all the drops were brilliant opaque white, and retained a good deal of water.

Potash alum forms similar films and globular masses. The mother-liquor of the ammonia alum sometimes slowly deposits short, fine, silky needles with a faint milky tinge and small globular masses. I have only recently adopted the method of using drops, and have not much leisure for working; but the field is so wide, and the results already obtained have such an important bearing on the theory of the crystallization of these solutions, that I have ventured to put them forward in their present incomplete state.

The most commonly received theory is that of which M. de Gernez is the most prominent advocate—that only a crystal of the same salt causes crystallization, and that these are introduced by the air, which is a vast storehouse of crystals of all kinds.

The following experiments seem to support the crystal theory; but at the same time they clearly show that the quantity of salts present in the atmosphere is indefinitely less than we have hitherto been led to suppose, and, in fact, they bring that quantity down within the limits of ordinary probability.

1. Put drops of a very strong solution of sulphate of soda on a plate on my laboratory table; waved a newspaper over them for some time, producing a strong current of air: most of them did not crystallize, and one slowly dried up in octahedra. I have repeatedly of late boiled sulphuric acid in the laboratory, so that there can be no lack of sodium sulphate in the dust.

2. Drew a strong current of air over drops of sodium sulphate in a glass tube: inactive.

3. Drops of sodium sulphate put upon the leaves of many plants in my garden. They slowly evaporated, giving the 7-atom salt. The leaves were covered with dust, as the garden opens on to a road, and the weather has been hot and dry; we are not far from Bristol, so we might expect to find sulphates.

4. Carried sodium sulphate to an upper room; drops on the wash-hand stand, on the window-sill inside and out, on the iron bars outside: all inactive. Washed my hands and spread a drop with the finger on the window-sill, inside: inactive. Three drops crystallized on the mantlepiece, and one on the window-sill. Several drops on the window-frame evaporated as 7-atom salt.

5. Potash alum on a window-sill outside gave a modified film.

6. Sodium acetate put upon the cork of a large bottle which had stood for two years untouched in my laboratory. The drops were quite thick with dust, but remained liquid for more than 24 hours.

7. Other drops of the same put on the floor of the laboratory, on the

dusty corners of the shelves, on paper, on every place and kind of surface I could find: remained liquid in all cases.

8. Spread a number of drops of the same on a glass plate, covering nearly the whole of it. Made about half crystallize. Left them exposed for three days; they remained liquid, though the normal salt effloresces slightly.

9. Ammonia alum: many drops on a glass plate; they formed films by evaporation; made a good many crystallize, when they broke open, early in the day: carried them out in a high wind to the house of a neighbour, and brought them back; then late at night put a number of fresh drops on the plate, and several of them remained liquid all night.

10. Sodium carbonate is not affected by any surface in my laboratory. I have spread a drop over a dirty glass plate so as to cover a good many square inches, and it slowly evaporated, giving crystals. Drops on the floor, shelves, bottles, &c. of the laboratory invariably remain liquid.

I could give many other instances, but these are sufficient to show that the air does not ordinarily contain these salts, and that it does not readily catch them up and deposit them on all kinds of surfaces; and yet these salts are remarkably sensitive to crystals of the same kind. The effect of using cotton-wool is a good example of this. Another is this:—Touched a crystallized drop of sodium acetate with a pin; passed the pin repeatedly through my coat: active at once. After touching a crystal the finger needs to be washed carefully. Again, sodium sulphate crystallizes almost invariably on any dirty surface in my laboratory, and ammonia alum generally. Even the sodium acetate crystallizes at times when I am at work with the same salt close by.

Sodium sulphate crystallizes generally on a clean plate exposed in my laboratory as 10-atom salt, whilst if protected by an inverted beaker it dries up by evaporation, forming the modified salt. So, again, I have had two drops of sodium sulphate liquid all night, and both crystallize within ten minutes of my entering the room in the morning. In my bedroom, however, I left a test-tube containing this solution open all night with the pipette on the mantlepiece. In the morning the solution had not crystallized, while the end of the pipette was covered with a white incrustation, which was inactive in the liquid. The incrustation was again left to dry up, and then contained plenty of water, being evidently the 7-atom salt.

For sodium acetate and carbonate it is quite useless to have any cover on the flask or test-tube which contains them, and also for the sulphate in an ordinary room. Care must be taken that crystals are not formed near the mouth of the tube, so as to fall in; but that is the only precaution necessary. Carbonate of soda by evaporation becomes oily like sodium and potassium acetates. I have not yet investigated the composition of the films and crystals which these solutions deposit.

Normal sodium acetate when heated leaves a white mass which deli-

quesces, forming a strongly supersaturated solution. The anhydrous sulphate also forms a supersaturated solution when added to water, as De Coppet pointed out.

I touched a drop of the acetate with the point of a penknife; a little drop crystallized on the penknife, but the drop itself did not. I then repeatedly touched the surface of the drop rapidly with the solidified part and obtained a little rod, formed of separate layers and nearly $\frac{1}{2}$ inch long. At last the rod broke in the drop, which instantly crystallized. I have repeated this with carbonate of soda. The fact is interesting as showing how very local the crystallizing force is. Faraday had an idea that this force might possibly be transferred by wires; but I have poured out part of a solution which was crystallizing into a test-tube, where it remained supersaturated.

Professor Tomlinson has long maintained with great ingenuity the theory that the cause of crystallization in these solutions is adhesion. To a surface covered with a film of greasy matter the salt adheres, while the liquid does not, and therefore separation follows. I do not think that theory can be sustained in the presence of the following facts:—

1. Rubbed the finger on the palm of the hand, and took up solution of alum from a drop, and deposited on another part of the same plate: inactive.

2. Rubbed oil on the palm of the hand, and repeated: again inactive.

3. Smeared oil over a glass plate: inactive to drops of alum.

4. Rubbed oil on the finger; took up some sodium carbonate, and rubbed it hard on the plate: inactive.

5. Repeated this with sodium acetate.

The mere fact, however, that the salts are, as a rule, perfectly insensible to every kind of surface, wood, paint, paper, glass, and dust of all kinds, seems to me fatal to this theory.

A solution of one part of normal sodium sulphate in about six of sulphuric acid possesses some curious properties. This solution, which sets quite firm, can be kept for a week in an open beaker, so that the air apparently has no crystals to introduce; and yet when dropped on to a dirty surface in my laboratory it more often crystallizes than not. It is thus much more sensitive than an aqueous solution of sodium carbonate or acetate. The crystals are apparently a hydrate of the hyperacid salt $NaH_3(SO_4)_2$; and it is almost inconceivable that the dust should contain crystals of this salt. It is extremely deliquescent, and the excess of acid should certainly be taken up by the dust, and very often by the surface itself.

The solution sometimes crystallizes suddenly in the test-tube as though something had fallen in. The crystallized drops will not stand exposure to air for more than 30 minutes or so. Hence, although there is plenty of sulphuric acid in my laboratory, where I have often heated this solution, I find it very hard to believe that the salt exists in this form

in any part of the room. The normal salt and the anhydrous salt are without action on the solution. It crystallizes in a test-tube in fine stellate masses, with projecting points on all sides, as alum sometimes does; these ultimately coalesce. These crystals are composed of very fine parallel fibres like ferns, and are opaque white. It sometimes sets in long fibres, radiating from different points like aluminium sulphate. Owing to the fineness of the fibres it would be very difficult to free them from the mother-liquor.

My reason for believing them to be a hydrate is this:—In a beaker this solution gradually deposits clear crystals, varying from very fine needles to rhombic plates, prisms, and short, nearly globular, highly modified forms. These are formed near the top, and may perhaps be different hydrates. They are formed, however, at the same time, and at present I believe them all to be the hyperacid salt. Similar ones are formed by putting the normal salt in the 6 to 1 solution, and this remains liquid, sometimes dissolving the crystals. An opaque amorphous mass is formed at the same time, which appears to be hydrated, but it also is inactive. A mixture of two parts of acid to one of salt in a flask, when boiled to get rid of all water, sets firmly in a clear mass, in which the opaque variety makes no change. Then if a little water is added the salt turns opaque white wherever the water reaches; and this is entirely absorbed, the cake remaining quite dry.

If this is again melted it deposits clear prisms, leaving a little mother-liquor; but the opaque variety when introduced from the 6 to 1 solution causes the whole mass to set firmly opaque white and become quite dry. The opacity spreads slowly, and a kind of beard of fine crystals can sometimes be seen growing round the prisms at the edge. Lovely foliated films are often formed at the same time. The clear crystals are inactive in the 6 to 1 solution, while the opaque is active; and this is a clear proof of their identity. Solutions of intermediate strength between 2 to 1 and 6 to 1 often deposit in flasks the whole excess in clear crystals, which are sometimes inactive in the 6 to 1 in a test-tube. It is almost impossible to obtain these solutions supersaturated in flasks, though it may be done with the utmost facility in test-tubes. Out of many trials with one flask I only succeeded once by leaving it to cool on the sand-bath. In a test-tube they give the same forms as the 6 to 1. The variety of the forms in which these solutions crystallize is truly astonishing, according to the proportion of acid and salt, amount of water, and the temperature. A flask once gave the most exquisite little, flat, open flakes closely resembling snow-flakes; but I have not been able to reproduce them. In short the relations of these two substances to each other want working out thoroughly. A certain amount of acid added to the salt which is in excess gives a thin liquid, which will not crystallize, and a little fine white powder, the anhydrous salt. Two drops of acid in a test-tube half-full of solution cause drops to evaporate

on a plate in octahedra; and when the anhydrous is thrown down on heating the test-tube locally after crystallizing, it is redissolved, leaving, however, well-marked octahedra just before it all disappears.

The most curious property, however, of the 6 to 1 solution is this:— On a clean glass plate it can be spread out into a thin covering of the plate with the handle of a tooth-brush; then with the end of a glass rod scratch a letter hard on the plate, and the letter will come out at once in slowly growing crystals. The effect is certain with the right proportions, and is most striking, as a plate of any size can be used. Scratching has the same effect when the solution is placed on gold or copper, but not on platinum foil, lead foil, bone, gutta percha, or any soft substance. The effect is of course analogous to that of scratching on the ammonio-magnesic phosphate and on soda water in a clean tumbler. Mr. Tomlinson explains these by supposing that a partial vacuum is formed into which the salt and gas separate. I confess it seems to me more probable that the result is due to vibration. With the same solution of sodium sulphate in acid, but of different strength, scratching is inactive, and I have tried it in vain on many aqueous solutions.

I cannot see why the vacuum should not act equally on all; but it is easy to understand how the molecular vibrations of one unstable system should be affected by a particular set of vibrations, whilst those of another system should not. The results obtained thus far, then, are:—

1. Exposure to air and dust has no effect on some supersaturated solutions.

2. The sulphates are the most sensitive. Exposure of a clean glass plate for half an hour to the air of my laboratory caused nearly all the drops of sodium sulphate put upon it to crystallize at once, whilst the same plate recently cleaned is quite inactive.

3. Even the sulphates are unaffected by the dust of the open air and generally of ordinary rooms.

4. Anhydrous salts or modified salts, sometimes new, are produced by the spontaneous evaporation of the solutions in drops.

5. Drops can be rapidly touched on their surface with crystals of the same salt without crystallizing.

6. Greasy surfaces, whether films or lenses, have no effect.

7. The shape of the vessel has sometimes a material influence on the possibility of obtaining a supersaturated solution.

8. Air and dirty surfaces are active on salts which apparently cannot exist in air.

9. Scratching a hard surface will cause a particular solution to crystallize.

The crystal theory, modified as it now must be, seems on the whole the best explanation of the phenomena. The case of the hyperacid sodium sulphate, however, remains to be explained. If the crystal

theory is true, the order of sensitiveness of the solutions should be the order of comparative rarity of the salts; and this remains to be proved.

As to the cause of supersaturation, a good many facts seem to show that it is the anhydrous salt which enters into solution. The lower hydrates seem to be first formed, as in the case of sodium sulphate and the alums. In the case of the hyperacid sodium sulphate with two parts acid to one of salt, repeatedly boiled, it seems to be the anhydrous salt which is first deposited. When the aqueous solutions of sodium sulphate and the alums are made to crystallize, the modified salts become opaque white, while the hyperacid salt remains unchanged, and can be obtained unchanged by heating the opaque variety from the top so as to dissolve this, but not the anhydrous.

Against the theory that it is the anhydrous sodium sulphate in solution at low temperatures must be set the following fact.

Löwel, in his Tables of the solubility of the three forms of sodium sulphate, which are found in all our text-books, gives 412 parts of salt to 100 of water as the maximum solubility of the 10-atom salt; and this is the highest number for any of the three kinds. Now I have dissolved 600 parts of 10-atom salt in 100 of water at 37° C. without throwing down a trace of anhydrous. I then warmed it: at 45° a doubtful trace of anhydrous; at 51° very few; at 60° still very few; at 67° about as much as would lie on a little-finger nail; at 75° eight or ten times as much, the liquid nearly opaque; at 80° a large quantity; boiled, the salt thickly covered the bottom of a large flask.

Now here the solution at 60° practically retained the whole of the 6 oz. of salt to 1 of water, while according to Löwel it should have retained only $2\frac{1}{2}$ oz.

Then between 70° and 80° a sudden change takes place, and a large quantity is thrown down. This agrees so far with Löwel's Table, as, according to him, at 84° the whole of the excess was practically thrown down. This looks very much like dissociation taking place at that temperature; and that would involve the supposition that it was the 10-atom salt in solution before. The difference in our results springs from the different modes of working. Löwel always maintained a large excess of anhydrous present, whilst I added the salt in small portions, carefully avoiding throwing down any anhydrous. This is pretty easily done by keeping up a very rapid motion so as to prevent the liquid from getting heated too much at any point. It seems to me that in any case, as the six ounces fairly dissolved, the solubility of the 10-atom salt should be given in those proportions. Further experiments would, I have no doubt, give still higher figures.

In conclusion, I would remark that if the crystal theory of these solutions be accepted we have a test of great delicacy in these drops for the presence of the salts. Interesting experiments might be made as

to the power of air to disseminate crystals of a salt thrown into it in fine powder.

De Coppet has already remarked that the mass of a solution exerts some influence on its crystallization, and I have shown that the form of the vessel also has a decided effect. The effect again of different vibrations on different solutions is worth trying, as there seems to be no reason why the hyperacid sodium salt should be an exceptional case.

A good deal of work has yet to be done before we arrive at a satisfactory explanation of these obscure phenomena.

IX. "On some Elementary Principles in Animal Mechanics.—No. VIII. The Law of Fatigue." By the Rev. SAMUEL HAUGHTON, M.D. (Dubl.), D.C.L. (Oxon.), F.RS., Fellow of Trinity College, Dublin.

In my last paper (No. VII.) I illustrated the Law of Fatigue by experiments made in lifting weights varying in amount, without rest, at a fixed rate of motion; I shall now illustrate the Law by experiments made in lifting a fixed weight at varying rates of motion, without rest, as before.

Law of Fatigue.

"*When the same muscle (or group of muscles) is kept in constant action until fatigue sets in, the total work done multiplied by the rate of work is constant.*"

The following experiments were made during the last six months by Dr. Macalister and myself:—

A pair of 10-lb. dumbbells, held one in each hand, were raised simultaneously from the vertical to the horizontal position, and again lowered, at a rate regulated by a metronome made for the purpose. No rest was allowed at the beginning or end of the motion, which took place as before, under the following conditions, viz.: —

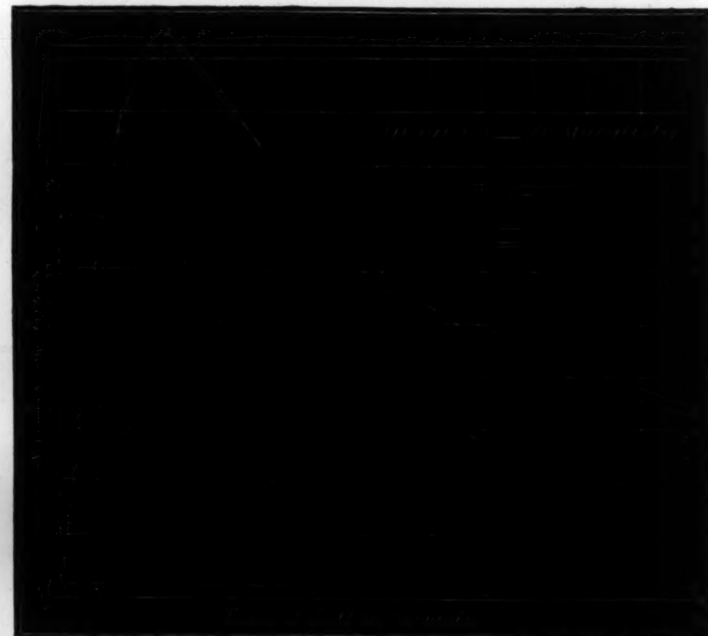
1. To keep time with the metronome.
2. To raise the weights in the transverse plane.
3. To supinate the hands.
4. To abstain from all bending of the knees or spinal column.
5. The experimenter not to count the lifts.

The experiments were made at intervals never less than 24 hours, so as to avoid all risk of the muscles becoming trained; and on each occasion the weights were lifted until it became impossible to effect another lift, without violating condition No. 4, indicating that other muscles were called in, to aid the shoulder-muscles already worn out. The following results were obtained, the exact weight of each dumbbell being 9·75 lbs.

Dr. Macalister.

Time of Lift.	Number of Lifts.										Mean.
0·50 sec.......	20	18	18	20	19	...	...	...	...	...	19·0
0·66 „	24	25	25	24	28	23	22	25	25	25	24·6
1·00 „	23	25	25	25	26	29	25	28	28	28	26·2
1·50 „	24	25	24	23	22	24	24	23	22	22	23·6
2·00 „	20	18	19	20	18	21	21	21	20	22	20·0
3·00 „	14	13	14	15	15	...	...	...	...	...	14·2
4·00 „	12	13	12	13	12	...	...	...	...	...	12·4
6·00 „	8	8	8½	8½	9½	...	...	...	...	...	8·5

In the following diagram these results are plotted to scale.



I shall now proceed to compare these results with calculations made from the Law of Fatigue. In the examples of the Law of Fatigue given in No. VII. the work done by the muscles is dynamical work, and consists in lifting weights at a fixed rate until fatigue sets in; but in the present experiments the work done is partly dynamical and partly statical, the latter consisting in the efforts made by the muscles to hold the weight and arm extended in positions varying from the vertical to the horizontal position.

Let W_1, R_1 be the dynamical work and rate of work, and let W_2, R_2 be the statical work and rate of work.

If the work done were purely dynamical or purely statical, we should have, by the Law of Fatigue, either

$$W_1 R_1 = \frac{W_1^2}{T} = \text{constant},$$

or

$$W_2 R_2 = \frac{W_2^{\,2}}{T} = \text{constant},$$

T being the total time of work.

The Law of Fatigue may be applied in one or other of two ways.

1°. The total work done is $W_1 + W_2$, and the rate of work $\dfrac{W_1 + W_2}{T}$.

Hence the Law of Fatigue gives us

$$\frac{(W_1+W_2)^2}{T} = \text{constant} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The dynamical work is proportional to x, the length of the arm, to $w+a$, the weight plus half the weight of the arm, and to n the number of lifts.

The statical work is proportional to x, the length of the arm, to $w+\alpha$, the weight plus half the weight of the arm, and to T the total time.

The total time T is proportional to

n, the number of lifts,

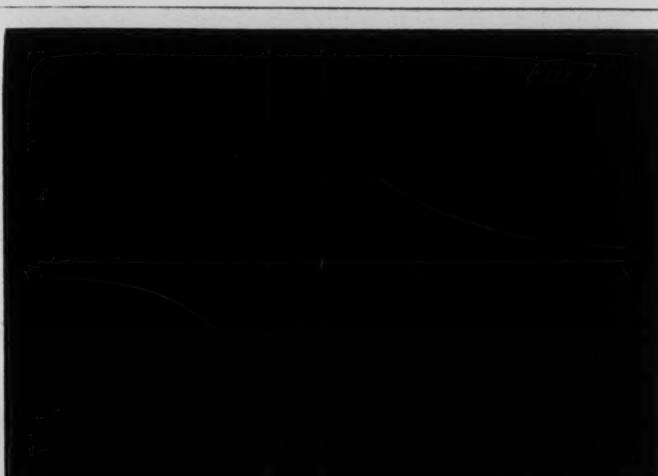
and to t, the time of each lift.

Hence equation (1) becomes

$$\frac{(w+\alpha)^2 x^2 n^2 (1+\beta t)^2}{nt} = \text{constant},$$

where β is an unknown constant. But $w+\alpha$ and x are constants in these experiments, and hence we find equation (1) reduced to the following:—

$$\frac{n\ (1+\beta t)^2}{t} = A \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)^*$$



* The general form of this *cuspidal cubic* is shown in fig. 1, in which XX′ is the single asymptote, corresponding to

$$t = 0,$$

This represents a *cuspidal cubic*; and we are required to find values for β and A which will satisfy the experiments.

2°. The second method of applying the Law of Fatigue leads to an equation which represents the experiments better than equation (2); and the principle on which it is founded is probably a more correct application of the Law of Fatigue. I assume that fatigue will occur when the dynamical work multiplied by its rate, together with the statical work multiplied by its rate, shall be constant; or if W R represent the total work of all kinds, and *rate of both works*, then

$$\mathrm{W\,R} = \mathrm{W_1\,R_1} + \mathrm{W_2\,R_2} = \text{constant} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

This is equivalent to assuming the total rate of work to be

$$\mathrm{R} = \frac{\mathrm{W_1\,R_1} + \mathrm{W_2\,R_2}}{\mathrm{W}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

as in the problem of the specific gravity of a binary compound.

Equation (3) becomes at once

$$\frac{\mathrm{W_1}^2 + \mathrm{W_2}^2}{\mathrm{T}} = \text{constant},$$

or

$$\frac{\overline{(w+\alpha)\,x\,n}^2 + \overline{(w+\alpha)\,x\,\beta t\,n}^2}{n\,t} = \text{constant}$$

or finally, since $(w+\alpha)$ and x are constants,

$$\frac{n\,(1+\beta^2\,t^2)}{t} = \mathrm{A} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)^*$$

We have now to take equations (2) and (5) in succession, and find which of them corresponds best to the observations. The method I have followed is this:—Let any value of β be assumed, and substituted in equations (2) and (5) for all the values of t; the resulting values of A will differ more or less from each other: let $\delta\mathrm{A}$ be the greatest difference between any two values, and let μ be the number of observations, and $\Sigma\,.\,\mathrm{A}$ the sum of the values of A; then I determine for each succes-

---

and AA′ is the double asymptote, corresponding to

$$1+\beta\,t = 0,$$

and having a cusp at negative infinity. There is a hyperbolic branch lying between X′P and A′P.

The portion of the curve with which we are concerned lies between OX and OY.

The number of lifts (n) attains a maximum MM′ when

$$1-\beta t = 0,$$

and the point of inflexion of the curve N occurs at double the preceding value of t; after which the curve becomes asymptotic to the line OX.

* This equation represents a *central cubic* whose general form is shown in fig. 2 (p. 135). It has a double point at infinity on the axis YY′, which is a conjugate point

sive value of β the quantity $\frac{\delta A}{\Sigma . A}$, and finally choose that value of β which makes $\frac{\delta A}{\Sigma . A}$ = minimum.

The greatest deviation per cent. of any value of A from the mean value is of course $\frac{\mu \delta A}{2 \Sigma . A}$ = maximum error per cent., where μ is the total number of experiments. Proceeding in this manner, we find, from equation (2), the following—

β.	$\frac{\delta A}{\Sigma . A}$.	
1·0	5·0	
1·4	4·43	
1·5	4·18	
1·6	4·45	
2·0	5·7	(Dr. Macalister.)

This gives a maximum error in the value of A of 16·72 per cent.

Applying the same method to equation (5), we obtain

β.	$\frac{\delta A}{\Sigma . A}$.	
0·6	10·2	
0·9	3·22	
1·0	1·55	
1·1	3·32	
1·6	8·4	(Dr. Macalister.)

(acnode) and not a cusp. The curve is central and has a point of inflexion at the origin, and the axis XX′ is asymptotic on both sides. The tangent at the origin is $n = A\,t$.

The ordinate (n) reaches a maximum for the values

$$\beta t \pm 1 = 0,$$

corresponding to M, M′.

The curve has also two real points of inflexion N, N′, corresponding to

$$\beta t \pm \sqrt{3} = 0.$$

This gives a maximum error in the value of A of 6·20 per cent. Hence we adopt the equation (5) as the best representation of the observations, and as the best application of the Law of Fatigue.

For $\beta = 1\cdot0$, we find

$$
\begin{array}{rl}
A = & 47\cdot5 \\
& 53\cdot3 \\
& 52\cdot4 \\
& 51\cdot0 \\
& 50\cdot0 \\
& 47\cdot0 \\
& 52\cdot7 \\
& 51\cdot8 \\
\hline
\text{Mean} \ldots\ldots & 50\cdot7
\end{array}
$$

We may now proceed to calculate the values of n from equation (5), using the constants

$$A = 50\cdot7$$
$$\beta = 1\cdot0;$$

and thus we obtain

Dr. Macalister.

No.	t.	n (obs.).	n (calc.).	Diff.
1.	0·50 sec.	19·0	20·2	− 1·2
2.	0·66 „	24·6	23·4	+ 1·2
3.	1·00 „	26·2	25·4	+ 0·8
4.	1·50 „	23·6	23·4	+ 0·2
5.	2·00 „	20·0	20·2	− 0·2
6.	3·00 „	14·2	15·2	− 1·0
7.	4·00 „	12·4	11·9	+ 0·5
8.	6·00 „	8·5	8·3	+ 0·2

This Table shows a very satisfactory agreement of the observations with the Law of Fatigue expressed by equation (5); and this agreement is also shown in the Diagram on p. 132, where the curve (5) is drawn to scale, and where the individual observations are marked by the small circles.

X. "On Repulsion resulting from Radiation. Influence of the Residual Gas."—(Preliminary Notice.) By William Crookes, F.R.S. &c. Received June 13, 1876.

I have recently been engaged in experiments which are likely to throw much light on some obscure points in the theory of the repulsion resulting from radiation. In these I have been materially assisted by Professor Stokes, both in original suggestions and in the mathematical

formulæ necessary for the reduction of the results. Being prevented by other work from completing the experiments sufficiently to bring them before the Royal Society prior to the close of the session, I have thought that it might be of interest were I to publish a short abstract of the principal results I have obtained, reserving the details until they are ready to be brought forward in a more complete form.

In the early days of this research, when it was found that no movement took place until the vacuum was so good as to be almost beyond the powers of an ordinary air-pump to produce, and that as the vacuum got more and more nearly absolute, so the force increased in power, it was justifiable to assume that the action would still take place when the minute trace of residual gas which theoretical reasoning proved to be present was removed. The first and most obvious explanation therefore was that the repulsive force was directly due to radiation. Further consideration, however, showed that the very best vacuum which I had succeeded in producing might contain enough matter to offer considerable resistance to motion. I have already pointed out that in some experiments, where the rarefaction was pushed to a very high point, the torsion-beam appeared to be swinging in a viscous fluid (194); and this at once led me to think that the repulsion caused by radiation was indirectly due to a difference of thermometric heat between the black and white surfaces of the moving body (195), and that it might be due to a secondary action on the residual gas.

On April 5, 1876, I exhibited at the Soirée of the Royal Society an instrument which proved the presence of residual gas in a radiometer which had been exhausted to a very high point of sensitiveness. A small piece of pith was suspended to one end of a cocoon fibre, the other end being attached to a fragment of steel. An external magnet held the steel to the inner side of the glass globe, the pith then hanging down like a pendulum, about a millimetre from the rotating vanes of the radiometer. By placing a candle at different distances off, any desired velocity, up to several hundreds per minute, could be imparted to the fly of the radiometer. Scarcely any movement of the pendulum was produced when the rotation was very rapid; but on removing the candle, and letting the rotation die out, at one particular velocity the pendulum set up a considerable movement. Professor Stokes suggested (and, in fact, tried the experiment at the time) that the distance of the candle should be so adjusted that the permanent rate of rotation should be the critical one for synchronism corresponding to the rate at which one arm of the fly passed for each complete oscillation. In this way the pendulum was kept for some time swinging with regularity through a large arc.

This instrument proved that, at a rarefaction so high that the residual gas was a non-conductor of an induction-current, there was enough matter present to produce motion, and therefore to offer resistance to motion. That this residual gas was something more than an accidental accom-

paniment of the phenomena was rendered probable by the observations of Dr. Schuster, as well as by my own experiments on the movement of the floating glass case of a radiometer when the arms are fixed by a magnet*.

My first endeavour was to get some experimental means of discriminating between the viscosity of the minute quantity of residual gas and the other retarding forces, such as the friction of the needle-point on the glass cup when working with a radiometer, or the torsion of the glass fibre when a torsion-apparatus was used. A glass bulb is blown on the end of a glass tube, to the upper part of which a glass stopper is accurately fitted by grinding. To the lower part of the stopper a fine glass fibre is cemented, and to the end of this is attached a thin oblong plate of pith, which hangs suspended in the centre of the globe; a mirror is attached to the pith bar, which enables its movement to be observed on a graduated scale. The stopper is well lubricated with the burnt india-rubber which I have already found so useful in similar cases (207). The instrument is held upright by clamps, and is connected to the pump by a long spiral tube. The stopper is fixed rigidly in respect to space, and an arrangement is made by which the bulb can be rotated through a small angle. The pith plate, with mirror, being suspended from the stopper, the rotation of the bulb can only cause a motion of the pith through the intervention of the enclosed air. Were there no viscosity of the air, the pith would not move; but if there be viscosity, the pith will turn in the same direction as the bulb, though not to the same extent, and, after stopping the vessel, will oscillate backwards and forwards in decreasing arcs, presently setting in its old position relatively to space.

It was suggested by Prof. Stokes that it would be desirable to register not merely the amplitude of the first swing, but the readings of the first five swings or so. This would afford a good value of the logarithmic decrement (the decrement per swing of the logarithm of the amplitude of the arcs), which is the constant most desirable to know. The logarithmic decrement will involve the viscosity of the glass fibre; but glass is so nearly perfectly elastic, and the fibre so very thin, that this will be practically insensible.

According to Professor Clerk Maxwell, the viscosity of a gas should be independent of its density; and the experiments with this apparatus have shown that this is practically correct, as the logarithmic decrement of the arc of the oscillation (a constant which may be taken as defining the viscosity of the gas) only slightly diminishes up to as high an exhaustion as I can conveniently attain—higher, indeed, than is necessary to produce repulsion by radiation.

I next endeavoured to measure, simultaneously with the logarithmic decrement of the arc of oscillation, the repulsive force produced by a candle at high degrees of exhaustion. The motion produced by the rotation of the bulb alone has the advantage of exhibiting palpably to the

* Proc. Roy. Soc. vol. xxiv. p. 409.

eye that there *is* a viscosity between the suspended body and the vessel; but once having ascertained that, and admitting that the logarithmic decrement of the arc of oscillation (when no candle is shining on the plate) is a measure of the viscosity, there is no further necessity to complicate the apparatus by having the ground and lubricated stopper. A movement of the whole vessel bodily through a small arc is equally effective for getting this logarithmic decrement; and the absence of the stopper enables me to have the whole apparatus sealed up in glass, and I can therefore experiment at higher rarefactions than would be possible when a lubricated stopper is present.

The apparatus, which is too complicated to describe without a drawing, has attached to it:—*a*, a Sprengel pump; *b*, an arrangement for producing a chemical vacuum; *c*, a lamp with scale, on which to observe the luminous index reflected from the mirror; *d*, a standard candle at a fixed distance; and *e*, a small vacuum-tube, with the internal ends of the platinum wires close together. I can therefore take observations of:—

1. The logarithmic decrement of the arc of oscillation when under no influence of radiation;

2. The successive swings and final deflection when a candle shines on one end of the blackened bar;

3. The appearance of the induction-spark between the platinum wires.

1 measures the viscosity; 2 enables me to calculate the force of radiation of the candle; and 3 enables me to form an idea of the progress of the vacuum according as the interior of the tube becomes uniformly luminous, striated, luminous at the poles only, or black and non-conducting.

The apparatus is also arranged so that I can try similar experiments with any vapour or gas.

The following are some of the most important results which this apparatus has as yet yielded.

Up to an exhaustion at which the gauge and barometer are sensibly level, there is not much variation in the viscosity of the internal gas (dry atmospheric air). Upon now continuing to exhaust, the force of radiation commences to be apparent, the viscosity remaining about the same. The viscosity next commences to diminish, the force of radiation increasing. After long-continued exhaustion the force of radiation approaches a maximum; but the viscosity measured by the logarithmic decrement begins to fall off, the decrease being rather sudden after it has once commenced.

Lastly, some time after the logarithmic decrement has commenced to fall off, and when it is about one fourth of what it was at the commencement, the force of radiation diminishes. At the highest exhaustion I have yet been able to work at, the logarithmic decrement is about one twentieth of its original amount, and the force of repulsion has sunk to a little less than one half of the maximum. The attenuation has now

become so excessive that we are no longer at liberty to treat the number of gaseous molecules present in the apparatus as practically infinite and, according to Professor Clerk Maxwell's theory, the mean length of path of the molecules between their collisions is no longer very small compared with the dimensions of the apparatus.

The degree of exhaustion at which an induction-current will not pass is far below the extreme exhaustions at which the logarithmic decrement falls rapidly.

The force of radiation does not act suddenly, but takes an appreciable time to attain its maximum—thus proving, as Prof. Stokes has pointed out, that the force is not due to radiation *directly* but *indirectly*.

In a radiometer exhausted to a very high degree of sensitiveness, the viscosity of the residual gas is almost as great as if it were at the atmospheric pressure.

With other gases than air the phenomena are different in degree, although similar in kind—aqueous vapour, for instance, retarding the force of repulsion to a great extent, and carbonic acid acting in a similar though less degree.

The evidence afforded by the experiments of which this is a brief abstract is to my mind so strong as almost to amount to conviction that the repulsion resulting from radiation is due to an action of thermometric heat between the surface of the moving body and the case of the instrument, through the intervention of the residual gas. This explanation of its action is in accordance with recent speculations as to the ultimate constitution of matter and the dynamical theory of gases.

XI. "Note on certain unusual Coagulation-appearances found in Mucus and other Albuminoid Fluids." By Charles Creighton, M.B., M.A. Communicated by Prof. Huxley, Sec. R.S. Received June 9, 1876.

The following observations were made in the course of re-examining a number of microscopic preparations that had been originally made for other purposes. They relate to certain unusual coagulation-forms that mucous or colloid or other albuminoid fluids assume when they are treated in a particular way.

In an early investigation of Virchow's ('Ueber die Form des geronnenen Faserstoffs') the production of the fibrinous threads of a coagulum was attributed to the contraction of the clot towards particular points, and was compared to the process of crystallization. "We may consider," says Virchow, "this process to be a kind of *organic crystallization*, wherein each separate fibril must be viewed as a complex of smaller crystalline particles. As in crystallization, so likewise in this case the separate molecules arrange themselves in particular directions to form delicate

columns, in which, however, no characteristic surfaces or angles can be discovered even with the highest magnifying-powers. . . . . In the case of mucus we have an appearance quite similar. The solidified jelly-like mucus is entirely homogeneous and hyaline; and only in the direction in which it is stretched, dragged, or torn does it show folds and markings, and, according to circumstances, even a network—an actual division into filaments or bundles. But if it is brought to coagulate by means of water, still better by means of alcohol or acids, the coagulation is followed by a shrinking, through which actual fibrils may result"*. The coagulation-appearances now to be described will be found to favour the analogy with crystallization. These exceptional coagulation-forms are also interesting as having recently been mistaken for something quite different. This mistake, which has greatly contributed to the spread of a reactionary and superficial pathology, will be referred to at the end of the note.

The mucus on which the observations have been made occurred in the acini and ducts of mammary glands of the bitch and cat, in the alveoli of the thyroid body, and in the alveolar spaces of two extensive colloid or myxomatous tumours that grew from bone or periosteum; the mammary gland is known to produce mucus as a normal secretion under certain circumstances †. The same appearances have been found also among the coagulated plasma with which the veins of a lymphatic gland were filled. The appearances found in these cases seem to depend on the mode of preparation, which was essentially the same in them all. The portions of gland or other mucus-containing tissue were immersed in the hardening fluid as soon as they were removed from the body and while they were still warm. The hardening fluid was either a $\frac{1}{6}$ per cent. solution of chromic acid, or the same solution mixed with an equal quantity of methylated spirit.

The preparations of the thyroid body are the best adapted to show the whole series of coagulation-forms and the gradations between them. A large number of the alveoli are filled with perfectly homogeneous or jelly-like coagulum, which has coloured purple with the logwood staining-fluid. The edge of the coagulum is very often vesiculated in such a manner that it seems to be attached to the wall of the alveolus by the points of crescent-shaped indentations. The same dentate edge of the coagulum is often seen within the veins in chromic-acid preparations. The form of coagulation in the thyroid that comes nearest to the homogeneous is where a number of fine granules appear to be imbedded in the jelly-like mucus; this produces a cloudy or spawn-like effect. Side by side with these alveoli occur others, in which the coagulum is nothing but a closely packed mass of granules without connecting substance; and

* Gesammelte Abhandlungen, pp. 66, 67.

† See the writer's paper on "Physiological Processes of the Mamma" in the Report of the Medical Officer of the Privy Council for 1875.

these granules vary much in size in different alveoli. In some the coagulated mucus becomes finely divided, while in others it is broken up into only a few rounded masses of relatively large size. Perhaps the commonest size of the globules or granules of mucus is about one third the diameter of the red blood-corpuscle, while the largest masses may be twice its diameter. All the other coagulation effects can be shown to be modifications of the granular condition; they range from not very complex groups of granules or spherules to a close meshwork, and to long branching filaments like the threads of ordinary fibrin. Many alveoli of the thyroid body show the simpler reticular arrangements of the coagulated mucus; the more complex reticular appearances are best seen in two preparations of the mammary gland with mucus in the ducts; and the filamentous appearance is best seen in the large colloid collections that occur throughout the two myxomatous tumours. The mucus in the mammary ducts has the following singular appearance:—Along both sides of the duct there is a strip of homogeneous mucus which adheres to the wall by means of the dentate points already mentioned; further towards the centre of the duct the mucus is broken up into a mass of granules; and the broad central space is occupied by a network, the meshes of which become larger towards the middle. The appearance looks as if it had been produced by a shrinking of the mucus towards the sides of the duct. The fibres of this reticulum are short and thick, and there arise from them numerous knob-like projections, chiefly at the points where the fibres seem to branch; and where the fibres seem in the section to end, their free extremities are found to be capped with the same knob-like enlargements. These round processes are of the same diameter as the threads or cords on which they are seated. The substance of the whole reticulum is uniform, and is coloured throughout with the staining-fluid.

The plan of this singular arrangement becomes clear by studying the simpler forms of it, as seen in the alveoli of the thyroid body. Several granules or spherules of mucus are found to have arranged themselves like a group of crystals. Round a central spherule three or four others (as it appears in the section) are regularly grouped to form a small rosette. When several of these rosettes are placed in apposition, the optical effect is that of branching cords or fibres with knob-like projections arising from them at short and equal intervals. The appearance of a nodulated fibre is best seen where the elements composing it are small. The smaller the granules or spherules are, the longer do the intervals seem between the upright knob-like projections; but if the focus is altered, there come into view other projections arising along the course of the apparent fibre at other angles or in other planes.

The common starting-point of the various coagulation-forms that have been described appears to be that, under the influence of certain reagents, the warm albuminoid fluid is deposited in the form of larger or

smaller droplets or granules. These droplets, which are analogous to the crystals deposited from a crystallizing solution, are sometimes found of considerable size among the homogeneous mucus: in many cases the whole coagulum is granular, the granules remaining closely packed together; but they sometimes group themselves at certain points in the field, leaving free spaces between the groups, and these groups have each something of the regularity of a rosette of crystals. The clusters of granules further assume more of a reticular arrangement or more of a linear, according to circumstances.

The plasma of the blood is found sometimes, in chromic-acid preparations, to assume the same coagulation-forms as those just described and explained for mucus; the necklace-like or nodulated fibres are obviously a modification of the ordinary fibrinous filaments of blood-clot.

The appearance described above of a reticulum of structureless or jointed filaments with knob-like projections arising at short intervals along their course and at their free extremities is precisely the same appearance as Dr. E. Klein found within the vesicles and pustules, as well as in the lymphatics, lymphatic spaces, and veins in the skin of sheep infected with variola (Transactions of the Royal Society, vol. clxv. pt. 1, p. 233 *et seq.*). The figures 11, 17, 18, and 19 of Dr. Klein's paper represent appearances that cannot be distinguished in any point from the particular coagulation-appearances that were found in the ducts of the mamma above described. The figs. 10 and 16, showing the more attenuated filaments with the knob-like projections at wider intervals, correspond to the coagulation-forms that were found chiefly in the colloid tumours. There can also be no doubt that the granular masses represented in figs. 7, 8, 9, and 13 of Dr. Klein's paper are the same forms of granular coagulation as those described in this note; Dr. Klein has himself noted the occurrence of the granular substance side by side with and passing gradually into the filamentous (p. 241). While there is, on the one hand, a remarkable resemblance as regards form between the various appearances figured by Dr. Klein and the various coagulation-appearances herein described, there is, on the other hand, an essential similarity in the circumstances under which they occurred in the respective cases. In the case of the sheep-pox preparations, the appearances were found either in vesicles that contained a coagulable fluid, or in lymphatics and interfascicular spaces that were distended by œdema, or in veins. The portions of skin were immersed, while still warm, in the hardening fluid, which was sometimes a weak solution of chromic acid and sometimes methylated spirit (p. 219). Chromic-acid preparations alone seem to have been used for studying that stage of the disease in which the vesicles and pustules are completely formed (pp. 219, 230).

Now, although Dr. Klein considered that he had before him in these

preparations the various conditions of a fungus, to which he gave a generic and a specific name, and although he professed to find the various conditions of spore, mycelium, and fructification occurring in their natural sequence, and that natural sequence to correspond with the regular advance of the pathological process, there is no doubt that this circumstantial account rests on erroneous observation and on defective evidence, and that the appearances found in the skin of the sheep are none other than those resulting from the coagulation of albuminoid fluids under particular circumstances.

XII. "Determination of Verdet's Constant in Absolute Units." By J. E. H. Gordon, B.A., Gonville and Caius College, Cambridge.—1st and 2nd Memoirs. Communicated by J. Clerk Maxwell. Received June 5, 1876.

(Abstract.)

[*Note.*—The whole of this work has been done under Prof. Clerk Maxwell's superintendence; he suggested the method and nearly all the details. He is, however, in no way responsible for any errors there may be in the numerical results.]

Introduction.

In the year 1845 Faraday discovered that if plane polarized light passes through certain media, and these media be acted on by a sufficiently powerful magnetic force, the plane of polarization is rotated.

About the year 1853 M. Verdet commenced a long and exhaustive examination of the subject, and his first result (published 'Ann. de Chimie et de Phys.' 3 série, tom. xli.) was that, for any given magnet and medium, "the ratio between the strength of the magnet and the amount of rotation is constant"*.

The object of the present research is to determine this constant in absolute measure—that is, in the C.G.S. system.

In order that the measurements may be expressed in absolute units, it is necessary to modify M. Verdet's mode of proceeding in several respects. In particular, an electromagnet with an iron core is unsuitable for this investigation, for both the amount and the distribution of the magnetic force between the poles depend on the properties of the iron core, and cannot be deduced from the strength of the current in the helix. Faraday's heavy glass and other media having the highest power of rotating the plane of polarization were also unsuitable to be used as standard media, on account of the difficulty of procuring specimens exactly alike. The following method was therefore adopted:—

The magnetic force was produced by means of an electric current in a

* This is expressed much more fully in Maxwell's 'Electricity,' vol. ii. p. 400, art. 808. The coefficient mentioned in the last line of the article may be defined as Verdet's constant. In the author's larger paper the identity of the two definitions is shown.

helix without an iron core, and bisulphide of carbon, enclosed in a tube with glass ends placed within the helix, was chosen as the medium.

The strength of the current in the helix was deduced from the deflection of a small magnet suspended near to it and outside it, and the rotation was measured by means of a divided circle.

The investigation then resolved itself into three parts:—

1. The determination of the constants of the helix.

2. The determination of the ratio which the rotation per unit of length bore to the tangent of the deflection of the suspended needle.

3. The determination of the horizontal component of the earth's magnetism at the time and place of observation.

The Experiments.

Determination of number of windings.

To determine the number of windings, it is necessary to know the difference of magnetic potential at the ends of the helix when a unit current passes in the wire.

To determine this the author places the helix and great dynamometer coaxial, and suspends a magnet and mirror at the centre of the dynamometer. By sliding the helix endways along the axis, so as to bring different points of it over the suspended mirror, he obtains the magnetic intensity at these points in terms of that of the dynamometer, which is known. Varying currents are set in opposite directions through helix and dynamometer till the action on the suspended magnet is zero. By integrating these values along the axis between limits corresponding to the ends of the helix the difference of magnetic potential at the ends for a unit current is determined. A rule known as Weddle's (see Boole's 'Finite Differences,' p. 47) is used for the integration.

This difference is called N, and from it is deduced the number of windings (n) by Maxwell's 'Electricity,' art. 676. After describing the mechanical arrangements and giving a drawing of the connexions, the author gives a Table showing the results of the experiments for the determination of N.

The final results are

$$N=10752,$$
$$n=1028{\cdot}15.$$

By an equation of units N is shown to be the ratio of two things of the same dimensions, and therefore a number.

Determination of Areas.

To calculate the strength of a current in a helix from the deflection of a magnet suspended outside it, it is necessary to know $\Sigma(A)$, the sum of the areas of the windings.

This was obtained by comparing the action of the helix on such a magnet

with those of coils of known areas. Two coils were used, a small one and the great electro-dynamometer of the British Association. With the small coil the same currents were sent through a coil and helix, and the distances from the suspended magnet varied; while with the large one the distances were the same and the currents varied. These latter experiments were made by Prof. Maxwell.

The following values were then obtained for the area of the helix:—

By the author with small dynamometer,

$$\Sigma(A)=77417{\cdot}2 \text{ sq. centims.};$$

by Prof. Maxwell with large dynamometer,

$$\Sigma(A)=77488{\cdot}8 \text{ sq. centims.};$$

the mean, 77453·0, of these was adopted.

Calculation of the strength of the current in terms of the deflection δ of the magnet suspended outside the helix and in the bisecting plane perpendicular to its axis.

The author shows that this is

$$C=\frac{Hr^3}{\Sigma(A)}\tan\delta,$$

where r is the distance from the suspended magnet to either end of the axis of the helix, and H the horizontal component of the earth's magnetism at the time and place of observation.

Formula for ω.

ω is the rotation of the polarized ray expressed in circular measure between two points in its path, whose magnetic potential differs by unity; thus

$$\omega=\frac{\theta}{V_L-V_M},$$

where L and M are the ends of the tube, and θ is half the difference of the circle readings expressed in circular measure.

An approximation is given for the difference of potentials at the ends of a tube (A B) of finite length projecting at each end of the helix (L M). The letters being in the order A, L, M, B, the formula for ω becomes

$$\omega=\frac{\theta}{\left\{4\pi n-(\Sigma(A))\left(\frac{1}{LA.LB}+\frac{1}{MA.MB}\right)\right\}\frac{Hr^3\tan\delta}{\Sigma(A)}}.$$

Tan δ.

The author explains at length the method of adjusting the telescope and scale.

A formula for deducing the angular deflection from the scale-reading is obtained.

The Light.

Monochromatic light was used, obtained by throwing a spectrum on a card, in which was a slit to admit the colour required. A method of localizing the light used is given.

The following results for $\frac{2R}{y}$, where $y \equiv \tan \delta$, are given, and the wave-length of the light was that of the green thallium line:—

Set.	2R.	Grove's cells.	$\frac{2R}{y} = \frac{\text{Const.}}{H}$.
2....	11° 58′ 30″	5	8861·1 min.
3....	13° 39′ 30″	6	8820·6 min.
1....	15° 26′ 0″	7	8917·5 min.

where 2R is the difference of the circle readings corresponding to the two directions of the current.

Value of H.

This was determined by vibrating the same magnet at Kew and at the author's laboratory at Pixholme, Dorking, where all the optical part of the work was done, and then deducing the force at Pixholme from the Kew magnetograph curves at the times of experiment. The magnet used was very kindly lent to the author by Mr. Whipple.

The result obtained was

$$\text{H at Pixholme} = (\cdot 993366)\ \text{H at Kew}.$$

Value of the Constant.

The values of H at Pixholme at the times of the optical experiments having been calculated, we have for the three values of the quantity which should be constant:—

$$\frac{2R}{yH} = \left\{\begin{array}{l} 1 \ldots\ldots 50118\cdot 4 \\ 2 \ldots\ldots 49767\cdot 0 \\ 3 \ldots\ldots 49538\cdot 7 \end{array}\right\} \begin{array}{c}\text{mean} \\ 49808\cdot 0.\end{array}$$

Extreme difference 0·6 per cent.

We have finally, if ω be the rotation in bisulphide of carbon of the plane of polarization of the ray whose wave-length is

$$\lambda = (5\cdot 349)10^{-5}$$

between two points whose magnetic potentials differ by unity,

$$\omega = 3\cdot 04763(10^{-5}).$$

The dimensions of the constant are

$$[\omega] = [M^{-\frac{1}{2}} L^{-\frac{1}{2}} T].$$

The paper concludes with a few very inadequate words of thanks to Prof. Maxwell for his great kindness in superintending the whole of the work for the year and eight months during which it has been in progress.

The author also records his thanks to Mr. Whipple and several other friends for assistance and suggestions.

An Appendix contains an analysis of the bisulphide used.

XIII. "Contributions to Terrestrial Magnetism."—No. XV. By General Sir Edward Sabine, R.A., K.C.B., F.R.S. Received June 14, 1876.

(Abstract.)

The paper now offered to the Society forms the fifteenth and last of a series of papers printed in the Philosophical Transactions, entitled "Contributions to Terrestrial Magnetism." The whole fifteen numbers are related to each other as "Contributions to the Magnetic Survey of the Globe." Four of them, viz. XI., XIII., XIV. and the present paper, contain the complete statement of this survey in the double form of "Catalogue" or "Tables" and of "Magnetic Maps;" of these maps there are twelve, one for each of the three magnetic elements in each of the four papers. The present paper consists (as did its last predecessor, No. XIV.) of four zones, each 10° in breadth:—

Zone 1,	comprehending	from	the equator	to 10° S.
Zone 2,	„	„	lat. 10° S.	„ 20° S.
Zone 3,	„	„	lat. 20° S.	„ 30° S.
Zone 4,	„	„	lat. 30° S.	„ 40° S.

In the Tables the observations are entered in each zone in the succession of their longitudes, beginning with the meridian of Greenwich. The statements in the introduction to No. XIII. regarding the different magnetic elements apply to the present paper, as they did also to the preceding paper, No. XIV.

The question of correction for secular change next presents itself. Happily the greater part of the observations were made within, or very near to, the "mean epoch," viz. 1840–1845. Sea-observations were not generally corrected for differences of epoch in the previous papers; but in the present paper such corrections have been introduced for observations within the range of places (land stations) where the rate of secular change has been sufficiently established.

Nos. XI. & XIII., published earlier, comprise the northern and southern portions of the globe from either pole to lat. 40°. These are the regions which have long been recognized as offering to the magnetician at once the most arduous and the most important field of research. In the middle or equatorial portions of the globe, comprised in the last and present papers, the magnetic relations are simpler, and the laying down of the lines representing them derives much aid from the adjacent portions of the north and south polar maps; therefore, both as regards observations and treatment, less abundant as well as less exact evidence, it is hoped, may suffice.

XIV. "Researches illustrative of the Physico-Chemical Theory of Fermentation, and of the conditions favouring Archebiosis in previously Boiled Fluids."—No. I. By H. CHARLTON BASTIAN, M.A., M.D., F.R.S., Professor of Pathological Anatomy in University College, London, and Physician to University College Hospital. Received May 25, 1876.

(Abstract.)

The author first calls attention to the fact that no previous investigator has professed to have seen well-marked fermentation set up in urine that had been boiled for a few minutes, if it has thereafter been guarded from contamination. The previous invariable barrenness of this fluid after boiling has been ascribed by germ-theorists to the fact that any organisms or germs of organisms which it may have contained were killed by raising it to the temperature of 212° F. (100° C.).

In executing some of the experiments with urine described in this communication, two chemical agents have been brought into operation under novel conditions, and an ordinary physical influence has been employed to an entirely new extent. In several respects, therefore, these new experiments differ much, as regards the conditions made use of, from those hitherto devised for throwing light upon the much-vexed questions as to the possible origin of fermentations independently of living organisms or germs, and as to the present occurrence or non-occurrence of Archebiosis.

The chemical agents employed under new conditions in these experiments were *liquor potassæ* and *oxygen*—both of them being well known as stimulants, if not as promoters, of many fermentative processes.

It has been recognized by several investigators of late years that neutral or slightly alkaline organic fluids are rather more prone to undergo fermentation than slightly acid fluids. This fact may be easily demonstrated. As the author pointed out in 1870, if two portions of an acid infusion are exposed side by side at a temperature of 77° F. (25° C.) fermentation may be made to appear earlier and to make more rapid progress in either of them by the simple addition of a few drops of liquor potassæ; on the other hand, if a neutral infusion be taken and similarly divided into two portions placed under the same conditions, fermentation may be retarded or rendered slower in either of them at will by the simple addition to it of a few drops of acetic or some other acid.

A neutral or faintly alkaline organic solution can in this way be demonstrated to possess a higher degree of fermentability than an otherwise similar acid organic solution. It seems, therefore, obvious that the changes capable of taking place in *boiled* acid and neutral solutions respectively should also vary considerably. Numerous experiments by

different observers have demonstrated the correctness of this inference. Boiled acid infusions guarded from contamination mostly remain pure and barren if kept at temperatures below 77° F. (25° C.), though other infusions similarly treated and similar in themselves, except that they have been rendered neutral by an alkali, will oftentimes become corrupt and swarm with organisms. The latter result follows still more frequently with neutral infusions when they are exposed to a higher generating temperature in the warm-air chamber; and under this stronger stimulus a small number of boiled acid fluids will also ferment.

On the other hand, the influence of oxygen in promoting fermentation has been fully appreciated since the early part of the present century. Formerly an influence was assigned to it as an initiater of fermentation as all-important as some chemists assign to living germs at the present day. But this was a very exaggerated view. In some fluids, as the author has shown, fermentation may be initiated just as freely, or even rather more so, in closed vessels from which the air has been expelled by boiling, as in others in which atmospheric air, and consequently oxygen, is present. The explanation of this fact is probably to be found in the supposition that, in starting the fermentation of these fluids, diminution of pressure may be of as much, or even of more, importance than contact with free oxygen. In respect to other organic fluids, however, the influence of oxygen seems decidedly more potent as a co-initiater of fermentation than that diminution of pressure which is brought about by hermetically sealing the vessel before the fluid within has ceased to boil. Urine will be found to be an example of this latter class of fluids.

The physical influence which has been employed in unusual intensity in the present researches is *heat*.

Previous experimenters have never designedly had recourse to a generating or developing temperature above 100° F. (38° C.). The heat employed has frequently been below 77° F. (25° C.), though a temperature between this and 95° F. (35° C.) has been regarded both by chemists and biologists as most favourable to the occurrence and progress of fermentative changes generally.

Early in the month of August 1875 the author ascertained the fact that some boiled fluids which remained barren when kept at a temperature of 77°–86° F. (25°–30° C.) would rapidly become turbid and swarm with organisms if maintained at a temperature of 115° F. (46° C.). More recently he has discovered the surprising fact that a generating temperature as high as 122° F. (50° C.) may be had recourse to with advantage in dealing with some fermentable solutions. Fluids which would otherwise have remained barren and free from all signs of fermentation have under the influence of this high temperature rapidly become turbid and corrupt. This discovery is regarded as of great importance in reference to the questions now under discussion, and it is one which was quite unexpected. The author had previously shared in

the generally received opinion that temperatures above 100° F. (38° C.) were likely to impede rather than promote fermentation.

In maintaining the experimental fluids at the high temperature above named, the vessels containing them were placed in the hot-air chamber of an incubator such as physiologists employ, to which one of the very ingenious gas-regulators of Mr. F. J. Page had been fitted (see Journal of the Chemical Society, January, 1876). In this way the fluids may be kept at a known and practically constant temperature for an indefinite time.

Liquor Potassæ as a promoter of Fermentation in Boiled Urine.

In the autumn of 1875 the author instituted some experiments to ascertain whether the fermentability of boiled urine, like that of many other fluids, could be increased by previously mixing with it a quantity of liquor potassæ sufficient for its neutralization.

The experiments answered this question in the affirmative. It was found that urine to which the above-named amount of liquor potassæ had been added would constantly ferment and swarm with organisms within a few days after it had been boiled, though some of the same stock of urine in the acid state (that is, without the addition of any alkali) would when similarly treated in other respects remain barren. The fact of the production of an increased fermentability in boiled urine by previous neutralization was thus established.

Further experiments were then instituted to throw light upon the cause of such increased fermentability. It was desirable to ascertain whether (1) it was due to survival of germs in the boiled neutralized fluid, or (2) to the chemical influence of potash in initiating or helping to initiate the molecular changes leading to fermentation in a fluid devoid of germs or other living matter.

The mode of testing the relative validity of these rival interpretations seemed easy. It was only necessary to ascertain what the effect would be of adding boiled liquor potassæ, in proper quantity, after the acid urine had been rendered barren by boiling it, instead of adding it previous to the process of ebullition. If fermentation occurred in the fluid thus neutralized without extraneous contamination, the first interpretation would obviously be negatived.

This crucial experiment was at first tried with flasks plugged with cotton-wool, the plug in each of them being penetrated by a closed glass tube containing the measured amount of liquor potassæ. The tubes having been drawn out to a capillary portion at the lower end, and bent at an obtuse angle, they could be easily broken by slight downward pressure against the bottom of the flask whenever it was desired to mix the liquor potassæ with the boiled urine. This apparatus was very similar to that first made use of by Dr. William Roberts in some experiments with hay-infusion (Phil. Trans. vol. clxiv. p. 474), in which he obtained opposite results from those now about to be recorded with urine. The

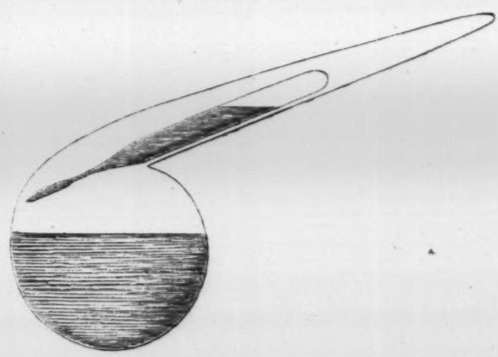
latter fluid, however, is for several reasons more suitable than hay-infusion for trying such experiments.

Several trials made with urine in this apparatus showed that its fermentability was just as much increased by adding boiled liquor potassæ after the urine had been boiled in the acid state, as by adding the alkali previous to the process of ebullition. Such a result was therefore quite opposed to the first interpretation as to the cause of the increased fermentability of neutralized urine.

The definite overthrow or establishment of this interpretation was so important that it seemed desirable to try such experiments again by some more rigid and certain method. The author, therefore, devised a new mode of experimentation in which sealed retorts replaced the flasks plugged with cotton-wool, and in which the contents of the enclosed liquor-potassæ tubes could be more effectually heated.

It was first of all ascertained that accurately neutralized urine boiled in a retort and sealed whilst boiling would ferment in a day or two if kept at a temperature of 122° F.*

This fact having been established, other retorts were charged with a measured amount of urine, and also with a small glass tube containing liquor potassæ in quantity almost sufficient to neutralize the urine employed †. The glass tubes containing the liquor potassæ had been drawn out at one end, sealed and then immersed in boiling water for different periods before introducing them into the retorts. After each



retort had been charged with urine and a liquor-potassæ tube, its neck was drawn out to a capillary point, the urine was boiled, and the retort was hermetically sealed before ebullition had ceased. Thus closed, the

* Though the boiled urine will ferment in retorts from which the air has been expelled by boiling, it will undergo this change more quickly if it is in the presence of purified or sterilized air. In the experiments now about to be described, however, it was much more convenient to use airless retorts.

† As a slight excess in the amount of liquor potassæ has been proved to have a most restrictive influence when dealing with urine, it was found safer in these experiments not to provide liquor potassæ sufficient for full neutralization. Many details on this subject are given in the memoir itself.

vessel was at once immersed with its neck downwards in a can of boiling water for from four to fifteen minutes, so as to expose it and its contents for an additional period to a temperature of 212° F. (100° C.).

The urine was thus boiled in its unaltered acid state and sterilized. After the retorts had cooled the liquor potassæ was liberated from its tube in all but one of the batch, which was kept as a control experiment. The liberation was easily effected. It was only necessary to give the retort a sudden shake so as to drive the capillary neck of the enclosed tube against its side. The tube was thus broken and immediately (owing to the comparative vacuum within the retort) the liquor potassæ was sucked out and mixed with the fluid which it was destined to neutralize.

The result of these experiments was similar to those executed with the plugged flasks and liquor-potassæ tubes. The boiled caustic potash added afterwards within the sealed retorts caused the previously barren fluids to ferment and swarm with *Bacteria*. The fluid in the control experiment remained pure, though after several days, or longer, it also could be made to ferment by breaking the liquor-potassæ tube and replacing the retort in the warm chamber.

Effects of liberating Oxygen by Electrolysis within the closed Retorts.—A few other experiments were made with retorts to which platinum electrodes had been fitted. These contained, as before, measured amounts of urine, together with liquor-potassæ tubes. All the preliminary stages were similar to those of the experiments above recorded; but just before breaking the liquor-potassæ tubes in these further experiments, oxygen and hydrogen were liberated from the boiled urine by electrolysis.

The result in the few experiments made was very remarkable. Under the combined influence of liquor potassæ, oxygen, and the high temperature of 122° F. (50° C.), the sterilized urine fermented and swarmed with *Bacteria* within the closed retorts in from 7–12 hours—that is, in a much shorter time than would suffice for the occurrence of similar changes in unboiled urine freely exposed to the air.

Behaviour of some specimens of unaltered Acid Urine under the influence of the High Generating Temperature of 122° F. (50° C.).

In the course of the previous experiments it was found that occasionally a specimen of boiled urine would ferment at a temperature of 122° F. without the addition of liquor potassæ. This was afterwards ascertained to occur invariably (with the urine experimented upon) when the acidity of the fluid was not higher than would be represented by six minims of liquor potassæ to the ounce (or about $1\frac{1}{2}$ per cent.). Urines slightly more acid than this sometimes did and sometimes did not ferment without liquor potassæ; but when the acidity exceeded what would be equivalent to two per cent. of liquor potassæ, the fluid did not ferment under the influence of the high generating temperature alone. Urines of all degrees of acidity, however, were found to ferment under the combined

influence of heat and liquor potassæ added afterwards, in the manner already detailed *.

It was further ascertained that the acidity of some specimens of urine was lessened during the process of ebullition (owing to the deposition of acid phosphates); and such urines boiled for six minutes were found to ferment in a much shorter time than when they were only boiled for three minutes. The prolongation to this extent of the germ-destroying temperature actually hastened the subsequent process of fermentation.

Interpretation of Results.

The generally received belief that all *Bacteria* and their germs are killed by exposing them even for a minute or two to the temperature of 212° F. (100° C.) has of late been strongly reinforced by Professor Tyndall. The fact, therefore, of the fermentation of some specimens of boiled acid urine, with the appearance of swarms of *Bacteria*, under the influence of the high generating temperature of 122° F. (50° C.), is inexplicable except upon the supposition that fermentation has in these instances been initiated without the aid of living germs, and that the organisms first appearing in such fluids have been evolved therein.

If the author's further position (Proceedings of Royal Society, Nos. 143 and 145, 1873), that *Bacteria* and their germs are killed in fluids whether acid or alkaline at a temperature of 158° F. (70° C.), is correct, then the occurrence of fermentation in the previously neutralized boiled urine would similarly disprove the exclusive germ-theory of fermentation and establish the occurrence of Archebiosis.

Any difficulty which might have been felt by others in accepting the above interpretation of the results of these latter experiments—in face of the view held by M. Pasteur that some *Bacteria*-germs are able in neutral fluids to survive an exposure to a heat of 212° F. (100° C.)—has been fairly met and nullified by the experiments (devised for the purpose) in which the urine was boiled in the acid state and subsequently fertilized by the addition of boiled liquor potassæ.

If we look at these latter experiments from an independent point of view, it will be found that this fertilization of a previously barren fluid by boiled liquor potassæ must be explained by one or other of three hypotheses:—

1*st Hypothesis. The boiled liquor potassæ may act as a fertilizing agent because it contains living germs.*—However improbable this hypothesis may seem on the face of it, it has been actually disproved by many of the experiments recorded in this memoir. These experiments show that boiled liquor potassæ will only act as a fertilizing agent when it is added in certain proportions. If it acted as a mere germ-containing medium, a

* In the urine of highest acidity with which experiment has been made, twenty minims of liquor potassæ to the fluid ounce (about 4 per cent.) was required for neutralization.

single drop of it would suffice to infect many ounces, a gallon, or more of the sterilized fluid. This, however, is never the case; it only fertilizes the barren urine when it is added in a proportion dependent upon the precise acidity and quantity of the fluid with which experiment is being made.

2nd Hypothesis. The fertilizing agent may act by reviving germs hitherto presumed to have been killed in the boiled acid urine.—The acceptance of this hypothesis would involve a general recantation of the previously received conclusion that *Bacteria* and their germs are killed by boiling them in acid fluids. But such a recantation would be scarcely justifiable or acceptable unless based upon good independent evidence.

The possibility, however, of accepting this second hypothesis is still further closed by the results of experiments in which a slight excess of liquor potassæ was added to the boiled urine. Such fluids invariably remained barren. Yet it can be easily shown that the mere development and growth of *Bacteria*-germs may take place both quickly and freely in boiled urine containing a very large excess of liquor potassæ *. It would seem that this agent mixed with boiled urine in quantity slightly more than is needed for neutralization, prevents the *origination* of living matter therein, although even when in considerable excess the same agent affords no obstacle to the development, growth, and multiplication of germs purposely added thereto.

In the face of these facts it would seem impossible to accept this second hypothesis, even if it had not been independently negatived by the great mass of evidence (lately reinforced by the experiments of Professor Tyndall) to the effect that *Bacteria* and their germs are really killed in fluids raised for a few minutes to the boiling-point (212° F.).

3rd Hypothesis. The fertilizing agent acts by helping to initiate chemical changes of a fermentative character in a fluid devoid of living organisms or living germs.—If the cause of the fermentation of the fluids in question does not exist in the form of living organisms or germs either in the fertilizing agent itself or in the medium fertilized, then it must be found in some chemical reactions set up between the boiled liquor potassæ and the boiled urine.

The experiments in which liquor potassæ is added to urine in definite proportions before and after it has been boiled with the result of inducing fermentation in the otherwise barren fluids, as well as those in which unaltered urine ferments under the influence of the high generating temperature of 122° F. (50° C.), all alike, therefore, point to the same conclusion. They show, as other experiments have done, that an exclusive germ-theory of fermentation is untenable; and they further show that

* A mixture of one part of liquor potassæ to seven of boiled urine poured into a bottle which has been washed with ordinary tap-water will, within forty-eight hours, swarm with *Bacteria* if it is kept at a temperature of 122° F.

living matter may and does originate independently during the progress of fermentation in previously germless fluids.

As a result of the fermentative changes taking place in boiled urine or other complex organic solutions, many new chemical compounds are produced: gases are given off, or these with other soluble products mix imperceptibly with the changing and quickening mother-liquid, in all parts of which certain insoluble products also make their appearance. Such insoluble products reveal themselves to us as specks of protoplasm, that is of 'living' matter; they gradually emerge into the region of the visible, and speedily assume the well-known forms of one or other variety of *Bacteria*.

These insoluble particles would thus in their own persons serve to bridge the narrow gulf between certain kinds of 'living' and of 'dead' matter, and thereby afford a long-sought for illustration of the transition from chemical to so-called 'vital' combinations.

XV. "On the Variations of the Daily Range of Atmospheric Temperature as recorded at the Kew Observatory." By Balfour Stewart, LL.D., F.R.S., Professor of Natural Philosophy at Owens College, Manchester. Received May 25, 1876.

(Abstract.)

The daily temperature-range was selected as an element which affords a good indication of the varying meteorological activity of the place, and the observations of which can be easily made and reduced.

The records of the Kew Observatory were chosen because there the atmospheric temperature has been very carefully observed during a long series of years. The writer desires to thank the Kew Committee for giving him access to the records of the maximum and minimum temperatures taken at the Kew Observatory.

Twenty-one years of these records have been reduced, beginning with the year 1855 and ending with 1875. Two complete sun-spot periods are embraced in these observations.

The first Table exhibits (a point already well known) the annual variation of the temperature-range, which is greatest in summer and least in winter.

The same Table shows that the yearly means of this element exhibit considerable fluctuations amongst themselves. Thus we have corresponding to the years 1856, 1866, and 1875 the values 12°·69, 13°·61, and 13°·25 respectively, while corresponding to the years 1859 and 1870 we have the values 14°·52 and 15°·63.

Inasmuch as the three former are years of minimum, and the two latter years of maximum sun-spots, this would seem to show that the daily temperature-range is least for minimum and greatest for maximum sun-spot years.

But, on the other hand, and against this evidence, there is a temperature-fluctuation between 1859 and 1866 as great, or nearly as great, as any which apparently corresponds to sun-spot period. This temperature-oscillation may perhaps be identified with a subsidiary sun-spot fluctuation as exhibited in the curves of Messrs. De La Rue, Stewart, and Loewy, but it is out of proportion to it in relative magnitude.

If we still regard it as most likely, though not proven, both from the evidence of the paper and from collateral considerations, that there is some connexion between the daily temperature-range and the state of the sun with regard to spots, then we may perhaps suppose that this redundant temperature-oscillation is a local phenomenon. There is, however, another possible explanation which will be afterwards alluded to.

It is then endeavoured to ascertain whether the temperature-range has any reference to the relative position of the sun and moon. For this purpose the whole period of 21 years has been portioned out into lunations, each lunation being divided into 8 parts:—(0), (1), (2), (3), (4), (5), (6), (7)—(0) corresponding to new and (4) to full moon.

From the whole series of lunations the following result is obtained:—

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Temp.-range......	14·08	14·20	14·29	14·05	13·95	13·83	14·04	14·17

which presents the appearance of a curve of double period superposed upon one of single period. The range, however, is not great.

If we now make use of the lunations corresponding to the six winter months (October to March), we obtain:—

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Temp.-range......	11·18	11·37	11·32	10·88	10·52	10·49	10·79	11·05

Treating in the same way the lunations for the six summer months, we obtain:—

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Temp.-range......	16·96	17·02	17·23	17·22	17·35	17·15	17·24	17·27

It is then noticed how large and persistent the winter lunar variation is, and how the series of observations may be split into two parts, each of which represents it. Thus we obtain:—

Phase of lunation............	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Winter variation (1855–65)	11·08	11·30	11·24	10·77	10·50	10·76	10·89	10·93
,, (1866–75)	11·29	11·44	11·40	11·00	10·57	10·17	10·69	11·18

Confining ourselves to the winter variation, which it thus appears is very prominent, it is then attempted to show that this variation (as far as we can gather from the Kew observations) appears to vary with the sun-spot period, being greatest at times of maximum spots and least at times of minimum spots, very nearly in the proportion of two to one.

Allusion is then made to certain recent researches of Mr. J. A. Broun, in which he shows that the sun appears to be one-sided as far as his action of certain kinds upon the earth is concerned. From this, and from the fact that while the moon appears to be concerned in the temperature-range,

its influence nevertheless appears to depend on the solar activity (a result similar to that obtained by Mr. J. A. Broun in the case of terrestrial magnetism), it is argued that even if there be a connexion between mean annual temperature-range and the sun-spot period, yet we cannot expect both periods to march together in the same way, inasmuch as the first is due entirely to the sun, while the latter appears to depend upon the moon as well.

This may possibly explain the redundant temperature fluctuation already alluded to; but the discussion of the subject must be further advanced before we can pronounce upon this point.

XVI. "On the Leaf-arrangement of the Crowberry (*Empetrum nigrum*)." By Hubert Airy, M.A., M.D. Communicated by Charles Darwin, M.A., F.R.S. Received May 8, 1876.

(Abstract.)

Pursuing the study of leaf-arrangement, the author finds that the crowberry of our moors (*Empetrum nigrum*) habitually exhibits a peculiar mode of variation in the arrangement of the leaves on different parts of the same twig. Out of fifty crowberry-twigs taken at random, only four (and these fragments) preserved the same arrangement throughout. In the remaining forty-six the leaf-arrangement was found to undergo a progressive change in ascending from the base of the twig to the summit—a change from a simpler order to others more complex. In general the basal order was that denoted by the fraction $\frac{2}{5}$; and this was found to pass most frequently into $\frac{2}{7}$, which in turn was found to pass into $\frac{2}{9}$, with or without an intermediate set of whorls of 4: $\frac{2}{9}$ generally passed into whorls of 5, sometimes into $\frac{2}{11}$, which was the most complex arrangement that was met with in this plant. The following is a list of the transitions found in the fifty specimens:—

Transition	from	$\frac{2}{5}$ (or $\frac{3}{8}$)	to $\frac{2}{7}$	occurred	22 times.
,,	,,	do. do.	,, $\frac{2}{9}$	,,	5 ,,
,,	,,	do. do.	,, whorls of 5	,,	1 ,,
,,	,,	whorls of 3	,, $\frac{2}{9}$	,,	2 ,,
,,	,,	$\frac{2}{7}$	,, whorls of 4	,,	10 ,,
,,	,,	$\frac{2}{7}$	,, a*	,,	2 ,,
,,	,,	$\frac{2}{7}$	,, $\frac{2}{9}$	,,	9 ,,
,,	,,	whorls of 4	,, $\frac{2}{9}$	,,	5 ,,
,,	,,	a*	,, $\frac{2}{11}$	,,	1 ,,
,,	,,	$\frac{2}{9}$	,, whorls of 5	,,	5 ,,
,,	,,	$\frac{2}{9}$	,, $\frac{2}{11}$	,,	1 ,,
,,	,,	whorls of 5	,, $\frac{2}{11}$	,,	1 ,,
				Total	64

* By a the author denotes a 4-, 6-, 10-ranked order, such as is found in heads of Dipsacaceæ.

In all these instances the striking peculiarity to be observed is that the arrangement passes from an order belonging to one phyllotactic series (*e.g.* from the order $\frac{2}{5}$ in the primary series $\frac{1}{2}$, $\frac{1}{3}$, $\frac{2}{5}$, &c.) to an order belonging to another phyllotactic series (*e. g.* to the order $\frac{2}{7}$ in the secondary series $\frac{1}{3}$, $\frac{1}{4}$, $\frac{2}{7}$, &c.), and that this is a phenomenon which could not result from uniform vertical condensation of the lower arrangement; whereas in other plants the ordinary transition is from one order to another of the *same* series (*e.g.* from $\frac{2}{5}$ to $\frac{3}{8}$, $\frac{5}{13}$, $\frac{8}{21}$, &c.), and is such as *would* result from uniform vertical condensation of the lower arrangement (as the author has shown in a paper read before the Royal Society on the 30th April, 1874: see Proc. vol. xxii. p. 298).

In order to examine the mode in which the above transitions were effected, the author constructed an instrument (described below) by means of which he obtained diagrams, pricked on paper, of the leaf-arrangement of the fifty specimens. On comparison of these diagrams it was found that, in every case where the details could be traced, a gradual dislocation (so to speak) took place between two adjacent spirals belonging to one of the two sets of spirals with least angular divergence, whereby the other set became deranged and gave rise to new sets having different numbers. Thus the transition from the arrangement $\frac{2}{5}$ (in which the spirals of least angular divergence are 2 in one direction and 3 in the other) to the arrangement $\frac{2}{7}$ (in which those spirals are 3 in one direction and 4 in the other) was effected by an apparent slip between two of the 3 spirals, whereby the 2 spirals became deranged and helped to form a new set of 4 spirals, which with the old persistent set of 3 formed the spirals of least angular divergence in the new arrangement $\frac{2}{7}$, while a new primary spiral arose, turning in an opposite direction to the old. (A diagram is required to make this clear.) In all these transitions the change of order is brought about not by any agency affecting all parts of the system uniformly all round the axis, but by a disturbance of the relative position of leaves along one special spiral tract.

The variations described by the Rev. G. Henslow (Trans. Linn. Soc. vol. xxvi. p. 647) as occurring in the leaf-arrangement of *Helianthus tuberosus* appear to be similar in character to those of *Empetrum nigrum*, but much more limited in range. The author has met with a few other instances of the same kind in other plants.

It is noteworthy, in the above transitions, that the primary spiral of the basal order is lost at the first change and gives place to a new one turning in the opposite direction, which, again, is replaced by another at the next change: in fact, in the crowberry, no rank or set of ranks is found to possess enduring value, but all are liable to derangement. They appear to be only the *local result of the geometrical conditions of mutual accommodation of contiguous leaves under mutual pressure* (*which brings with it the common need of economy of space*) *in the bud.*

This principle the author has before enunciated, with regard to the

more common forms of leaf-arrangement, in the paper above referred to (Proceedings of the Royal Society, 1874, vol. xxii. p. 298, &c.), and in a "Note on Variation of Leaf-arrangement," read before the British Association at Belfast in August 1874 (see Rep. Brit. Assoc. 1874, Trans. Sect. p. 128).

Having there drawn attention to two ways in which modification of leaf-arrangement appears mainly to have been brought about—namely, (1) *direct variation of number of vertical leaf-ranks*, producing the fundamental orders of different phyllotactic series, and (2) *subsequent variation of degree of uniform condensation*, effecting transition between different orders of the *same* series—he now shows (3), from examination of the crowberry, that transition between different orders of *different* series is brought about apparently by means of unequal condensation resulting in *spiral dislocation* between adjacent secondary ranks.

The instrument ("taxigraph") used in this research consists of a twig-holder which rotates in fixed bearings, the twig being held in the axis of rotation under a framed lens which slides on fixed guides parallel to the twig. Thus the angular position of any leaf is observed by aid of the rotation of the twig-holder, and the vertical position by aid of the sliding motion of the lens. The sliding motion of the lens is conveyed by cords and pulleys to a light frame carrying a strip of paper beneath the platform, which is the base of the instrument; and the rotary motion of the twig-holder is made, by aid of cords and pulleys, to draw a sliding pin-holder to and fro along a slit in the platform at right angles to the direction of motion of the strip of paper below. The pin, in any position of the slider along the slit, is made to pierce the strip of paper on pressing a lever. Every leaf on the twig is thus observed, and a corresponding pin-prick made in the paper. The longitudinal position of any pin-prick shows the vertical position of the leaf, and the lateral position of the pin-prick shows the angular position of the leaf. The whole figure represents the leaf-arrangement of the twig in radial projection, and is bounded laterally by two vertical lines corresponding to one and the same vertical line on the twig. By enlargement of the lateral dimension the arrangement is shown relatively foreshortened.

The author suggests modifications of this instrument by which it could be adapted to other forms of stem or twig; *e.g.* by causing the paper to rotate under the sliding pin-holder, and by giving the lens a quadrantal motion, it would be adapted to flat or conical systems such as the heads of Compositæ.

The paper was accompanied by the original fifty specimens (less one, lost), and by the fifty "taxigrams" obtained from them: two of these were especially referred to in illustration of the paper. The instrument used was also exhibited.

XVII. "Preliminary Note on the Use of the Piezometer in Deep-Sea Sounding." By J. Y. Buchanan, Chemist to the 'Challenger' Expedition. Communicated by Professor Williamson, Foreign Secretary R.S. Received June 14, 1876.

In order to determine the depth of the sea independently of the length of sounding-line used, piezometers filled with distilled water were frequently attached to the line along with the deep-sea thermometers. The combined effects of change of temperature and change of pressure were registered by a steel index of ordinary form. The temperature of the bottom-water being given by the deep-sea thermometer, the effect of temperature on the apparent volume of water in the piezometer could easily be calculated; and from the residual effect, the pressure, and therefore the height, of the column of water to which the instrument had been subjected could be deduced.

The piezometer did not differ materially from the ordinary ones used for the determination of the compressibility of liquids. A minute description of the fittings necessary for their safe use on the sounding-line cannot be given without reference to a drawing or model, and must therefore be postponed.

It is manifest that if the apparent compressibility of water is accurately known, we shall be in a position to determine, by means of our instrument and a deep-sea thermometer, the depth to which it has been sunk, independently of the lengths of sounding-line used; for the indications of the instrument depend solely on the temperature of the water at the depth in question, and on its vertical distance from the surface.

The determination of the effect of change of temperature on such an instrument does not demand explanation. It is, however, otherwise with the effects of pressure. In submitting an instrument of the kind to high pressures in an hydraulic machine, we encounter difficulty in accurately determining the pressure to which it is exposed, and also, although in a minor degree, in making our observations at the low temperature usually obtaining in deep ocean waters. I have therefore taken as basis for the determination of the apparent compressibility of water the results obtained when the instrument has been sent down on the sounding-line, either to the bottom or to intermediate depths, in positions where there has been no apparent disturbance from currents, and where the amounts of compression produced have been proportional to the depths recorded by the sounding-lines. Where currents are absent, and their presence is at once detected by the behaviour of the sounding-line, the depth, as determined according to the method of sounding adopted on board the 'Challenger,' gives an excellent measure of the pressure exercised on the instruments. As the variations in the temperature, the salinity, and the compressibility of sea-water with the depth have been thoroughly inves-

tigated for the soundings in question, the weight of a column of sea-water in any of these localities can be calculated with great accuracy.

The observations which have been taken as a basis for determinations of depth were made in the latter part of the year 1875 in the South-Pacific Ocean. They were twenty in number, and were made at depths varying from 500 to 2300 fathoms, and at temperatures varying from 1°·4 to 4°·03 C. The mean compressibility of water determined from these observations was 0·0008986 per 100 fathoms of sea-water, the extreme values being 0·000915 and 0·000882. Observations made at greater depths in the North Pacific gave as a mean of six observations at depths varying from 2740 to 3125 fathoms the value 0·000878, indicating a slight diminution in the coefficient of compression at very high pressures.

The effect of pressure being thus known, we are in a position, by comparing the indications of the instrument with those of a trustworthy deep-sea thermometer, to determine the absolute depth to which it has been sunk beneath the surface; and assuming the depth as indicated by the sounding-line to be correct, we should be able to determine the temperature at the depth in question from the indications of our instrument, and without the use of a thermometer. For the latter purpose, however, the instrument, as above described, is useless, because the dilatability of water at the low temperatures obtaining in deep water is so small as to be negligible compared with its elasticity.

The application, however, of the principle above indicated would manifestly present some very great advantages in the determination of deep-sea temperatures.

In the open ocean, where, as a rule, the temperature diminishes constantly as the depth increases, the Millar-Casella thermometer gives sufficiently accurate results. In the case of enclosed seas, or in the neighbourhood of ice, however, this is not always the case. In the Mediterranean, the Red Sea, and many of the seas of the Eastern archipelago, besides, possibly, large tracts both of the Atlantic and Pacific Oceans, the temperature decreases regularly down to a certain depth, which is different for different seas; and at all greater depths the Millar-Casella thermometer gives identical readings, indicating that the water is either at the same temperature or some higher one. In the neighbourhood of ice, layers of water are frequently met with at various depths whose temperature, being higher than that of the surface, is indicated by the maximum index of the Millar-Casella thermometer. Besides these layers there may be, and there probably are, others whose temperature is higher than that of the water immediately above them without reaching that of the surface, and their temperature would remain unrecorded. It would therefore be of great advantage if the piezometer could be adapted for the determination of temperatures at known depths. An efficient instrument for this purpose has been obtained by filling the bulb of the piezometer with mercury

instead of water. The portion of the stem in which the index moves is filled with water, and, as in the other, the open end dips into a cup of mercury. We have thus an instrument filled with a very large quantity of mercury and a very small quantity of water; and after immersion the position of the index shows the apparent volume assumed by this mixture under the combined influence of temperature and pressure. As far as the effects of temperature are concerned, the amount of water in the instrument is almost wholly negligible; but when the effect of pressure is considered, the apparent compressibility of mercury is so small, being little more than one fiftieth of that of water, that the pressure of even so small a quantity of water as can be contained in the graduated tube increases very materially the amount of contraction produced by pressure. The instrument, which has been in use since the beginning of November last year, and which is designated XVII. *a*, contains 256·61 grammes of mercury in the bulb and stem immediately above it; the volume of the part of the stem filled with water is 0·1935 c. c. The apparent contraction of this mass of mercury and water is 0·000581 cubic centimetres per 100 fathoms, and 0·0025 c. c. per degree respectively. A fall therefore of one degree in temperature produces the same effect as an increase of pressure equal to 430 fathoms of sea-water. Hence (and this forms the important peculiarity of the instrument) as long as the temperature of the sea does not increase with the depth at a greater rate than 1° C. per 430 fathoms, the instrument will record the temperature correctly. The ratio subsisting between the amount of temperature and the column of water, which produce the same effect on the apparent volume, is a constant for every instrument; in our one it is $\frac{1}{430}$. By altering only very slightly the amount of water, the sensibility to pressure is greatly increased or diminished, while that to temperature remains practically unchanged. As the instrument XVII. *a* was intended principally for bottom-waters, the above ratio ($\frac{1}{430}$) was considered sufficient, and it has proved practically useful. It must be remembered that the greater the value of this ratio is made, the greater is the error introduced into the determination of the temperature by any inaccuracy in the measurement of the depth.

By attaching a combination of one, or better of two, of each of these instruments close to the weight at the end of the sounding-line, the depth of the sea and the temperature of the water at the bottom at any locality can be accurately determined, provided that sufficient evidence is afforded, either by the presence of a sample of bottom in the sounding-tube, or by the rate at which the line runs out, that the instruments have been at the bottom; otherwise the depth which they have attained and the temperature at that depth will be correctly given. For this purpose it is necessary first to let the line run out until, from observations on its velocity, it is evident that the weight has reached the bottom; the length

of line which has so run out will give the depth approximately, but more or less in excess of the truth according to circumstances. Allowing for the contraction which would be produced by this depth in the case of the mercury piezometer, a first approximation to the temperature of the bottom-water is at once obtained; and it is sufficiently accurate for the purpose of correctly determining the contraction produced on the water piezometer by the change of temperature, and consequently for deducing the depth to which the instrument has been sunk. By now applying the more correct depth to the reading of the mercury instrument, we obtain the correct temperature, and if necessary the approximation might be carried still closer.

As an example of the use of the combined instruments, the observations made on the 29th February, 1876, may be taken. The position of the sounding was lat. 36° 9′ S., long. 48° 22′ W., and the depth by line was 2800 fathoms. The sea was quite calm, but there was a strong current setting to the south-east, rendering it probable that the depth, as determined by line, was considerably in excess of the true depth. The mercury instrument (XVII. *a*) registered 166·2 millims. In order to clear this reading for a depth of 2800 fathoms, we have to subtract 16 millims., and we obtain 150·2 millims. as the corrected reading, from which we determine the temperature to be +0°·2 C. The reading of the water instrument was 283·8 millims. Assuming the temperature to have been 0°·2 C., this would indicate that the water had suffered an apparent contraction, owing to pressure alone, of 0·1923 c. c., which would be produced by a column of 2480 fathoms of sea-water. Assuming now 2480 fathoms to be the true depth, we find the corrected reading of the mercury instrument (XVII. *a*) to be 152·1 millims., which indicates a temperature of −0°·5 C. The Millar-Casella thermometers gave the temperature as −0°·4. Assuming this as the correct bottom temperature, and reducing the reading of the water instrument (C. No. 1) accordingly, we find the contraction produced by pressure to be 0·1924 c.c., which agrees sensibly with that found on the assumption of the higher bottom-temperature of +0°·2 C.

It will thus be seen that the two instruments fulfil the conditions required of them; namely, that the one which is to indicate the temperature of the water shall be independent of great accuracy in the determination of the depth, and the one which is to indicate the depth shall be equally independent of accurate determination of the temperature; whilst by combining the results obtained by the two, an accurate determination is obtained both of the depth and of the temperature of the water at that depth.

XVIII. "On Organo-boron Compounds." By E. FRANKLAND, D.C.L., F.R.S. Received June 14, 1876.

In a former communication to the Royal Society* I described the action of zincethyl and zincmethyl upon boric ethylate, and showed that these organo-metallic bodies displace the ethoxyl ($Eto=C_2H_5O$) of the ethylate by the organic radical which they contain. Thus the action of zincmethyl upon boric ethylate is expressed by the following equation:—

$$\underset{\text{Boric ethylate.}}{\mathbf{B}Eto_3} + \underset{\text{Zincmethyl.}}{3\mathbf{Zn}Me_2} = \underset{\text{Boric methide.}}{\mathbf{B}Me_3} + \underset{\text{Zincic methoethylate.}}{3\mathbf{Zn}MeEto.}$$

It was also shown that spontaneously inflammable boric ethide ($\mathbf{B}Et_3$), when cautiously exposed to a slow current, first of air and then of oxygen, combines with two atoms of oxygen forming boric ethodiethylate ($\mathbf{B}EtEto_2$)—a liquid boiling with partial decomposition between 95° and 125° C., but capable of being distilled without change *in vacuo*. By contact with water, this compound was instantly transformed into boric ethodihydrate, thus:—

$$\underset{\text{Boric ethodiethylate.}}{\mathbf{B}EtEto_2} + \underset{\text{Water.}}{\mathbf{O}H_2} = \underset{\text{Boric ethodihydrate.}}{\mathbf{B}EtHo_2} + \underset{\text{Alcohol.}}{2EtHo.}$$

I have recently resumed the investigation of these compounds, and have obtained results of which the following is a preliminary description.

Boric Ethide.

Instead of using boric ethylate and zincethyl for the preparation of this compound, it may be obtained by passing a current of the vapour of boric chloride through zincethyl. The reaction appears to take place according to the following equation:—

$$\underset{\text{Boric chloride.}}{\mathbf{B}Cl_3} + \underset{\text{Zincethyl.}}{3\mathbf{Zn}Et_2} = \underset{\text{Boric ethide.}}{\mathbf{B}Et_3} + \underset{\text{Zincic ethochloride.}}{3\mathbf{Zn}EtCl.}$$

Diboric Ethopentethylate.

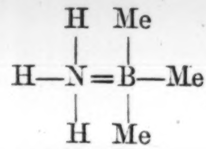
When equal molecules of boric ethylate and zincethyl are digested at about 100° C. for four or five hours, and then submitted to distillation, there is obtained a liquid distillate, which yields, on rectification, a considerable fraction boiling at 110° to 113° C., whilst zincic ethoethylate is left in the retort. On analysis this liquid was found to consist of equal molecules of boric ethylate and boric ethodiethylate. The following equation explains the reaction:—

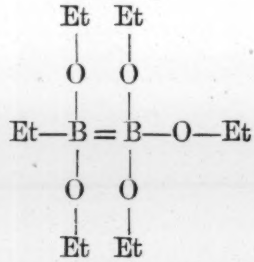
$$\underset{\text{Boric ethylate.}}{2\mathbf{B}Eto_3} + \underset{\text{Zincethyl.}}{\mathbf{Zn}Et_2} = \underset{\text{Diboric ethopentethylate.}}{\mathbf{B}EtEto_2, \mathbf{B}Eto_3} + \underset{\text{Zincic ethoethylate.}}{\mathbf{Zn}EtEto.}$$

The new compound is a colourless mobile liquid boiling at about 112° C., and condensing again unchanged. It possesses a slight ethereal odour and a sweetish taste. The specific gravity of its vapour, taken at 114° and 120° C., is 69 ($H=1$), which agrees exactly with the calculated

* Phil. Trans. vol. clii. p. 167.

density, on the assumption that, in passing from the liquid to the gaseous condition, the compound is broken up into its two constituent molecules—boric ethodiethylate and boric ethylate. On volatilization, therefore, it behaves exactly like ammonia boric methide ($\mathbf{N}H_3$, $\mathbf{B}Me_3$) described in my former paper; and this deportment suggests the possibility of a pentadic condition of the boron atom corresponding to that of nitrogen, phosphorus, and arsenic. On this supposition the compounds just mentioned would have the following constitutional formulæ:—

Ammonia boric methide 

Diboric ethopentethylate 

Brought into contact with water, diboric ethopentethylate is instantly decomposed, forming boric acid and boric ethodihydrate:—

$$\underset{\text{Diboric ethopentethylate.}}{\mathbf{B}EtEto_2, \mathbf{B}Eto_3} + \underset{\text{Water.}}{5\mathbf{O}H_2} = \underset{\text{Boric ethodihydrate.}}{\mathbf{B}EtHo_2} + \underset{\text{Boric acid.}}{\mathbf{B}Ho_3} + \underset{\text{Alcohol.}}{5EtHo.}$$

If the quantity of water used for this decomposition be small, the boric acid separates, almost completely, in minute crystals, whilst the boric ethodihydrate remains in solution, and may be obtained in the crystalline form by simple evaporation over sulphuric acid *in vacuo.*

Boric Diethethylate.

Intermediate between boric ethide and boric ethodiethylate, theory indicates the existence of a compound containing an atom of boron combined with two semimolecules of ethyl and one of ethoxyl,

$$\mathbf{B}Et_2Eto.$$

I find that this body is produced in large quantity when boiling boric ethylate is digested for several hours with twice as much zincethyl as that which was employed to form diboric ethopentethylate:—

$$\underset{\text{Boric ethylate.}}{\mathbf{B}Eto_3} + \underset{\text{Zincethyl.}}{2\mathbf{Zn}Et_2} = \underset{\text{Boric diethethylate.}}{\mathbf{B}Et_2Eto} + \underset{\text{Zincic ethethylate.}}{2\mathbf{Zn}EtEto.}$$

After rectification in an atmosphere of carbonic anhydride, the product boiled at 102° to 104° C.; but analysis showed that it persistently retained a small quantity of boric ethylate, or diboric ethopentethylate, from which it could not be freed by repeated rectification. When, however, diboric ethopentethylate was treated with the necessary amount of zinc-

ethyl, there was obtained, by distillation in an atmosphere of carbonic anhydride, a liquid product, which, on rectification, yielded a considerable fraction boiling between 102° and 103° C. This fraction gave, on analysis, numbers for carbon, hydrogen, and boron closely corresponding to those required by the above formula. Boric diethethylate is produced from diboric ethopentethylate by the following reaction:—

$$\underset{\text{Boric ethopentethylate.}}{\mathbf{B}EtEto_2, \mathbf{B}Eto_3} + \underset{\text{Zincethyl.}}{3\mathbf{Zn}Et_2} = \underset{\text{Boric diethethylate.}}{2\mathbf{B}Et_2Eto} + \underset{\text{Zincic ethethylate.}}{3\mathbf{Zn}EtEto.}$$

Boric diethethylate is a colourless, transparent, mobile, and neutral liquid, of ethereal odour and pungent taste. It boils at 102° C., and distils unchanged. Its vapour-density, taken at 135°·5 C., is 56·5, the theoretical specific gravity for a two-volume vapour of the above formula being 57 (H=1). In air, boric diethethylate is spontaneously inflammable, burning with a green and slightly luminous flame. Exposed to a slow current, first of dry air and then of oxygen, it is converted into boric ethodiethylate:—

$$\underset{\text{Boric diethethylate.}}{\mathbf{B}Et_2Eto} + O = \underset{\text{Boric ethodiethylate.}}{\mathbf{B}EtEto_2.}$$

Boric Diethohydrate.

When boric diethethylate is shaken up with water and then allowed to stand, a spontaneously inflammable ethereal liquid, similar in appearance to boric ethide, rises to the surface of the water. This liquid cannot be distilled without decomposition; but on being submitted to analysis, after drying over calcic chloride, it gave numbers for carbon, hydrogen, and boron closely approximating to those required by the formula

$$\mathbf{B}Et_2Ho.$$

Boric diethohydrate has an ethereal odour and a pungent taste, very unlike the intense sweetness of boric ethodihydrate. It is produced by the substitution of one semimolecule of hydroxyl for one of ethoxyl, according to the following equation:—

$$\underset{\text{Boric diethethylate.}}{\mathbf{B}Et_2Eto} + \underset{\text{Water.}}{\mathbf{O}H_2} = \underset{\text{Boric diethohydrate.}}{\mathbf{B}Et_2Ho} + \underset{\text{Alcohol.}}{EtHo.}$$

Boric Ethethylate-hydrate.

Submitted to a slow current of dry air, boric diethohydrate absorbs oxygen; and if the vessel in which the reaction is conducted be kept cool, colourless crystals gradually make their appearance. These crystals are soluble in the diethohydrate; but when the oxidation of the latter is completed, the entire liquid solidifies at a temperature slightly below 8° C., but immediately liquefies again when that temperature is exceeded. Submitted to analysis, the new body gave numbers for carbon, hydrogen, and boron, closely agreeing with the formula

$$\mathbf{B}EtEtoHo.$$

This compound is therefore obviously produced by the following reaction:—

$$\mathbf{B}Et_2Ho + O = \mathbf{B}EtEtoHo.$$

Boric diethohydrate. Boric ethethylate-hydrate.

Boric ethethylate-hydrate is a transparent and mobile liquid at ordinary temperatures, but below 8° C. it is a white crystalline solid, which has an odour like that of boric ethide and a strong pungent taste. It is neutral to test-papers, and is rapidly decomposed by water, exchanging its semimolecule of ethoxyl for hydroxyl, and forming boric ethodihydrate and alcohol; thus,—

$$\mathbf{B}EtEtoHo + \mathbf{O}H_2 = \mathbf{B}EtHo_2 + EtHo.$$

Boric ethethylate-hydrate. Water. Boric ethodihydrate. Alcohol.

Unlike boric diethethylate and boric diethohydrate, this body is not spontaneously inflammable; but, like the latter, it cannot be distilled at the ordinary atmospheric pressure without undergoing decomposition.

I am now engaged in submitting the organo-boron compounds and their derivatives to a thorough investigation, and hope shortly to have the honour of laying before the Royal Society further details respecting this family of organic compounds.

In conclusion I have much pleasure in expressing my thanks to my assistant, Mr. J. M. Cameron, for the efficient aid which he has afforded me in this work.

XIX. "A Contribution to Terrestrial Magnetism." By Vice-Admiral Sir Charles Shadwell, K.C.B., F.R.S. Received June 15, 1876.

(Abstract.)

In this paper the author records the results of a series of observations for the "Magnetic Dip" taken during the voyages of H.M.S. 'Iron Duke' in the eastern seas, visiting China, Japan, and adjacent places, in the years 1871–75.

The dip-circle employed was furnished with two needles, and the concluded dip recorded is the mean result of the observations by both needles.

The observations are sixty-six in number, taken at forty-two places, opportunities having been frequently afforded of repeating them on successive visits to the same station. In some instances the observations were repeated after an interval of from twelve to fourteen years, at stations formerly visited by the author on a previous visit to China and Japan (1857–59), thus affording data for the deduction of the approximate values of the secular change of this element. Moreover some of the observations having been taken at off-lying stations to the eastward of the coast-line of the Asiatic continent, data have been obtained for

determining to a certain extent the general direction of the isoclinal lines by graphic projection.

The observations are arranged in two tables—Table I. recording those taken during the outward and homeward voyages to and from Singapore, and Table II. containing those made in the eastern seas from Singapore up to Nicolaevsk on the River Amur. They are arranged in order of the latitudes of the places visited; but in all cases where the observations have been subsequently repeated at the same station, all those taken at the same place are noted in the Table in succession.

Finally, the author has added a few remarks suggested by a comparison of these observations with those formerly taken by him during his previous visits to China and Japan, 1857–59.

XX. "Experiments on Contact Electricity between Non-Conductors." By JOSEPH THOMSON, Student at the Physical Laboratory of Owens College. Communicated by B. STEWART, F.R.S. Received May 23, 1876.

It was observed that when a plate of copper was lifted from a plate of glass the copper was electrified, and also that when a plate of glass was lifted from a plate of wax the glass was electrified, care being taken to have as little friction as possible; it was afterwards found that the former experiment had already been made by Fechner (see Wiedemann's 'Galvanismus,' page 21), who also tried lifting copper from sulphur and got the same effect; although the plates were lifted as carefully as possible, yet it was not certain that friction had been entirely got rid of, so the following experiments were made to show that there is an electrical displacement when two non-conductors or a conductor and a non-conductor are put in contact without friction.

The arrangement used was as follows :—

Glass rods, AB, CD, EF, GH, were fixed in a wooden frame ACGE; round these rods silk threads, BF, DH, were wound; an aluminium needle carrying a mirror, M, was hung by a silk thread from a brass rod, T, fastened in the wooden frame; a wire from the needle dipped into a glass vessel, N, containing sulphuric acid; a small magnet was fastened to the back of the mirror, and a glass case was placed over the whole; outside the glass case were magnets, by means of which the position of the needle was regulated; a wire also from the outside dipped into the vessel N, and was used to charge the needle with electricity; positive electricity was got from an ordinary electrophorus, negative from an electrophorus in which the resin was replaced by a plate of glass which was excited by silk. If wax and glass were the substances experimented on, a cake, OQRP, was made, one half of which, OSQ, was glass, the other half, RPS, being wax; the junction of the wax and glass was parallel

to OQ, the wax sticking fast to the glass: this cake was then placed on the silk threads under the needle, and it was found possible to bring the

needle into such a position that when it was charged with positive electricity it was deflected from the glass part of the cake, when charged with negative it was attracted towards it. In order to get rid of any electricity which might have got on the cake in the making, the cake was made the day before it was placed on the threads, and the experiment was made at least a day, sometimes a week, after putting the cake on the threads; pieces of glass and sulphur which had been treated in as nearly as possible the same way as those of which the cakes were made were taken and placed separately on the threads, but no electricity could be detected on them.

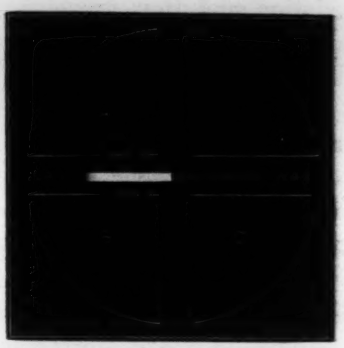
The following cakes were tried:—

Glass and wax	glass +, wax − ;
Glass and resin..........	glass +, resin − ;
Glass and sulphur	glass +, sulphur − ;
Glass and solid paraffin ..	glass +, paraffin − ;
Zinc and sulphur	zinc +, sulphur − ;
Sulphur and vulcanite	vulcanite +, sulphur − ;

the series so far being in the same order as the frictional series: this seems to suggest that the electrical displacement which takes place when two non-conductors are put in contact acts as a predisposing cause, in

virtue of which the work done by rubbing them together is converted into electrical separation.

The following experiment, suggested by Dr. Schuster, was also tried: the needle was taken out of a Thomson's quadrant electrometer and replaced by a needle, half of which was sealing-wax and the other half



glass; the quadrants C and D were connected with the earth; and it was found possible, after the needle had stood undisturbed for ten days, to set the needle in such a position that when the quadrants A, B were charged with negative and positive electricity respectively, the needle rotated in the direction of the hands of a watch, and when A and B were charged with positive and negative electricity respectively, the needle rotated in the opposite direction.

When two non-conductors, A, B, are placed in contact, the electricity will not be distributed through the whole of A and B; and if the displacement is from B to A and parallel to the sides of the cake, the only parts electrified will be the middle and ends of the cake; the A end of the cake will be positively and the B end negatively electrified; and if A has a greater specific inductive capacity than B the middle will be negatively electrified; if it has a less specific inductive capacity than B it will be positively electrified.

I intend to examine more substances, and to endeavour to make some quantitative measurements. The above experiments were made in the Physical Laboratory of Owens College, Manchester; and I have much pleasure in thanking Dr. Stewart, Dr. Schuster, and Mr. Kingdon for the assistance they have given me.

XXI. "Physiological Action of the Bark of *Erythrophleum guinense* (Casca, Cassa, or Sassy Bark)." By T. Lauder Brunton, M.D., F.R.S., and Walter Pye, M.R.C.S. Received June 15, 1876.

(Abstract.)

1. It diminishes oxidation, and thus prevents fresh vegetable tissues from communicating a blue colour to tincture of guaiac.

2. It does not hinder the development of the yeast-fungus nor the germination of seeds.

Penicillium grows freely in a solution of it.

3. A watery solution of the alcoholic extract prevents the development of *Bacteria*, but one of the watery extract does not do so.

4. It does not destroy the life of *Bacteria* or Infusoria. The motion of cilia is not arrested by it.

5. It arrests amœboid movement in leucocytes.

6. It has no action on fresh muscular fibre; but muscular tissue, when kept in a solution of the alcoholic extract for some days, undergoes extensive fatty metamorphosis, but does not become putrid.

It does not alter the sensibility of muscle to electrical stimuli, nor does it diminish its lifting power.

7. It has little, if any, poisonous action on the Invertebrata.

8. It has a comparatively slight action on fishes and frogs. The symptoms produced by its administration are failure of muscular power, preceded by irregular muscular movement.

9. On birds a small dose produces violent vomiting and irregular muscular movements, with difficult respiration. These symptoms are followed by loss of muscular power and death.

10. In cats and dogs the symptoms are restlessness, nausea succeeded by violent vomiting, spasmodic jerks of the limbs during locomotion, quickened respiration, staggering gait, inability to stand, and death generally during a convulsion of an emprosthotonic character, apparently connected with an attempt to vomit. Consciousness seems to be preserved to the last.

When injected subcutaneously, although it produces violent vomiting, it never purges; division of the vagi before its administration lessens or prevents the vomiting usually observed, as well as the other symptoms of distress; and in one instance a dose which would ordinarily have been speedily fatal produced no apparent effect in an animal thus operated on.

11. When injected into the stomach of a cat it produces violent vomiting and purging. Sometimes this is followed by recovery, in other cases by loss of muscular power and death.

12. Injection of atropia does not prevent death; and although in one case it prolonged life for two hours, in other instances it seemed rather to accelerate a fatal issue.

13. It causes the heart in frogs to pulsate more slowly; the ventricle

becomes irregularly contracted, leaving pouches over the surface, and finally is arrested in systole; the auricles contract for some time longer.

14. In cats the ventricle also becomes irregularly contracted before finally stopping.

15. In frogs it causes no rise of the blood-pressure in the aorta, but raises the oscillation of the mercurial column connected with the vessel to three times its previous height.

16. In cats and dogs moderate doses injected into the jugular vein first raise the blood-pressure without altering the rate of cardiac pulsation or the extent of oscillation at each beat; they then slow the heart by stimulating the roots of the vagus. The tension rises, notwithstanding the slowness of the heart's beats. An additional dose paralyzes the ends of the vagus in the heart, and quickens its pulsations; the pressure rises slightly. A further dose again slows the heart by acting on its ganglionic apparatus, and the beats sometimes fall as low as three per minute, three or four respirations occurring during each cardiac diastole. Notwithstanding the very slow action of the heart, the pressure may remain as high as 165 millimetres of mercury, a fact which indicates that the arterioles are in a state of extreme contraction. After the heart has ceased, the pressure falls very slowly. Slight pulsations of the ventricle occasionally occur when the thorax is opened.

17. Small doses do not seem to increase the excitability of the peripheral ends of the vagi to electrical stimuli; moderate and large doses paralyze these nerves.

18. After injection of casca into the veins of an animal completely narcotized by chloroform, electrical irritation of the central end of the divided vagus of one side, the other remaining intact, is followed after a short interval by marked slowing of the pulse, fall of blood-pressure, and increased oscillation.

19. When injected into the veins of a cat after division of the spinal cord opposite the second cervical vertebra, the blood-pressure rises to a greater height than is attained under other conditions.

20. When in the rabbit the sympathetic has been divided in the neck on one side, subsequent injection of casca into the jugular vein produces pallor of the recently congested ear of the side on which the division had been made.

21. When locally applied to the web of a frog's foot temporary slowing of the circulation was observed, but no alteration in the diameter of the blood-vessels.

When injected beneath the skin of the back of a frog it produces no visible effect on the vessels of the web.

22. It does not appear to possess any special action on reflex excitability.

23. In moderate doses it increases the secretion of urine at the same time that it raises the blood-pressure. Further doses diminish the secretion, while they raise the pressure yet more; and at the time when the pressure reaches its maximum the secretion of urine is entirely

arrested. When the pressure begins to fall the secretion of urine again commences. The urine collected after the recommencement of the secretion was not albuminous.

24. When injected into a loop of intestine it does not increase the secretion, nor does it produce any distinct congestion.

25. When applied to the eye it has no action on the pupil, nor does it cause congestion of the conjunctiva or lachrymation.

26. When administered to a pregnant cat it did not produce abortion.

27. The temperature of the body is not affected by administration of the drug.

XXII. "Note on Independent Pulsation of the Pulmonary Veins and Vena Cava." By T. Lauder Brunton, M.D., F.R.S., and Sir J. Fayrer, M.D., K.C.S.I. Received June 15, 1876.

In a former communication* we incidentally mentioned that in a rabbit killed by the injection of cobra-poison into the jugular vein we had observed the pulmonary vein pulsating after all motion had ceased in the cavities of the heart. We have since observed the same phenomenon three or four times under conditions which show that this pulsation is not due to the action of the cobra-poison with which the animal in which we first observed it had been killed. The following example will show the changes in rhythm observed in these pulsations.

A cat was chloroformed, and the vagi exposed and irritated by an interrupted current. Artificial respiration was kept up by air containing chloroform vapour, and the thorax was then opened, and a solution of atropia injected directly into the heart by means of a Wood's syringe. The vagi were again irritated, but without any effect being produced on the heart, the inhibitory apparatus in it being evidently paralyzed by the atropia. A solution of glycerine extract of physostigma was now injected into the heart in a similar way. The vagi were now irritated again, and the heart stood still, the effect of the atropia having been counteracted by the physostigma. After the irritation ceased the heart again commenced to pulsate.

Artificial respiration was now discontinued, but all the cavities of the heart continued to beat for a considerable time. The ventricles then stopped, but the auricles continued to beat. It was then noticed that the pulmonary veins in the right lung, which was exposed to view, were pulsating. The veins, as well as both auricles, pulsated at the rate of 119 per minute, but the contractions of the veins were not synchronous with those of the auricles. Both auricles next ceased to beat, but the pulmonary veins in both lungs continued to pulsate. The ventricles now began to beat again, while the auricles remained still. The ventricles pulsated at the rate of 8 per minute, while the pulmonary veins pulsated at the rate of 46 per minute; and no motion was perceptible in any part of the auricles.

* Proceedings of the Royal Society, 1874, vol. xxii. p. 125.

One hour and twenty minutes after the thorax had been opened, and about an hour and ten minutes after artificial respiration had been discontinued, the ventricle was still pulsating. Its rhythm was very irregular. After one beat a pause of half a minute followed, and then 37 pulsations all together. One hour and forty minutes after opening the thorax the inferior vena cava was noticed to be pulsating close to its entrance into the auricle. A contraction spread like a wave from the vena cava over the right auricle, and the appendix contracted after the auricle itself. The superior vena cava also pulsated close to the heart. The left auricle had ceased to pulsate a considerable time previously, and the ventricles had also stopped. After the auricles had pulsated for a while the ventricles again began. At one hour and fifty minutes after opening the thorax the inferior vena cava was still pulsating. In ten minutes more all movement had nearly ceased, and the observation was discontinued.

At one hour and fifty minutes after opening the thorax slight contractions of the diaphragm were noticed.

The striking points in this experiment are the contractions of the pulmonary veins and the vena cava independently of the heart, the long time during which they retained their irritability, and the continuance of their pulsations after the other parts of the heart had ceased. The pulsation of the pulmonary veins and of the ventricles at the same time, while the auricles were motionless, is also deserving of attention.

In another experiment we found the pulmonary veins pulsating in a cat killed by a blow on the head. We have also seen pulsation in animals killed in other ways; but the proportion of cases in which we have seen it to those in which we have not seen it is very small. On looking through several modern text-books of physiology, we have failed to find any mention of the rhythmical contractile power of the pulmonary veins and vena cava; but the earlier anatomists were well acquainted with it; and Haller* states that he has seen the pulmonary veins continue to pulsate for two hours, and that others had seen the vena cava pulsate for three hours while all motion in the other cavities of the heart had already ceased. Johannes Müller† has also observed contractions of the vena cava and pulmonary veins; and in young animals the contractions of the pulmonary veins extend as far as they can be followed into the lungs.

The importance of contraction of the vena cava and pulmonary veins in preventing reflux of blood into them during the contraction of the auricle, under circumstances when any hindrance is opposed to the free flow of its contents into the ventricle, is self-evident. Indeed Haller‡ says that it was supposed to exist by Senac, although he had not seen it.

* 'Elementa Physiologia,' 1757, tom. i. pp. 410 & 399; and 'Mémoires sur la Nature sensible et irritable des parties du corps animal,' 1756, tom. iv. p. 4.

† Müller's 'Physiology,' translated by Baly, 2nd ed. vol. i. p. 182.

‡ *Op. cit.* p. 410.

Especially in cases of valvular disease of the heart is it likely to be of great service; and we think it advisable to bring again before the notice of physiologists and physicians this power of the veins, which, although so long known, appears of late years to have been overlooked.

XXIII. "On certain Integrals." By W. H. L. Russell, F.R.S.

Received June 15, 1876.

The following are certain definite integrals which will, I hope, be found interesting:—

$$\int_0^\infty dz\,\epsilon^{-(r+1)z+x\epsilon^{-z}}=\frac{\epsilon^x}{x}\left(1-\frac{r}{x}+r(r-1)\frac{1}{x^2}-\ldots\right).$$

$$\int_0^\pi \frac{\theta\sin\theta\,d\theta}{1-x^2\cos^2\theta}=\frac{\pi}{2x}\log_\epsilon\frac{1+x}{1-x}.$$

$$\int_0^\pi \frac{\theta\sin\theta\,d\theta}{1+x^2\cos^2\theta}=\frac{\pi\tan^{-1}x}{x}.$$

$$\int_0^{\frac{\pi}{2}} d\theta\,\epsilon^{\cos^3\theta\cos 3\theta}\cos(\cos^3\theta\sin 3\theta)=\frac{\pi}{2}\sqrt[3]{e}.$$

$$\int_0^\pi d\theta\,\theta\sin\theta\sqrt{1+x^2\cos^2\theta}=\frac{\pi}{2x}\{x\sqrt{1+x^2}+\log_\epsilon(x+\sqrt{1+x^2})\}.$$

$$\int_0^\pi \theta\sin\theta\{\sqrt[3]{1+x\cos\theta}+\sqrt[3]{1-x\cos\theta}\}d\theta=\frac{3\pi}{4x}\{(1+x)^{\frac{4}{3}}-(1-x)^{\frac{4}{3}}\}.$$

$$\int_0^\pi \frac{\theta\sin\theta\,d\theta}{1+x^4\cos^4\theta}=\frac{\pi}{2^{\frac{5}{2}}x}\log_\epsilon\frac{1+x\sqrt{2}+x^2}{1-x\sqrt{2}+x^2}+\frac{\pi}{2^{\frac{3}{2}}x}\tan^{-1}\frac{x\sqrt{2}}{1-x^2}.$$

$$\int_0^{\frac{\pi}{2}}\log_\epsilon\cos\theta\{\epsilon^{x\sin 2\theta}\cos(x\cos 2\theta-2\theta)+\epsilon^{-x\sin 2\theta}\cos(x\cos 2\theta+2\theta)\}d\theta=\frac{\pi\sin}{2x}$$

$$\int_0^{\frac{\pi}{2}} d\theta\sqrt{(1+\alpha\cos^2\theta+\sqrt{1+(2\alpha+\alpha^2)\cos^2\theta}}=\frac{\pi}{2}\sqrt{\alpha+2}.$$

$$\int_0^{\frac{\pi}{2}} d\theta\frac{1+\epsilon^{\alpha\cos^2\theta}\cos(\alpha\sin\theta\cos\theta)}{1+2\,\epsilon^{\alpha\cos^2\theta}\cos(\alpha\sin\theta\cos\theta)+\epsilon^{2\alpha\cos^2\theta}}=\frac{\pi}{2(\epsilon^{\frac{\alpha}{2}}+1)}.$$

$$\int_0^{\frac{\pi}{2}} d\theta\sqrt{1+\epsilon^{\alpha\cos^2\theta}\cos(\alpha\sin\theta\cos\theta)+\sqrt{1+2\epsilon^{\alpha\cos^2\theta}\cos(\alpha\sin\theta\cos\theta)+\epsilon^{2\alpha\cos}}}$$

$$=\frac{\pi}{\sqrt{2}}\sqrt{1+\epsilon^{\frac{\alpha}{2}}}.$$

$$\int_0^{\frac{\pi}{2}} d\theta\frac{\epsilon^{\alpha\cos^2\theta}\cos\theta\cos(\theta-\alpha\sin\theta\cos\theta)-\cos^2\theta}{1-2\epsilon^{\alpha\cos^2\theta}\cos(\alpha\sin\theta\cos\theta)+\epsilon^{2\alpha\cos^2\theta}}=\frac{\pi}{4(\epsilon^{\frac{\alpha}{2}}-1)}.$$

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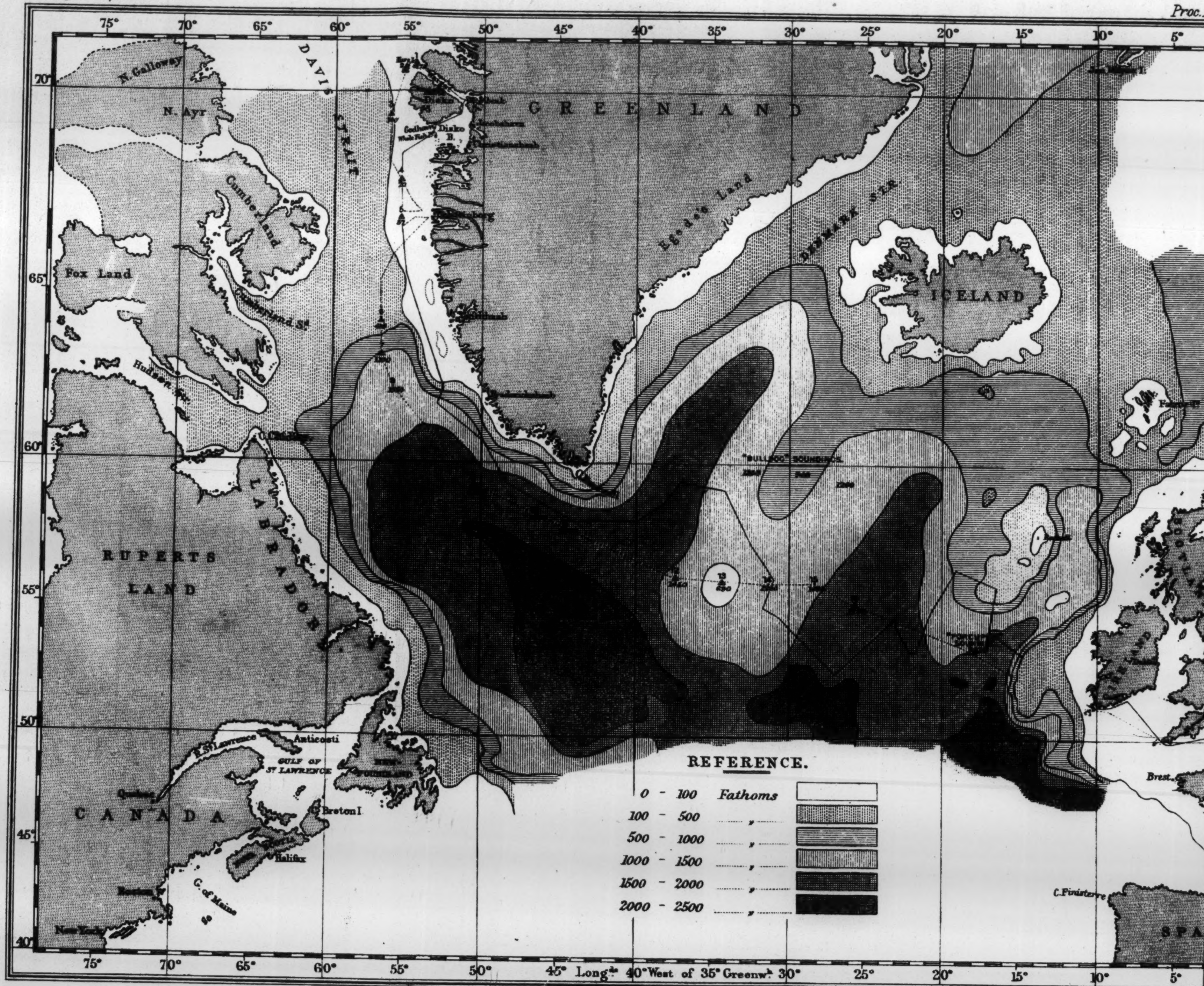
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N. Ayr
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DENMARK STR.
Disko B.
RUPERTS LAND
LABRADOR
CANADA
Anticosti
GULF OF ST LAWRENCE
NEWFOUNDLAND
Breton I.
Halifax
Quebec
Boston
G. of Maine
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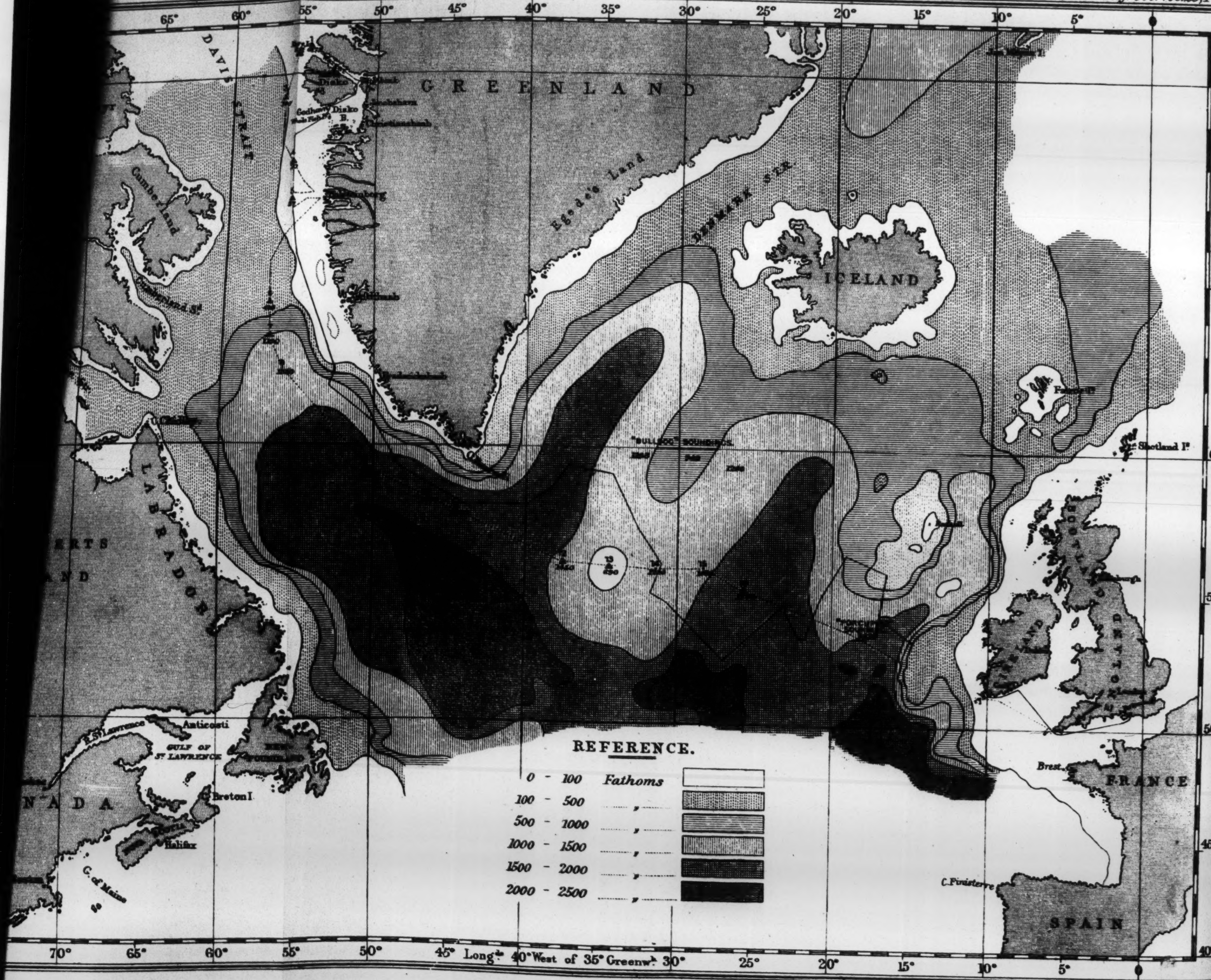

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XXIV. "Preliminary Report of the Biological Results of a Cruise in H.M.S. 'Valorous' to Davis Strait in 1875." By J. Gwyn Jeffreys, LL.D., F.R.S. Received June 12, 1876.

[Plates 2–4.]

On this occasion I propose to adopt the same course that was taken in presenting to the Royal Society the Preliminary Report of the scientific exploration of the deep sea in H.M. Surveying-vessel 'Porcupine' in the years 1869 and 1870, in both of which expeditions I took a part.

Narrative.

1. The Government having, at the instance of the Society and other scientific bodies, determined to equip and send out last year two ships (the 'Alert' and 'Discovery') on a North-Polar Expedition, and with these vessels the 'Valorous' frigate as a store-ship to accompany them as far as Disco in Davis Strait, it was considered desirable to make the last-named vessel available for sounding and dredging on her return voyage. Accordingly the following correspondence with the Admiralty took place, and will serve to explain the circumstances under which the scientific results now about to be noticed were obtained :—

"Council Minutes, April 15.

"Read the following letter from Dr. Carpenter :—

"University of London, Burlington Gardens, W., April 14, 1875.

"Dear Mr. President,—Since I wrote to you on the subject of the Arctic Expedition, the Admiralty has decided upon sending as a store-ship, not the merchant-vessel then contemplated, but a man-of-war of considerable tonnage. Although there will be no opportunity on the voyage *to* Disco for any scientific observation that would delay the progress of the Expedition, yet such opportunity will be available both while the ship remains at Disco and on the *return* voyage, which will be made at a time of the year most likely to be favourable. And I have reason to believe that the Admiralty authorities are quite willing that advantage should be taken of this opportunity, to such an extent as circumstances may permit, if the Council of the Royal Society should be of opinion that valuable results are likely to be obtained by the utilization of it.

"I have already pointed out to you that a Temperature-Section across the North Atlantic to Cape Farewell and Temperature-Sections, transverse and longitudinal, in Baffin's Bay are just what are needed to complete the survey of the thermal condition of the great oceanic areas that is being so admirably carried out by the 'Challenger.'—The same remark applies also to the zoological inquiry, the animal life of great depths in the North Atlantic and Arctic Seas being still very imperfectly known.

So strongly does Mr. Gwyn Jeffreys feel interested in carrying out this inquiry, that he has volunteered his services for the purpose; and I cannot but believe that the Council will cordially recommend the acceptance of his offer by the Admiralty.

"In the event of this arrangement being carried out, it will, I believe, be better that the Council should not ask the Government for more than accommodation and rations for Mr. Gwyn Jeffreys and for a Junior Assistant whom he wishes to take with him; and that the Council should provide out of the Donation Fund the sum which will be required for the payment of the Assistant, and for providing the requisite supply of jars, spirit, &c. Mr. Jeffreys and I estimate this amount at £120.

"I would venture to suggest, therefore, that the Council should represent to the Admiralty the importance of taking advantage of this opportunity of carrying out a Physical and Biological exploration of the Deep Sea between the British Isles and Cape Farewell, and also in Baffin's Bay; and that the Admiralty be requested to institute a systematic series of Temperature Soundings, and to give such facilities as they may be able for deep dredging. Also that they provide accommodation and mess for Mr. Jeffreys and his Assistant.

"Believe me, dear Mr. President,
"Yours faithfully,
"WILLIAM B. CARPENTER."

"*Dr. Hooker, P.R.S.*"

"Resolved,—That a Letter be addressed to the Admiralty applying for accommodation and rations for Mr. Gwyn Jeffreys and his Assistant on board the 'Valorous' Store-ship in the Arctic Expedition; and that £120 from the Donation Fund be granted for payment of the Assistant and provision of materials."

[Copy.]

"Admiralty, 28th April, 1875.

"SIR,—With reference to your letter of the 15th instant, I am commanded by my Lord Commissioners of the Admiralty to acquaint you that there will be no objection to the Naturalist who may be selected by the Royal Society proceeding with his Assistant to Disco and back in H.M.S. 'Valorous,' and that the Lords Commissioners of H.M. Treasury have sanctioned the cost of naval rations for these gentlemen during the time they are absent with the ship; they will, however, be expected to defray certain mess expenses, and My Lords request it to be understood that any other expenses connected with these gentlemen accompanying the expedition cannot be borne by Government.

"2. I am at the same time to state that it is the desire of their Lordships, in acceding to the request of the Royal Society, that duplicates of

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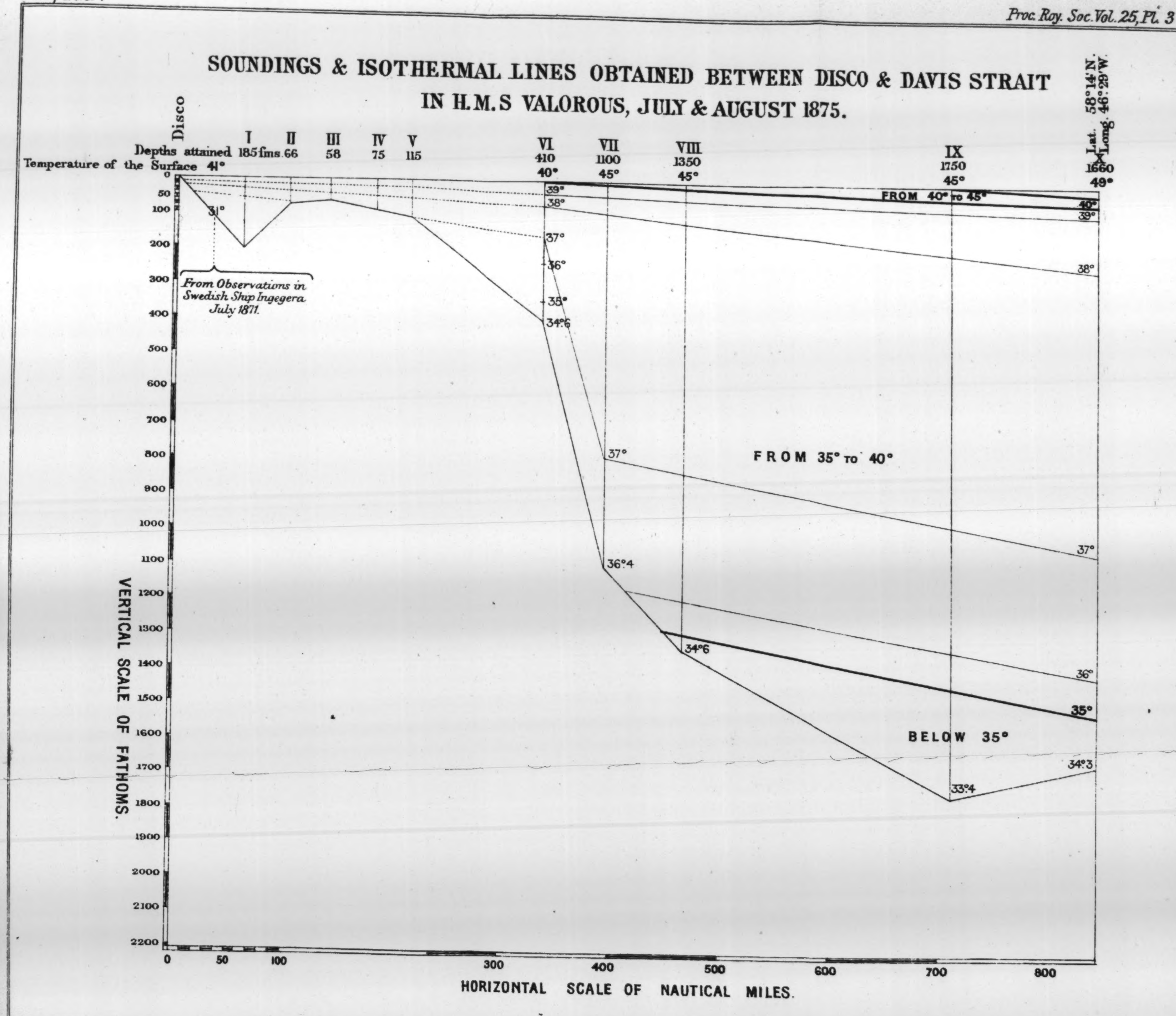

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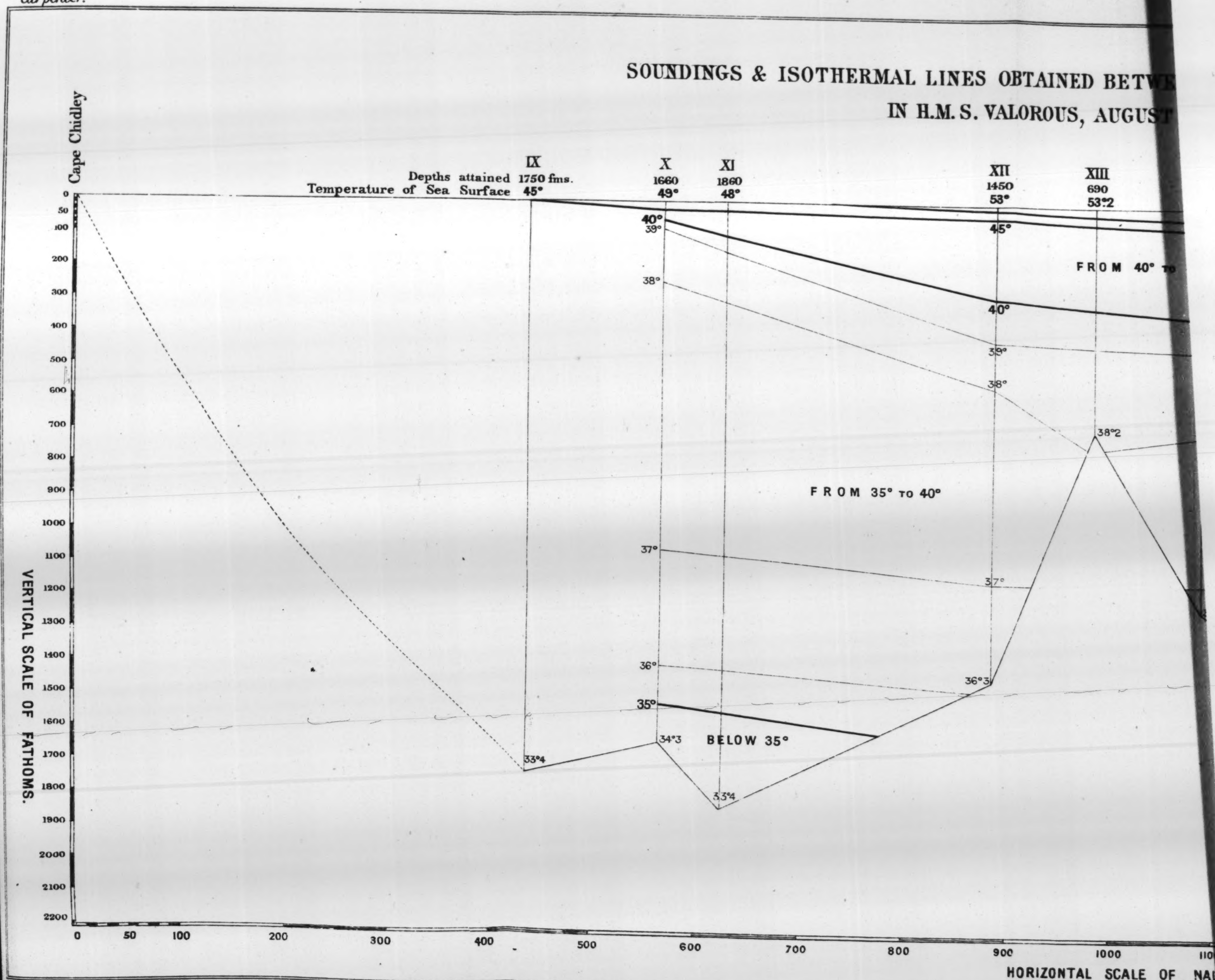
Carpenter:



SOUNDINGS & ISOTHERMAL LINES OBTAINED BETW
IN H.M.S. VALOROUS, AUGUST
Cape Chidley
Depths attained 1750 fms.
Temperature of Sea Surface 45°
IX
X
1660
49°
XI
1860
48°
XII
1450
53°
XIII
690
53°2
40°
39°
38°
45°
40°
39°
38°
38°2
FROM 40° TO
FROM 35° TO 40°
37°
37°
36°
36°3
35°
34°3
33°4
33°4
BELOW 35°
VERTICAL SCALE OF FATHOMS.
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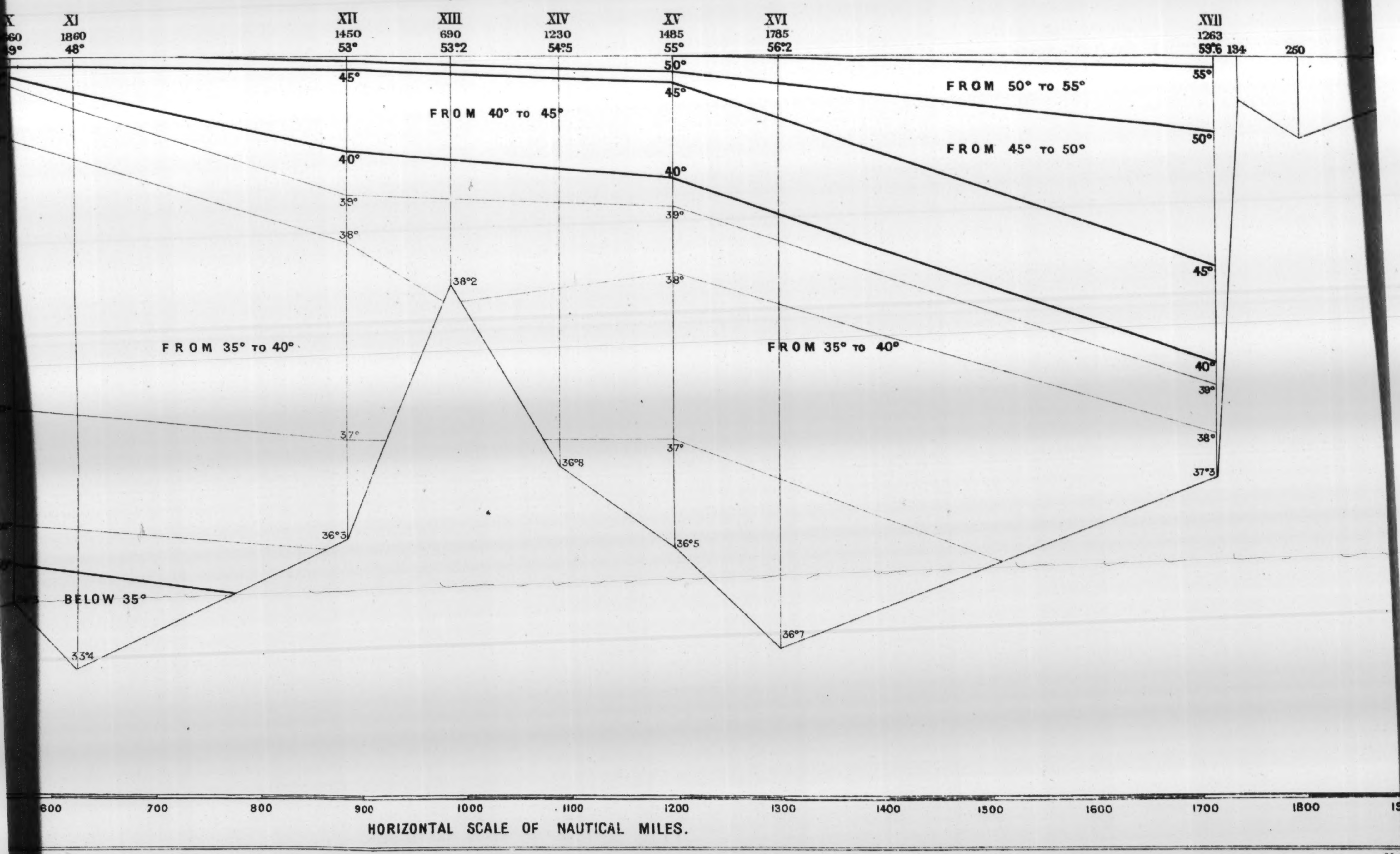
HORIZONTAL SCALE OF NAU

SOUNDINGS & ISOTHERMAL LINES OBTAINED BETWEEN DAVIS STRAIT & ENGLAND
IN H.M.S. VALOROUS, AUGUST 1875.



ISOTHERMAL LINES OBTAINED BETWEEN DAVIS STRAIT & ENGLAND
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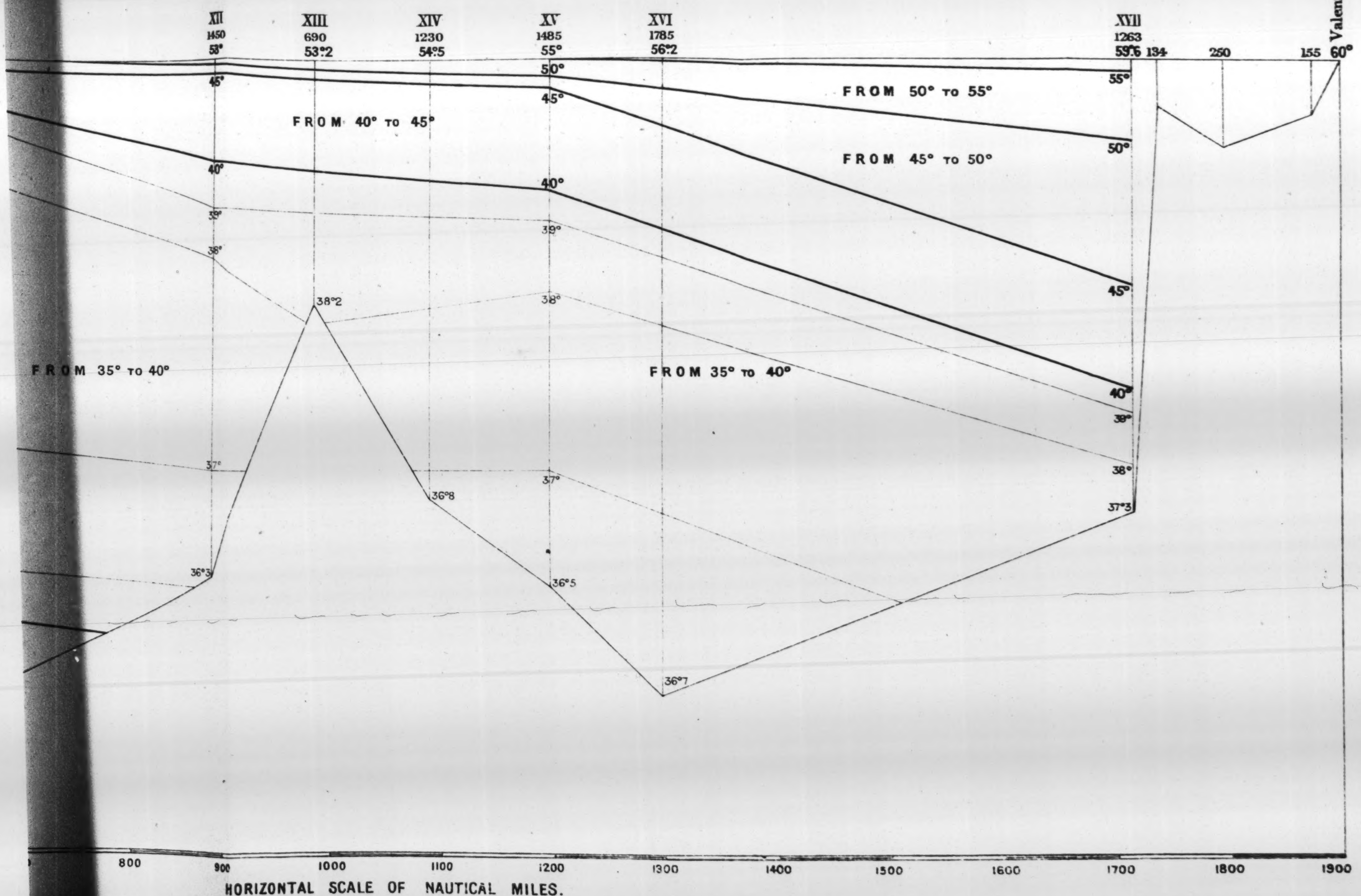

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the specimens of Natural History obtained may be placed at the disposal of the Admiralty for transmission to the British Museum.

"I am, Sir,
"Your obedient Servant,
(Signed) "VERNON LUSHINGTON."

"*The Secretary to the Royal Society,*
Burlington House."

2. The 'Valorous' is a paddle-wheel steamer of 1257 tons and 400 horse-power, commanded by Capt. Loftus F. Jones, and having a crew of 248, including officers, seamen, and marines. She carried six guns; but these had been taken out to make room for extra cabin accommodation and stores. We had a donkey-engine and a good supply of ropes, dredges, with nets, accumulators (or indicators), and various other apparatus for sounding, dredging, and collecting specimens. I was fortunate in being able to secure the services of Mr. Herbert P. Carpenter as my assistant. He is a son of Dr. Carpenter, and accompanied his father in the short deep-sea exploration of H.M.S. 'Lightning' in 1868. He was of the greatest assistance to me, and showed such scientific abilities of a high order, that I shall not be wrong in predicting for him a most successful and distinguished career as a naturalist. I would here also take the opportunity of acknowledging my sincere obligations to Capt. Jones, not only for his personal attentions, but for the highly satisfactory manner in which he carried out the scientific operations in the face of considerable difficulties. The other officers also deserve my best thanks for the unvaried kindness of their companionship.

3. On Saturday, the 29th of May, at 4 P.M., we left Spithead in company with the 'Alert' and 'Discovery.' Our vessel was deeply laden with coal and provisions for the other vessels; and while sailing we had some difficulty in keeping up with them. After touching at Cork to post letters for the squadron and take in more coal, and my going on board the 'Alert' at the entrance of Bantry Bay to greet my friends, Capt. Nares, Commander Markham, and Capt. Feilden (the naturalist), and to make the acquaintance of the other officers, we parted company with the Arctic ships on the 5th of June, and did not again rejoin them until we met at Disco on the 6th of July. On our passage we encountered very heavy weather, with strong persistent north-westerly winds, which greatly retarded our course, and made every one extremely uncomfortable. One day we gained 6 miles only, and another day lost 8. We had, in nautical phrase, "a regular dusting."

4. The only natural-history work we could do on the outward voyage was, during the intervals between storms, to use the towing-net. In latitude 58° 59′ N., longitude 34° 13′ W. (between 200 and 300 miles east of Cape Farewell), we caught some floating masses of pulpy greenish matter, which at first looked like an oceanic sponge, but has since been

made out by Professor Dickie to be an undescribed Diatom, and named by him *Synedra Jeffreysi*. An account of this remarkable organism will be appended to the Report. We afterwards found it covering to a considerable extent all that part of the North-Atlantic Ocean. It contained within its meshes numerous living *Globigerinæ* in different stages of growth, proving that *Globigerina* inhabits the surface of the sea. During one of the gales a wave larger than usual broke over the bow and washed on board a young cuttlefish of the same species (*Leachia borealis*, Steenstrup) as that which I had taken with the tow-net in the first cruise of the 'Porcupine' off the western coast of Ireland. Occasionally two tow-nets were put out at the same time, each at the end of a spar, with a guide-line. One of these spars was lashed to the cat-head of the bow anchor, and the other to the starboard paddle-box. The nets were thus kept clear of the ship's refuse and of the wash of the paddle. Attached to the stalk of a floating *Laminaria* was a cluster of the egg-capsules of *Buccinum Grœnlandicum*, with the spawn of a Nudibranch (probably *Doris repanda*), *Spirorbis borealis*, and a sessile calcareous Polyzoon, besides countless numbers of a microscopic mite, which swarmed everywhere and appeared to be busily engaged in eating the outer layer of the seaweed as well as the spawn of the Nudibranch and the polyparies of the Polyzoon. This very curious parasitic mite could only be detected by the aid of a microscope. The oceanic Fauna and Flora offer a vast and inexhaustible field for scientific investigation.

5. Having entered Davis Strait and approached the "Boreæ finitimum latus," we met with several icebergs and a quantity of loose pack-ice, which must have been brought from East Greenland, if not from Spitzbergen, round Cape Farewell. We were obliged to give the pack-ice a wide berth; and, notwithstanding the greatest care, our paddles did not escape some damage. But I will not diverge from my biological text, nor say any thing about glacial phenomena; although I must confess that the beautiful and impressive spectacles of this nature which I witnessed in my voyage to the arctic regions both at sea and on land cannot be effaced from my memory. We had several showers of hail and snow; and on the night of the 3rd July the temperature of the air fell to $29\frac{1}{2}°$ Fahr. or $2\frac{1}{2}°$ below freezing-point. We reached Godhavn in Disco Island on the 4th of July, after a run of 37 days.

6. At Godhavn the rocks on the shore were covered with a stunted variety of *Littorina rudis*, closely resembling a variety of the same species which I found in brackish water on the banks of the river Deben near Sutton. The periodical melting of the ice and snow in Greenland would cause an admixture of fresh and salt water similar to that of the river-water and the sea on the coast of Suffolk. The arctic form has been considered a distinct species and named *Grœnlandica* by Menke, Möller, and Mörch, *Davidi* by Bolten, and *castanea*

by Deshayes; Fabricius mistook it for the *Turbo littoreus* of Linné. It is the "Grönlandische Mondschnecken" of Chemnitz. During our stay at Godhavn we dredged now and then in one of the ship's cutters at depths of from one to eighty fathoms. The results were to me very interesting; for opportunities were thus afforded me of observing in their native habitat the same arctic Mollusca which I had long studied at home in our posttertiary and glacial deposits. *Cardium Islandicum*, *C. Grœnlandicum*, and *Tellina calcaria* were the most common species at Godhavn and occurred at all depths. On the land I found *Vitrina pellucida* of Müller (*V. angelicæ*, Beck), not at the roots of *Archangelica officinalis*, but among moss and various water plants at the sides of small streams formed by the melting of ice. The pursuit of this branch of science was very disagreeable, by reason of the swarms of stinging gnats or mosquitoes which infested the low grounds. The weather was rainy and foggy, with occasional sunshine; the land was treeless and had a gloomy aspect. On the 13th of July the thermometer showed 78° in the sun; one day at Ritenbenk Kulbrud it was 81° in the midst of icebergs.

7. No time was lost at Godhavn in transferring the stores of coal and provisions to the Arctic ships; and we had also to give them some of our boats to replace those which had been lost in the gales on the outward voyage. All the ships left northward on the 15th of July; and after touching at the Danish settlement of Ritenbenk in Waigat Strait, we reached the Kulbrud, where we had to procure by digging a supply of coal, being a kind of lignite, from the Miocene strata which composed the cliffs. By dint of hard and continuous work 105 tons of this coal were got in the course of four days. We had a little boat-dredging in from 15 to 25 fathoms near the cliffs, among melting icebergs and the mud brought down by glacier-streams. There was no diminution of life. The Arctic ships here left us for their destination, and parting signals were exchanged. Mr. Clements Markham (who went out in the 'Alert' with his cousin, Commander Markham) came on board the 'Valorous' and made the return voyage with us. He was an agreeable accession to our small party in the Captain's cabin. Our position was at this time critical, in consequence of the narrowness of the Strait and the passage of numerous large icebergs. Some of these had been aground; but as they melted and became lighter they floated and whirled about the ship so as to endanger the paddles. We left at midnight on the 21st, and resumed our voyage northward, so as to get out of the Strait at the upper end of Disco Island.

8. On leaving the Strait we got our first dredging from the ship in lat. 70° 30′ N., long. 54° 41′ W., at a depth of 175 fathoms. The tangles or swabs brought up several beautiful specimens of *Asterophyton eucnemis* (*Asterias caput-Medusæ* of Fabricius), besides other starfishes; and the dredge had a goodly cargo of mud. The dredge weighed 78 lbs. When it was lowered a small guide-rope with a running noose or "guy" was

attached to the dredge-rope, and held by a man on the fore bulwark, in order to regulate the descent of the dredge and afterwards to assist in its being hoisted on board; and great care was taken that the swabs were let down before the dredge, so that they did not get into the mouth and choke it. Other particulars of a dredging-operation have been already given in the Preliminary Reports of the 'Porcupine' Expedition, and published in the 'Proceedings' of the Society. A small portion of the fore deck behind the capstan was enclosed by a sail and ropes for our sifting and examination of the mud; three large tubs made for this purpose, and nested or packed one within another for the convenience of stowage, were filled with sea-water; a tarpaulin was spread out, a seat and rough table provided, and our sieves (a nested set of five and a globe-sieve) were at once used to sift the mud. An array of basins, soup-plates, jam-pots, and glass bottles with other apparatus were in order on the table. Mr. Carpenter undertook the sifting, and I examined the results and reserved some of the animals for more leisurely description. The only Mollusca worth special notice were *Terebratella Spitzbergensis* and fragments of *Fusus Sabini* of Gray. At 4 P.M. we sounded and dredged again in 85 fathoms. The dredge came up empty; but on one of the swabs was a fine specimen of *Antedon Eschrichti.*

9. Steamed slowly down the eastern coast of Davis Strait, and dredged on the 28th in 100 fathoms, with no particular result as regards the Mollusca. The next day we dredged twice on the Upper Torske Bank (lat. 67° 50′ N., long. 55° 27′ W.) in 20 fathoms, where a great many of the usual Arctic Mollusca were obtained. On the 26th dredged twice in 60 fathoms. These last two hauls were very productive, and yielded among other Mollusca the following species:—*Montacuta Dawsoni, Tellina inflata, Pilidium radiatum*, and several species of *Pleurotoma*, including *P. declivis* and a remarkable variety of *P. Trevelyana*, which I propose to name *Smithii*, after Mr. Edgar Smith of the British Museum.

10. My narrative must now allude, although briefly, to an anxious state of things which took place on the 27th of July, when we were about to enter the natural harbour of Holsteinborg for ballast. The weather was foggy, and we were therefore going slowly and cautiously under steam. Without our having any suspicion of danger we suddenly found ourselves stranded on a sunken reef of rocks about ten miles from Holsteinborg, which had not been laid down on the chart. The wind was freshening, and the ship was continually bumping and straining; but most providentially the tide was rising. After a suspense that lasted several hours the bow became free, and soon afterwards the ship floated and was got safe to Holsteinborg. I cannot sufficiently express my admiration of the prompt and skilful manner in which the Captain behaved in this trying emergency. My feelings at the time were those of intense disappointment; because I feared that, even if we escaped with our lives,

the scientific object for which I had undertaken such a long and uncomfortable voyage would probably be frustrated.

11. While the divers were at work under water examining the keel and timbers of the ship and fixing iron plates, and the carpenters were building a bulkhead at the bow, where the most dangerous leak existed, some of the officers went on shore trout-fishing; and Mr. Broad (the Navigating Lieutenant) most obligingly brought me specimens of two very curious kinds of Crustacea from a pool of fresh water in a neighbouring island, viz. *Apus glacialis* and *Branchipus paludosus*; the *Apus* is allied to the king crab or *Limulus*, and consequently to the extinct Trilobites. Mr. Carpenter and I had some boat-dredging in shallow water. *Rhynchonella psittacea* and *Pecten Islandicus* * were here the most common Mollusca; and a living specimen of a new species of *Pleurotoma* (*P. rubescens*, J.) was discovered in 10 fathoms. This last-named species is described in the footnote†.

We left Holsteinborg on Sunday the 8th of August, and did not again touch land until we returned to Plymouth. I cannot omit here publicly thanking Inspector Krarüp Smith, and Governors Fencke and Lassen, for the great kindness and hospitality shown by them at Godhavn, Ritenbenk, and Holsteinborg.

12. The variation of the compass is so great in these parts that the ship was steered in a north-westerly direction, although she was actually going south. We recrossed the arctic circle in 66° 32′ N. lat. 10th August. Foggy, damp, and sunless. Thermometer 35° only. Sounded in 410 fathoms, took serial temperatures, and dredged. The results of this our first deep-sea dredging in Davis Strait were scanty, but interesting in every department of the marine Invertebrata. Among the Mollusca were *Eulima stenostoma* and *Fusus fenestratus*, both new to Greenland and having

* *Pecten Islandicus* is excellent eating, and not inferior to *P. maximus*, which is sold as a delicacy by our best fishmongers.

† *Pleurotoma rubescens*[1], Jeffr.

Body yellowish white; *tentacles* short; *eyes* small, on stalks which are united with the tentacles; *foot* long, squarish and double-edged in front, rounded behind; *canal-fold* short; *operculum* small, ear-shaped, and elongated.

Shell oval, solid, opaque, of a dull hue: *sculpture*, rather strong, rounded but sharp and curved longitudinal ribs, which on the body-whorl extend to the suture and reach rather more than halfway down; there are twelve on each of the last two whorls; the whole surface is covered with numerous fine, irregular, impressed spiral striæ, which cross the ribs; the uppermost whorls are fretted: *colour* pale purplish red: *spire* short, ending in a somewhat abrupt and blunt point: *whorls* 5–6, convex, regularly increasing; the last occupies about three fifths of the shell: *suture* deep: *mouth* oval, rather wide; length rather exceeding one half that of the shell: *canal* short, wide, nearly straight: *outer lip* flexuous, slightly incurved, with a sharp edge: *labial notch* shallow and indistinct, placed near the top of the body-whorl: *inner lip* broad, somewhat excavated, and polished: *pillar* flexuous. Length 0·35; breadth 0·125.

One specimen only, from 10 fathoms at Holsteinborg. Unlike any European or North-American species.

[1] Inclined to blush.

a range of distribution from Norway to the Bay of Biscay. Caught by the tangles was a fine Gorgonian, which Mr. Norman considers a new species of *Mopsea*. The bottom-temperature was 34°·6. Next day we sounded, and dredged in 1100 fathoms. A live specimen of that remarkable Brachiopod *Atretia gnomon*, besides other Mollusca familiar to me from the 'Porcupine' expeditions (e.g. *Nucula reticulata*, J., *Limopsis aurita*, *Axinus eumyarius*, and *Dentalium candidum*, J.), were scientifically important captures.

13. Sounded on the 12th of August in 1350 fathoms; but no dredging, because the wind was fair. All the pumps were obliged to be kept going day and night. If a small brig had been in the first instance despatched to Disco with a supply of coal for the Polar Expedition we need not have entered Waigat Strait, and lost so much time in digging fuel of an inferior quality on an exposed and dangerous coast, nor have thus unnecessarily consumed our own provisions; and as ballast could have been taken in at Godhavn, the unfortunate accident which crippled the ship might have been avoided. In that case the instructions of the Admiralty, in compliance with the request of the Society for scientific investigations, could have been completely and satisfactorily carried out.

14. Sounded and dredged on the 14th of August in 1750 fathoms, at the entrance of Davis Strait. The dredge brought up nearly 3 cwt. of soft yellowish-brown mud. The Mollusca comprised *Siphonodentalium vitreum*, *S. Lofotense*, and several undescribed forms, most of which I had found at less depths in the 'Porcupine' Expedition of 1869. A remarkable new genus of Echinoidea occurred, which I at first took to be a *Pourtalesia*; but Mr. Norman will give an account of it, as well as of a Crustacean (*Leucon longirostris*) which Dr. Sars described from a fragment procured in the 'Josephine' Expedition off the Straits of Gibraltar. Dr. M'Intosh has also made out a new genus of Annelids under the name of *Tachytrypane*.

15. We now got into the Atlantic, and on the 16th and 17th of August took soundings in 1660 and 1860 fathoms. On the 19th sounded and dredged in 1450 fathoms. The bottom-temperature was 36°·3, being nearly two degrees higher than in 410 fathoms off Godthaab in Davis Strait. A large stone, as big as a man's head, came up on the weights above the dredge, but unfortunately fell off before the weights were brought on board. Let the submarine telegraph companies look to this! The mud in the dredge contained a great many small stones, to one of which was attached a living and beautiful specimen of *Discina Atlantica*. There were also a new species of *Terebratula* (*tenera*, J.), fragments of *Atretia gnomon*, *Amussium* (*Pleuronectia*) *lucidum*, *Lima ovata* (a Coralline-Crag and Monte-Mario fossil), *Dacrydium vitreum*, *Leda acuminata*, many of the 'Porcupine' deep-water species (including an undescribed species of the curious genus *Fissurisepta*), *Malletia excisa*

(Norway and West of Ireland, and fossil in Sicily), and an exquisite species of a new genus which I will name *Seguenzia*, and presently describe. The last is likewise a Sicilian fossil, and was found by me in the 'Porcupine' Expedition of 1870 off the Atlantic coasts of Spain and Portugal. Dr. M'Intosh notes a new and remarkable species of *Ditrypa*, and Mr. Norman several interesting Crustacea, Foraminifera, and a Sponge.

16. 20th August. Fine and sunshiny, with a calm sea. A sounding in lat. 56° 1′ N., 34° 42′ W., gave 690 fathoms only. Surface temperature 53°, bottom 38°·2. Dredged here and got *Discina Atlantica*, *Leda acuminata*, *Limopsis minuta* (*borealis*, Woodward), *Fusus Berniciensis*, and *Scaphander puncto-striatus* (*librarius*, Lovén), besides some of the 'Porcupine' novelties, such as *Dentalium capillosum*, J., *Fusus attenuatus*, J., and another species of *Seguenzia* hereafter noticed as *carinata*. The *Dentalium* had been also dredged by Count Pourtales in the Gulf of Mexico, and since in the 'Challenger' Expedition. There were likewise fragments of a volcanic or igneous rock (which, according to Mr. Etheridge, came probably from Iceland) as well as stones in the dredgings from 1750 and 1450 fathoms. The great difference of depth in the same track between the last and next sounding (1450 : 690 : 1230) was very striking; and we almost fancied that we had got on the sunken land of Buss. (See Dr. Wallich's 'North-Atlantic Sea-bed.') But a more likely explanation may be, that the intermediate and shallowest depth represents a submarine ridge corresponding with that discovered in the 'Bulldog' (viz. 1168 : 748 : 1260) between 59° and 60° N. lat. The fauna appeared to be the same on each side of the ridge. We sounded the next two days in 1230 and 1485 fathoms.

17. Our last sounding and dredging were made on the 23rd of August in 1785 fathoms. The sifting of a good load of ooze did not yield much. More fragments of *Atretia gnomon*, *Malletia excisa*, *Axinus Croulinensis*, *A. ferruginosus*, and the fry of *Isocardia cor*, with a few of the 'Porcupine' deep-water species, were the principal results in the Mollusca. Mr. Norman reports some undescribed Echinodermata, Isopoda, and Ostracoda; and Dr. M'Intosh a second species of his genus *Tachytrypane* under the name *arctica*. The following day another Atlantic gale came on, with violent squalls; the water in the dam increased from 3 feet 10 inches to 8 feet; and we were battened down. This stopped all further scientific exploration; but eleven out of twenty stations in the Admiralty programme had been examined, and we had nearly joined the soundings westward of Ireland obtained in the 'Porcupine.' We returned home safely on the 29th of August, after a rather eventful cruise of three months.

It is hoped that the scientific work thus done has not been unprofitable, and that it may in some measure serve to supplement the far greater exploration of the 'Challenger,' which did not extend north of

our meridian. We have had a mere glimpse of that "wonderland" which underlies the vast ocean; and our curiosity is very far from being satisfied, especially as regards the arctic seas. It is a new world, full of interest not only to naturalists but to every man of science.

Although we have of late years done a great deal to promote submarine researches, as shown by the expeditions of H.M.SS. 'Lightning,' 'Porcupine,' 'Shearwater,' 'Challenger,' and 'Valorous,' our comparatively poor neighbours in Scandinavia have been earlier in the field and not less energetic. From the 'Notices sur la Suède,' published on the occasion of the International Congress of Geographical Sciences in 1875 at Paris, it appears that between the years 1837 and 1875 seventeen scientific expeditions were made from Sweden, of which fifteen explored the arctic regions. Professors Lovén, Torell, and Nordenskiöld, with other distinguished naturalists, took an active part in these expeditions. The sister kingdom of Norway has now engaged in the same course of discovery; and a well-equipped Government expedition has within the last few days set out from Bergen, with the view of examining the region of sea surface and bottom bounded by Norway, the Shetlands, Färöes, Iceland, East Greenland, Jan Mayen, and Spitzbergen. This will be done during the present and the next year or two. Dr. G. O. Sars (son of the late Professor Sars and a zoologist of great reputation) is the naturalist in charge of the Norwegian expedition; and the harvest is sure to be abundant and valuable.

But after all it must be borne in mind that if every civilized nation in the world were every year during the next century to send out similar expeditions, with improved appliances for exploring the sea-bed, the field would be far from being exhausted. Every such expedition must be more or less tentative, and can only form the basis for a more complete investigation of "the deep bosom of the ocean." The area of this must be measured by many millions of square leagues; whereas all that has hitherto been effected has been to scrape in an imperfect manner the surface of a few scores of acres.

My attention has been directed exclusively to the Mollusca. Indeed I could not have prepared this Report but for the valuable assistance which has been kindly given me by naturalists who have specially studied other groups of the marine Invertebrata. Mr. Norman has worked out the Crustacea, Tunicata, Polyzoa, Echinodermata, Actinozoa, Foraminifera, Polycystina, and Sponges, Dr. M'Intosh the Annelida, Professor Allman the Hydrozoa, Professor Duncan the Corals, and Professor Dickie the Diatoms. Dr. Carpenter has undertaken the report of the physical results, and to complete the examination of the Foraminifera.

MOLLUSCA.

The total number of marine species procured during the 'Valorous' cruise was 181, viz. 122 in Davis Strait, and 59 in the North Atlantic, besides fragments of several undetermined species. The most complete and modern list of Greenlandic species is that which Dr. Mörch, the eminent conchologist of Copenhagen, prepared for the Manual of 'The Natural History, Geology, and Physics of the Arctic Regions, 1875.' This Manual was published by authority of the Lords Commissioners of the Admiralty for the use of the North-Polar Expedition. Dr. Mörch's list gives 155 marine species from Greenland, after deducting doubtful species and varieties. I am now enabled to add to that list 33 species, viz. 21 already described, and 12 undescribed or new. These last, with one exception (*Pleurotoma rubescens*), were from depths exceeding 1000 fathoms. I obtained altogether from Davis Strait and the North Atlantic no fewer than 37 undescribed species (Brachiopoda 2, Conchifera 16, Solenoconchia 7, Gastropoda 11, Pteropoda 1, Cephalopoda 0), all except the *Pleurotoma* from great depths. The only land-shell which occurred to me in Greenland was *Vitrina pellucida*, Müller, = *V. angelicæ* (Beck), Möller, which is a native of all the four old quarters of the globe. Several species from deep water were familiar to me from my dredgings in the 'Porcupine' off the west of Ireland and in the Bay of Biscay, as well as from the newer tertiary deposits in Sicily—thus showing a range of distribution from 56° to 38° N. lat., or between 1200 and 1300 miles. One of the most remarkable instances of such distribution, both in space and time, consisted in the rediscovery in comparatively high latitudes of two exquisite and peculiar species which cannot be referred to any known genus, and for which I will propose the name of *Seguenzia*, in honour of my friend Signor Seguenza, Professor of Geology and Palæontology at Messina. The genus evidently belongs to the *Solarium* family, but is distinguished by having a broad and deep open furrow (rather than a cleft) in the upper part of the last whorl. I have three species, all undescribed (*S. formosa*, *S. elegans*, and *S. carinata*), the first of which has no umbilicus, the other two being deeply umbilicated. The newer Tertiaries of Sicily also contain several other species of northern Mollusca in a fossil state which do not appear to inhabit the Mediterranean. Some of these (e. g. *Mya truncata*, *Saxicava Norvegica*, and *Buccinum undatum*) are comparatively shallow-water species; and as their transport or migration southwards cannot be accounted for by the action of deep submarine currents, it is difficult to conceive how they could have lived in that part of the Mediterranean where Sicily now stands, unless the climate of that region had changed in the same way as must have been at one time the case in Great Britain. Possibly the North Pole may formerly have been placed in Scotland!

The consideration of the Mollusca in Davis Strait gives rise to

a curious and interesting question as to whether the Greenlandic fauna is European or American. According to the learned President of the Royal Society, the flora of Greenland is European (see page 198 of 'The Natural History, Geology, and Physics of the Arctic Regions, 1875'). My examination of the Mollusca in the North Atlantic, from Norway and Spitzbergen to the United States, as well as in Davis Strait, induces me to extend Dr. Hooker's opinion to the marine Invertebrate fauna; and Dr. M'Intosh concurs with me in this as to the Annelida. Another of my colleagues, the Rev. A. M. Norman, believes, on the other hand, that the fauna of Davis Strait is American and not European, because out of 30 species of Echinodermata procured during the cruise of the 'Valorous,' 27 are American and 21 only are European, and out of 15 stalk-eyed Crustacea 13 are American and 11 only are European. In the other classes of the Crustacea, as well as in all the remaining orders of Invertebrata examined by him, the percentage is largely in favour of the fauna being European. The Mollusca on the eastern coasts of the United States have been most assiduously and carefully worked out by a host of able conchologists during more than half a century, and especially of late years by Professors Stimpson and Verrill and Mr. Whiteaves; so that I do not imagine that many more species remain to be gleaned on those coasts. Now the accompanying lists which I have prepared show that there are 116 North-American species which have not occurred on the coasts of Greenland nor in the European seas; that 52 other species are Greenlandic and European, not American; that 39 others are American and European, not Greenlandic; that only 3 others are American and Greenlandic, not European; and that 5 others are exclusively Greenlandic, and not American nor European. The total number of species from the north-eastern coasts of America is about 400. The result therefore shows very decidedly that the Mollusca of Greenland are more European than American, and implies that the course of migration has been in a westerly and not easterly direction.

Besides the examination of the shells of Mollusca I had an opportunity of examining and describing the "animals" or soft parts of 58 species, including such rare and peculiar forms as *Atretia gnomon*, *Discina Atlantica*, *Menestho* (not *Monoptygma*) *albula*, and *Pilidium radiatum*. I may here mention that I watched for a long time and on different occasions living specimens of *Rhynchonella psittacea*, with their valves opening and opened; but I could never detect any cilia (much less the arms) protruding. *Buccinum Grœnlandicum* takes in Davis Strait the place of our common *B. undatum*; its odontophore is very different, and, according to Mr. Jabez Hogg, the formula of *B. Grœnlandicum*, var. *sericata*, is 3.4.3, that of *B. undatum* being 4.7.4.

I will add diagnoses of three new genera, *Atretia*, *Glomus*, and *Seguenzia*. The new species will be described elsewhere.

Mollusca,

Greenlandic and European, not American.

From the 'Valorous' Cruise.

Names and Synonyms.	Habitats and Remarks.
1. *Montacuta Dawsoni*, Jeffreys ..	Aberdeenshire and west of Ireland: Christianiafiord: 'Porcupine' Expedition, 1870 (off Cape Sagres), 45 fathoms.
2. *Axinus eumyarius*, Sars	Norway (Sars): Porc. Exp. 1870 (Bay of Biscay), 227–740 fms.
3. *Cardium elegantulum*, Möller..	Norway (Sars and M'Andrew).
4. *Trochus umbilicalis*, Broderip and Sowerby (*Margarita*). Not *Margarita Grœnlandica*, var., to which Mörch refers it.	Wellington Channel (Belcher): Spitzbergen (Eaton).
5. *Pilidium radiatum*, Sars, 1850 (*Capulus* afterwards *Capulacmæa*), = *Pilidium commodum*, Middendorff, 1851, = *Piliscus commodus* and *P. probus*, Lovén, 1859, = *Capulus dilatatus* and *C. depressus*, A. Adams, 1860 and 1864.	Norway (Sars): Sea of Okhotsk (Middendorff): Spitzbergen (Torell): Japan (A. Adams): Aleutian Isles, N. Pacific (Dall): Uddevalla, fossil (J. G. J.): Moray Firth, semifossil (Robert Dawson): Montreal, fossil (Principal Dawson).
6. *Aporrhaïs Serresianus*, Michaud, 1828 (*Rostellaria*), var., = *A. pes-carbonis*, Forbes and Hanley (not Brongniart), 1853, = *A. Macandreæ*, Jeffr. 1867.	Fragments only. Not *A. occidentalis*. Norway, Shetland, Ireland, Bay of Biscay, and Mediterranean, 45–1230 fathoms.
7. *Fusus fenestratus*, Turton, 1834, = *Buccinum fusiforme*, Broderip, 1829 (specific name inappropriate).	South of Ireland: Norway: Porc. Exp. 1869, 200–1630 fathoms; 1870, 220–718 fathoms.
8. *Pleurotoma elegans*, Möll. (*Defrancia*), 1842, = *P. elegantior*, S. Wood, 1872.	Iceland (Torell): Porc. Exp. 1869, 560 fathoms.
9. —— *declivis*, Lovén (*Tritonium*).	Norway: Porc. Exp. 1869, 64–345 fms.; 1870, 507 fms.
10. —— *cinerea*, Möll. (*Defrancia*).	Spitzbergen (Torell): Porc. Exp. 1869, 290 fathoms.
11. —— *viridula*, Möll. (*Defrancia*).	Porc. Exp. 1869, 560 fathoms.

Oceanic.

12. *Clio pyramidata*, Browne	North Atlantic.

Deep Water.

13. *Atretia gnomon*, Jeffr.	1100–1785 fms.: Porc. Exp. 1869, 1380–1443 fms.: North-Atlan-

Names and Synonyms.	Habitats and Remarks.
	tic cable, 2400 fms. (Sir James Anderson).
14. *Pecten fragilis*, Jeffr.	1450–1785 fms.; fragments.
15. *Lima gibba*, Jeffr.	1450–1785 fms.
16. *Nucula reticulata*, Jeffr. (not *Leda reticulata*, Hinds).	1100 fms.: Porc. Exp. 1869, 1180–1476 fms.: 'Challenger' Exp., off Azores, 1000 fms.
17. *Leda pusio*, Philippi, var. *latior* .	1450–1750 fms.: Porc. Exp. 1869, 1180–1215 fms.; 1870, 257–994 fms.: Sicilian and Calabrian Tertiaries.
18. —— *acuminata*, Jeffr.	690–1750 fms.: Porc. Exp. 1869, 422–862 fms.; 1870, (Bay of Biscay) 45–1095 fms., (Mediterranean) 92–1456 fms.: Mediterranean, 40–120 fms. (Carpenter); 200–300 fms. (Nares); 310 fms. (Spratt); 230 fms. (Sir James Anderson): 'Challenger' Exp., Setubal Bay, 470 fms.; off Azores, 450 and 1000 fms. Fossil at Messina (Seguenza, as *L. Messanensis*, MS.). Perhaps the latter specific name ought to be substituted for mine, because Von Buch had previously described a Liassic species of *Leda* (his *Nucula acuminata*), and his name has been adopted by palæontologists.
19. —— *expansa*, Jeffr.	1450–1750 fms.: Porc. Exp. 1869, 1180–1380 fms.
20. —— *lata*, Jeffr.	1750 fms.: Porc. Exp. 1869, 165–1443 fms.; 1870, 740–1095 fms.: 'Challenger' Exp., off Azores, 1000 fms.
21. *Glomus nitens*, Jeffr.	1750 fms.: Porc. Exp. 1869, 557–1476 fms.
22. *Limopsis aurita*, Brocchi	1100 fms.: Shetland: Wellington Channel (Belcher): Porc. Exp. 1869, 155–458 fms.; 1870, (Bay of Biscay) 220–690 fms., (Mediterranean) 92 fms.: 'Lightning' Exp. 189 fms.
23. *Malletia cuneata*, Jeffr.	1450–1750 fms.: Porc. Exp. 1869, 1215–1443 fms.; 1870, (Bay of Biscay) 718–1095 fms., (Mediterranean) 1415 fms.

Names and Synonyms.	Habitats and Remarks.
24. *Axinus cycladius*, S. Wood (*Kellia*).	1750 fms.: Shetland (J. G. J.): Porc. Exp. 1870, 386 fms.: Mediterranean, 30–40 fathoms (Nares), 100 fms. (Spratt), 40–120 fms. (Carpenter).
25. —— *incrassatus*, Jeffr.	1750 fms.; also in North Atlantic, 1450 and 1785 fms.
26. *Dentalium candidum*, Jeffr.	1100 fms.: Porc. Exp. 1869 (Bay of Biscay), 2435 fms.: 'Challenger' Exp., off Azores, 450 and 1000 fms.

From Dr. Mörch's List, the Copenhagen Museum, and other authorities.

27. *Leda tenuis*, Philippi, 1836 (*Nucula*), = *Nucula lenticula*, Möll., = *Nucula pygmæa* (v. Münster ?), Ph. 1844 (not v. Münster's species).	British and Scandinavian, 20–300 fms.: Gulf of Gascony, 40–80 fms. (Marquis de Folin): Porc. Exp. 1869, 96–1630 fms.; 1870, (Bay of Biscay) 128–1095 fms., (Mediterranean) 40–1456 fms.: 'Lightning' Exp. 189–650 fms.: Mediterranean, 40–120 fms. (Carpenter), 30–300 fathoms (Nares), 130–310 fms. (Spratt).
28. —— *abyssicola*, Torell	Wellington Channel (Belcher): Spitzbergen (Torell): Norway (G. O. Sars): Shetland (J. G. J.): Porc. Exp. 1869, 165–862 fms.; 1870, 304–717 fms.
29. —— *intermedia*, Sars	Wellington Channel (Belcher): Norway (Sars): Spitzbergen (Torell): Shetland (J. G. J.).
30. *Arca glacialis*, Gray, 1824, = *A. obliqua*, Philippi, 1844, = *A. lactea*, Malm, 1853 (non Linné), = *A. Korenii*, Danielssen, 1859.	Wellington Channel (Belcher): Spitzbergen (Torell): Scandinavia (Malm and others): Shetland (J. G. J.): Porc. Exp. 1869, 64–422 fms.; 1870, (Bay of Biscay) 45–58 fms., (Mediterranean) 92–1456 fms.: Mediterranean, 95 fms. (Acton), 30–300 fms. (Nares), 30–120 fms. (Carpenter), 100–250 fms. (Spratt).
31. *Astarte Warhami*, Hancock, 1846, = *A. fabula*, Reeve, 1855.	Davis Straits (Hancock): Wellington Channel (Belcher): Greenland (Verkrüzen): Spitzbergen (Torell and Eaton).
32. *Pecchiolia abyssicola*, Sars	Davis Straits (Mus. Copenhagen): Baffin's Bay, 199–336 fms.

Names and Synonyms.	Habitats and Remarks.
	(Lindahl): Norway (Sars): Porc. Exp. 1869, 557–670 fms.; 1870 (Bay of Biscay), 567 fms.
33. *Neæra cuspidata*, Olivi	Greenland (Wallich, *fide* Mörch): Spitzbergen (Torell): British, Scandinavian, and Mediterranean.
34. *Chiton cinereus*, Linné	Greenland (Mörch): British and Scandinavian: Heligoland: Bay of Biscay: 'Lightning' Exp. 189–530 fms.
35. *Lacuna puteolus*, Turt.........	Greenland (Möller, as *L. pallidula*): British, Scandinavian, and Icelandic: 'Lightning' Exp. 530 fms.
36. —— *crassior*, Montagu, 1803 (*Turbo*), = *L. glacialis*, Möller, 1842.	Greenland (Möller): Spitzbergen, 5–12 fms. (Torell): British, N. France, N. Pacific.
37. *Rissoa scrobiculata*, Möller	Greenland (Möller and J. G. J.): Spitzbergen (Torell).
38. —— *cimicoïdes*, Forbes, = *R. intermedia*, Aradas.	Greenland (Barrett), coll. M'Andrew.
39. *Homalogyra rota*, Forbes and Hanley (*Skenea*?).	Off Hamilton's Inlet, 1622 fms. (Wallich): British, Scandinavian, Mediterranean, and Madeiran.
40. *Aclis Walleri*, Jeffr..........	Off Hamilton's Inlet, 1622 fms. (Wallich): British, Scandinavian, Mediterranean.
41. *Velutina lanigera*, Möller	Greenland (Möller): Norway (Sars).
42. —— *plicatilis*, Müller, 1776 (*Bulla*), = *Bulla flexilis*, Mont. 1808.	Greenland (coll. Möller in Mus. Copenh.): British and Scandinavian: Iceland (Steenstrup): N. Pacific (Middendorff).
43. *Trichotropis conica*, Möller	Greenland (Möller): Oxfjord, Finmark, 40–100 fms. (Sars).
44. *Buccinum Belcheri*, Reeve	Wellington Channel (Belcher): Vadsö, Finmark (Verkrüzen).
45. *Fusus lachesis*, Mörch, 1869, = *Tritonium terebrale*, Sars, MS. Not *Neptunea terebralis*, Gould, which is *F. Spitzbergensis* of Reeve.	Greenland, 80 fms. (Olrik, *fide* Mörch): Norway (Sars): Porc. Exp. 1869, 440 fms.
46. —— *tortuosus*, Reeve, 1855, = *F. Sabini* (Gray), Hancock, 1846. Not *Buccinum* (*Fusus*) *Sabinii*, Gray.	Wellington Channel (Belcher): Spitzbergen (Torell): Norway (Verkrüzen).

Names and Synonyms.	Habitats and Remarks.
47. *Fusus propinquus*, Alder	Greenland (Mörch): East Greenland (Möbius): British and Scandinavian: Porc. Exp. 1869, 66–1230 fms.: 1870 (Bay of Biscay), 109–1380 fms.: 'Lightning' Exp., 189–530 fms.
48. —— *Islandicus*, Chemnitz, 1780 (*Murex*), = *Tritonium antiquum*, Fabricius, 1780 (non Linné).	Greenland (Fabricius and others): Icelandic, Scandinavian, and British, 30–300 fms.: Gulf of Gascony (Lafont): Porc. Exp. 1869, 85–155 fms.
49. *Mitra Grœnlandica* (Beck), Möll.	Greenland (Möller): Baffin's Bay (Gray): Wellington Channel (Belcher): Porc. Exp. 1869, 200–420 fms.
50. *Utriculus expansus*, Jeffr.	Greenland (Torell and Mus. Copenhagen): Norway (Sars and others): Shetland (J. G. J.): Porc. Exp. 1869, 542–670 fms.
51. *Philine scabra*, Müll., 1788 (*Bulla*), = *Bullæa punctata*, Möll., 1842 (non Clark).	Greenland (Möller): British coasts, Iceland, Scandinavia, Bay of Biscay, and Mediterranean: Porc. Exp. 1870 (Mediterranean), 92 fms.
52. *Leachia hyperborea*, Steenstrup .	Greenland (Mörch): Porc. Exp. 1869, and 'Valorous' in North Atlantic.

Mollusca,

Greenlandic and North-American, not European.

1. *Amaura candida*, Möll.	Greenland (Möller and others): Gulf of St. Lawrence (Whiteaves).
2. *Fusus Kröyeri*, Möll.	Greenland (Holböll and Barrett): Labrador (Stimpson): Murray Bay (Principal Dawson).
3. *Æolis Bostoniensis*, Couthouy . .	Greenland (Olrik, *teste* Mörch). "Approaching closely *Æolis coronata* of Forbes," which is European.

Greenlandic, not North-American nor European.

1. *Trochus Vahli*, Möll. (*Margarita*).	Greenland (Möller and others).
2. *Acirsa Eschrichti*, Möll. (*Scalaria*).	Greenland (Möller and others). Fossil at Uddevalla and on the coasts of Aberdeenshire and Antrim. *Acirsa* differs from *Scalaria* in sculpture, form of

Names and Synonyms.	Habitats and Remarks.
	the mouth, and apex. *S. subdecussata* of Cantraine also belongs to the genus *Acirsa*.
3. *Pleurotoma rubescens*, Jeffr., sp. n.	Holsteinborg, 10 fms. (J. G. J.).
(*From Deep Water.*)	
4. *Kellia symmetros*, Jeffr., sp. n. . .	Davis Straits, 1750 fms. (J. G. J.). A single specimen.
5. *Utriculus substriatus*, Jeffr., n. sp.	Davis Straits, 1750 fms. (J. G. J.). Fragments only; but evidently belonging to an undescribed species allied to *U. expansus*, which is quite smooth.

MOLLUSCA,

North-American and European, not Greenlandic. (See the second edition of Gould's 'Invertebrata of Massachusetts,' and the Reports of the British Association for 1872.)

Names of Species.	Synonyms and Remarks.
1. *Teredo navalis*, Linné.	
2. —— *Norvegica*, Spengler.	
3. —— *pedicellata*, Quatrefages.	
4. *Pholas crispata*, L.	
5. *Solen ensis*, L.	
6. *Solenomya togata*, Poli, 1791 (*Tellina*).	*Solemya velum*, Say, 1822; *Solemya borealis*, Totten, 1834.
7. *Neæra pellucida*, Stimpson.	
8. *Kellia suborbicularis*, Montagu (*Mya*).	
9. *Lucina borealis*, L. (*Venus*).	
10. *Cardita sulcata*, Bruguière, 1792; var.	*C. borealis*, Conrad, 1836.
11. *Mytilus modiolus*, L.	
12. *Anomia ephippium*, L., and vars.	
13. *Philine lima*, Brown, 1827 (*Bulla*).	*Bulla lineolata*, Couthouy, 1839.
14. *Scaphander puncto-striatus*, Mighels and Adams, 1842 (*Bulla*).	*S. librarius*, Lovén, 1846.
15. *Polycera Lessoni*, D'Orbigny.	
16. *Doris tuberculata*, Cuvier, 1802 .	*D. diademata*, Ag., 1870.
17. *Dendronotus arborescens*, Müller (*Doris*).	
18. *Doto coronata*, Gmelin (*Doris*).	
19. *Æolis papillosa*, L. (*Limax*).	
20. —— *rufibranchialis*, Johnston (*Eolidia*).	

Names of Species.	Synonyms and Remarks.
21. *Æolis picta*, A. & H.	
22. —— *despecta*, Johnston.	
23. *Chiton marginatus*, Pennant....	*C. cinereus*, Gould (not L.). A doubtful identification.
24. —— *mendicarius*, M. & A., 1842.	Described as "one inch long and four inches broad"! *C. Hanleyi* (Bean), Thorpe, 1844, sec. spec. Americ.
25. *Dentalium striolatum*, St., 1851 (*Entalis*).	*D. abyssorum*, Sars, 1858.
26. *Crepidula fornicata*, L. (*Patella*).	
27. *Ianthina communis*, Lamarck ..	Oceanic, and questionably indigenous.
28. *Trochus obscurus*, Couth. (*Margarita*).	
29. —— *varicosus*, M. & A., 1842 (*Margarita*).	*M. elegantissima* (Bean), S. Wood, 1848.
30. *Hydrobia ventrosa*, Mont., 1803; var. (*Turbo*).	*Rissoa minuta*, Tott., 1834.
31. *Littorina litorea*, L. (*Turbo*).	
32. *Cerithiopsis tubercularis*, Mont., 1803 (*Murex*).	*Cerithium Greenei*, Ad., 1839.
33. —— *trilineata*, Philippi, 1836 (*Cerithium*).	*Cerithium terebrale*, Ad., 1841.
34. *Odostomia impressa*, Say, 1822. .	*O. cœlata*, Cailliaud, 1865.
35. —— *interrupta*, Tott., 1834 (*Turritella*).	*Melania rufa*, Ph., 1836 (afterwards *Chemnitzia*).
36. *Bulbus Smithii*, Br., 1839 (*Natica*).	*Natica flava*, Gd., 1840; *N. aperta*, Lov., 1846.
37. *Trophon muricatus*, Mont. (*Murex*).	Doubtful as American; perhaps *T. truncatus*, Ström.
38. *Melampus myosotis*, Draparnaud (*Auricula*).	
39. *Cavolina trispinosa*, Lesueur (*Hyalæa*).	Oceanic.
39, including 2 oceanic.	

Mollusca

inhabiting the eastern coasts of North America, which have not occurred on the coasts of Greenland nor in the European seas.

(See the second edition of Gould's 'Invertebrata of Massachusetts,' 1870, and my communication to the British Association in 1872, "On the Mollusca of Europe compared with those of Eastern North America.")

Names of Species.	Synonyms and Remarks.
1. *Teredo Thompsonii*, Tryon.	
2. *Xylotrya fimbriata*, Jeffreys.	

Names of Species.	Synonyms and Remarks.
3. *Pholas costata*, Linné.	
4. —— *truncata*, Say.	
5. *Solecurtus gibbus*, Spengler.	
6. —— *divisus*, Sp.	
7. *Siliqua squama*, Blainville.	
8. —— *costata*, Say.	
9. *Corbula contracta*, Say.	
10. *Pandora trilineata*, Say.	
11. *Lyonsia hyalina*, Conrad.	
12. *Anatina papyracea*, Say.	
13. *Thracia Leana*, Conr.	
14. —— *Conradi*, Couthouy.	
15. —— *septentrionalis*, Jeffr.	
16. *Mactra solidissima*, Chemnitz.	
17. —— *lateralis*, Say.	
18. *Cumingia tellinoïdes*, Conr.	
19. *Mesodesma deauratum*, Turton.	
20. *Petricola pholadiformis*, Lamarck.	
21. *Tellina tenta*, Say.	
22. —— *tenera*, Say.	
23. *Lucina dentata*, Wood.	
24. *Astarte castanea*, Say; and var. (*quadrans*).	
25. *Crassatella mactracea*, Linsley.	
26. *Venus convexa*, Say.	
27. —— *mercenaria*, L.; and young (*gemma*).	
28. *Gemma Manhattensis*, Prime.	
29. *Cardium Mortoni*, Conr.	
30. *Arca pexata*, Say.	
31. *Nucula proxima*, Say.	
32. *Leda obesa*, Stimpson.	
33. —— *myalis*, Couth.	
34. *Mytilus plicatulus*, Lam.	
35. *Crenella glandula*, Totten.	
36. *Pecten tenuicostatus*, Mighels and Adams.	
37. —— *irradians*, Lam.	
38. *Ostrea Virginiana*, Lister.	
39. *Philine sinuata*, St.	
40. *Utriculus hyemalis*, Couth.	
41. —— *canaliculatus*, Say.	
42. *Bulla incincta*, Migh.	
43. —— *solitaria*, Say.	
44. *Actæon puncto-striatus*, Ad.	
45. *Doris tenella*, Agassiz.	

Names of Species.	Synonyms and Remarks.
46. *Doris grisea*, St.	
47. *Ancula sulphurea*, St.	
48. *Æolis pilata*, Gould.	
49. —— *stellata*, St.	
50. —— *purpurea*, St.	
51. —— *diversa*, Couth.	
52. *Calliopœa? fuscata*, Gd.	
53. *Embletonia fuscata*, Gd.	
54. —— *remigata*, Gd.	
55. *Hermœa cruciata*, Alex. Ag.	
56. *Alderia Harvardiensis*, Ag.	
57. *Elysia chlorotica*, Ag.	
58. *Placobranchus catulus*, Ag.	
59. *Limapontia zonata*, St.	
60. *Chiton apiculatus*, Say.	
61. *Amicula Emersonii*, Couth.	
62. *Crepidula convexa*, Say.	
63. *Crucibulum striatum*, Say.	
64. *Rissoa sulcosa*, Migh.	
65. —— *exarata*, St.	
66. *Littorina irrorata*, Say.	
67. *Scalaria lineata*, Say.	
68. —— *multistriata*, Say.	
69. *Cæcum pulchellum*, St.	
70. *Vermetus radicula*, St.	
71. *Aporrhaïs occidentalis*, Beck.	
72. *Cerithium nigrum*, Tott.	
73. —— *Emersoni*, Ad.	
74. *Triforis nigrocincta*, Ad.	
75. *Odostomia producta*, Ad.	
76. —— *fusca*, Ad.	
77. —— *dealbata*, St.	
78. —— *modesta*, St.	
79. —— *bisuturalis*, Say.	
80. —— *seminuda*, Ad.	
81. —— *nivea*, St.	
82. *Eulima oleacea*, Kurtz and St.	
83. *Natica heros*, Say.	
84. —— *pusilla*, Say.	
85. —— *immaculata*, Tott.	
86. —— *duplicata*, Say.	
87. *Pleurotoma plicata*, Ad.	Different from *P. declivis* of Lovén, to which I had referred it, judging from description only.

Names of Species.	Synonyms and Remarks.
88. *Columbella avara*, Say.	
89. —— *dissimilis*, St.	
90. —— *lunata*, Say.	
91. *Nassa obsoleta*, Say.	
92. —— *trivittata*, Say.	
93. —— *vibex*, Say.	
94. *Urosalpinx cinerea*, Say.	
95. *Fusus curtus*, Jeffr.	*F. Islandicus*, Gd. (not Ch.).
96. —— *ventricosus*, Gray	Perhaps a monstrous variety of *F. curtus*.
97. —— *pygmæus*, St.	
98. —— *decemcostatus*, Say.	
99. *Busycon canaliculatum*, L.	
100. —— *carica*, Gmelin.	
101. *Fasciolaria ligata*, M. & A.	
102. *Ranella caudata*, Say.	
103. *Melampus corneus*, Deshayes.	*M. bidentatus*, Say (not Mont.).
104. *Psyche globulosa*, Rang	Oceanic.
105. *Loligopsis pavo*, Lesueur.	
106. *Loligo punctata*, De Kay.	
107. —— *Pealei*, Les.	
108. *Spirula australis*, Bruguière.	Oceanic, and questionably indigenous.

(From Professor Verrill's papers in 'The American Journal of Science and Arts,' 1873–75, and not in the second edition of Gould's 'Invertebrata of Massachusetts.')

109. *Pecten pustulosus*, Verrill.
110. *Stilifer Stimpsoni*, Verr.
111. *Ringicula nitida*, Verr.
112. *Pleurotomella Packardi*, Verr.
113. *Doto formosa*, Verr.
114. *Idalia modesta*, Verr.
115. *Loligo pallida*, Verr.
116. *Octopus Bairdi*, Verr. *

Additions now made by me to Dr. Mörch's list of Greenlandic Mollusca.

Species already described.

Names of Species.	Depth in fathoms and Remarks.
1. *Crenella faba*, Fabricius	1–60.
2. *Leda lucida*, Lovén	410.
3. —— *frigida*, Torell	175.
4. —— *pusio*, Philippi; var.	1750; North Atlantic, 1450.
5. —— *acuminata*, Jeffr.	1750; N. Atlantic, 1450 and 690.

* It is no answer to say that most of the above are also southern species.

Names of Species.	Depth in fathoms and Remarks.
6. *Limopsis aurita*, Sasso	1100.
7. *Axinus eumyarius*, Sars	1100.
8. —— *cycladius*, S. Wood	1750.
9. *Tellina inflata*, Stimpson	60.
10. *Dentalium striolatum*, St.	410 and 1750.
11. *Siphonodentalium Lofotense*, Sars.	1750.
12. *Chiton ruber*, Pennant	12. Not *C. ruber* of Linné.
13. *Trochus umbilicalis*, Broderip and Sowerby.	20. Not a variety of *T. Grœnlandicus*.
14. *Rissoa arenaria*, Mighels and Adams.	5–35.
15. *Eulima stenostoma*, Jeffreys	410.
16. *Pilidium radiatum*, Sars	12 and 60.
17. *Aporrhaïs Serresiana*, Michaud .	410. Fragments. Not *A. occidentalis*.
18. *Trophon truncatus*, Ström	12–60 fms.
19. *Fusus fenestratus*, Turton	410.
20. *Pleurotoma declivis*, Lov.	60.
21. *Spirialis retroversus*, Fleming ..	1750. Oceanic, and having sunk (with *Globigerinæ*) to the bottom.
Species undescribed*.	
22. *Atretia gnomon*, Jeffr.	1100. See below.
23. *Pecten fragilis*, Jeffr.	1750.
24. *Lima gibba*, Jeffr.	1750.
25. *Nucula reticulata*, Jeffr.	1100. Also 'Porcupine' Expedition, 1869.
26. —— *expansa*, Jeffr.	1750. Also Porc. Exp., 1869.
27. *Glomus nitens*, Jeffr.	1750. Also Porc. Exp., 1869.
28. *Malletia cuneata*, Jeffr.	1750. Also Porc. Exp., 1869 and 1870.
29. *Kellia symmetros*, Jeffr.	1750.
30. *Axinus incrassatus*, Jeffr.	1750.
31. *Dentalium candidum*, Jeffr.	1100. Also Porc. and 'Challenger' Expeditions.
32. *Pleurotoma rubescens*, Jeffr.	10.
33. *Utriculus substriatus*, Jeffr.	1750. Fragments.

ATRETIA †, Jeffreys.

Nothing could be observed as to the animal, except that a few delicate bristles (which were persistent or fixed) protruded beyond the edges of the valves in an adult as well as a young specimen. The larger specimen was partly covered with small sessile Foraminifera (*Truncatulina lobatula*)

* Some of these species have been elsewhere noticed, and will be described before the Report is published.

† From ἄτρητος, imperforate.

and some young of this remarkable Brachiopod. Byssus long, tubular, and flexible, attached to a fragment of the case of a tubular Foraminifer.

SHELL inequivalve, triangular, imperforate, of a fibrous texture: *beak* prominent and pointed, but not incurved: *byssal foramen* elongated: *hinge-line* narrow: *skeleton* composed of two funnel-shaped processes, which diverge from the beak in the upper or larger valve, and of two blade-like processes, besides an upright plate (like the hand or index of a sundial) in the upper part of the lower or smaller valve.

Its nearest ally is *Rhynchonella*, from which it appears to be distinguishable only by the straight instead of incurved beak, and by the arms or brachial cirri not being coiled.

I know of one species only, which I propose to name *gnomon*. It has been figured by Mr. Davidson in the publications of the Palæontographical Society for 1874, pl. i. f. 7–10.

'Valorous' Expedition, 1100–1750 fms.; 'Porcupine' Expedition, 1869, 1380–1443 fms.

GLOMUS*, Jeffr.

SHELL nearly spherical: *cartilage* internal, elongated: *teeth* numerous, minute, and set obliquely.

Has the aspect of *Pectunculus* and the hinge of *Leda*; but the teeth are not arranged as in either of those genera. One species only is known to me, which I have named *nitens*; it is minute, about $\frac{1}{10}$ of an inch. It occurred in Davis Strait, 1750 fms.; also 'Porcupine' Expedition, 1869, on the west coast of Ireland, 1180–1476 fms., and North Sea, 557 fms.

SEGUENZIA†, Jeffr.

SHELL globular or conical, glossy, without epidermis, exquisitely sculptured; upper part of the last whorl deeply and widely grooved: *pillar* abruptly notched below, and exhibiting a small tooth-like process: *base* either deeply umbilicated or imperforate.

This genus evidently belongs to the *Solarium* family, and is allied to that subgenus or section of the genus *Solarium* which the late Dr. J. E. Gray named *Philippia*, and founded on the *Trochus hybridus* of Linné. *Seguenzia* differs, however, from *Solarium* and from every other genus of that family in the last whorl having a deep and wide groove, which is placed in the upper part, instead of a narrow slit placed in the middle or periphery as in *Scissurella* and *Pleurotomaria*; nor is the mouth of the shell entire as in those two genera. Three species are known to me, viz.:—

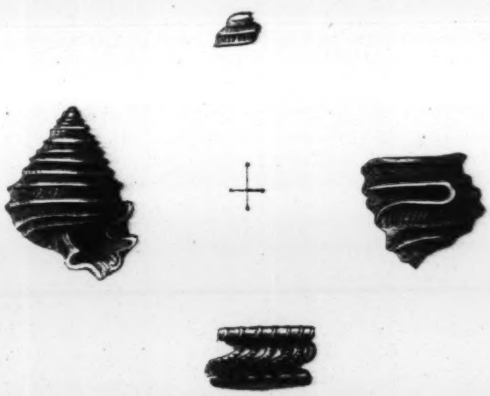
1. *S. formosa*, sp. n. Base imperforate. 'Valorous' Expedition, North Atlantic, 1450 fms. 'Porcupine' Expedition, Bay of Biscay, off

* A ball of thread.

† Dedicated to Professor G. Seguenza, the distinguished Palæontologist at Messina.

the coasts of Spain and Portugal, 718–795 fms. 'Challenger' Expedition, Station 56, S.W. of Bermudas, 1075 fms. Gulf of Mexico, 325 fms. (Pourtales). Fossil at Trapani in Sicily (Seguenza). Prof. Seguenza referred this species to the genus *Trochocochlea* of Klein; but the type of that subgenus of *Trochus* is *Trochus lineatus* of Da Costa, which has no relation to the present genus.

The accompanying figures of a perfect specimen from the 'Valorous' dredgings will serve to show the beauty of this species and the peculiarity of the genus.



Seguenzia formosa, Jeffr.

2. *S. elegans*, sp. n. Base perforated or umbilicated. Porc. Exp. 1870, off the coast of Portugal, 740–1095 fms.
3. *S. carinata*, sp. n. Base widely and deeply umbilicated. 'Valorous' Exp., North Atlantic, 690 fms.; Porc. Exp. 1870, Bay of Biscay, off the coasts of Spain and Portugal, 718–1095 fms. 'Challenger' Expedition, west of Fayal, Azores, 1000 fms.

Note on the Odontophore of *Buccinum Grœnlandicum.*

By Jabez Hogg, F.L.S.

The odontophore of *B. Grœnlandicum* differs from that of *B. undatum* both in the lateral and central teeth. In *B. Grœnlandicum* the lateral portion of the ribbon contains about sixty rows of bold translucent teeth, arranged in series of three set on the same shaft; the superior of which is recurved and much prolonged, while the central one is small and subdued, and the third or last is of medium size and slightly recurved. The median (rachidian) portion of the ribbon bears upon its more central part a series of symmetrically arranged rows of four teeth, all of which are of the same length, short and more suppressed than the smaller of the laterals, but far more developed than those of *B. undatum*.

I have noticed, in my former examinations of the odontophores of *Buccina*, that in the early condition (embryonic state) there are three

centres of silication, and that the ribbons when fully developed are enclosed in a kind of muscular and membranous sheath. This sheath can be stripped off and allow of an easy separation of the three bands.

In *B. undatum* the odontophore is said to be longer than the whole length of the body of the animal. This I should think is a mistake; at all events that of *B. Grœnlandicum*, when stretched out, is somewhat less than half the length of the body.

Crustacea, Tunicata, Polyzoa, Echinodermata, Actinozoa, Foraminifera, Polycystina, and Spongida. By the Rev. A. M. Norman, M.A.

Four hundred and seven Invertebrata brought home by H.M.S. 'Valorous,' and belonging to the above classes have been examined. Considering the very short time allowed for dredging, the state of the weather, and other circumstances, the results of the Expedition are surprising and reflect great credit on all concerned. Moreover, if a proof were needed, it is here found how little is yet known of the fauna of the deep waters within the Arctic Circle, or even of the inhabitants of the shallower parts of these northern seas. It is necessary to bear in mind that the investigations of the 'Valorous' were chiefly confined to those portions of the coast which had been previously most carefully worked by the Scandinavian naturalists. Each time that the dredge was let down in the deeper parts of Davis Strait, it brought up animals of high interest altogether new to science; and it is not a little remarkable that among these were representatives of genera which had until lately been regarded as exclusively confined to tropical or subtropical seas. The results of this Expedition show how desirable it is that really systematic dredging should be carried out in Davis Strait, and still more that the wholly unexamined fauna of Baffin's Bay and Smith's Sound should be investigated. It is to be hoped that the Arctic Expedition may be enabled to carry out successful researches with respect to the fauna of these high latitudes. The determination of the character of the animals living in the abyss of the Arctic seas is a matter of no small importance whether regarded from a zoological or from a geological standpoint.

Of the 407 species, 339 were procured in Davis Strait, and 128 in the North Atlantic, many animals, especially among the Foraminifera, having been brought home from both areas.

I have made a careful examination of all that has been written on the animals belonging to those classes on which it is my duty to report which had been brought by previous British Arctic expeditions from Davis Strait, and I find that the total aggregate of these Invertebrata of earlier expeditions is at the most 166 species as against 339 brought home by the 'Valorous,'—a convincing proof of the results which can be obtained by the use of modern appliances when in skilful hands.

Of the 113 Greenland Crustacea, 43 are known as North-American,

82 as European, 49 as British; but one only, the Amphipod *Anonyx gulosus*, Kröyer, has as yet been found living in the Mediterranean.

Of the 66 Polyzoa, 33 are American, 59 European, 35 British, 1 Mediterranean. This Polyzoon is *Lepralia hyalina*, the range of which seems to be cosmopolitan.

Of the 30 Echinodermata, 27 are American, 21 European, and 9 British, but not any Mediterranean.

Of the 103 Foraminifera, 46 are known as American, 54 as European, 52 as British, and many Mediterranean.

Taking these four classes together, therefore, we find that of 312 Greenland species, 149 (or 47 per cent.) are North-American, 216 (or 69 per cent.) are European (including Spitzbergen under that term), and 145 (or 46 per cent.) are British. We might thus be led to infer that the Greenland Marine Invertebrata approached much more nearly in character to the European than to the American fauna. Closer examination, however, of the facts seems to prove that such a conclusion would be erroneous; for while the Marine Invertebrata of Europe have been very carefully worked out, very much remains to be done among all the less conspicuous animals of the North-American coasts*. Thus, as regards the great class of the Crustacea, comparatively little is known of any orders except those which contain the large stalk-eyed forms.

If, then, disregarding all other classes and orders, we confine our percentages to the Echinodermata and Stalk-eyed Crustacea, which we know to have been well worked up on the North-American coast, we find the results altogether changed; for of the 30 Greenland Echinodermata 27 (or 90 per cent.) are American, 21 (or 70 per cent.) are European, and 9 (or 30 per cent.) are British; and of the 15 Stalk-eyed Crustacea, 13 (or 86 per cent.) are American, 11 (or 73 per cent.) are European, and 6 (or 40 per cent.) are British; and we cannot but conclude that when the American marine fauna shall have been as fully known as that of European seas, the fauna of Davis Strait will be found to possess an American rather than a European character, though the contrary might at first sight be inferred from our present unequal knowledge of the several faunæ.

The following Tables will show at a glance:—(1st) the number of animals belonging to the several classes, (2nd) to the orders of the *Crustacea*, *Polyzoa*, and *Echinodermata*, which have been dredged by the 'Valorous,' whether in Davis Strait or the North Atlantic, and the proportionate geographical range of the forms in the American, European, and British seas; 3rd, our knowledge of the fauna of Davis Strait previous to the 'Valorous' Expedition, and the increased knowledge which is the fruit of that expedition.

* The more the fauna of the western side of the North Atlantic is studied the nearer it is found to approximate to that of the western side. This has become very evident from the recent valuable operations carried on, under the conduct of Messrs. Verrill and Smith, by the American Government, and, under Mr. Whiteaves in the Gulf of St. Lawrence, by the Canadian Government.

TABLE I.—Summary of the Crustacea, Tunicata, Polyzoa, Echinodermata, Actinozoa, Foraminifera, Polycystina, and Spongida, showing geographical distribution and other particulars.

	GREENLAND.						
	1. Greenland and Davis Strait, 'Valorous.'	2. Known as N.-American.	3. Known as European.	4. Known as British.	5. Total number of species brought home by other British Arctic Expeditions.	6. Brought by 'Valorous' from North Atlantic.	7. Total species brought home by 'Valorous.'
CRUSTACEA	113	43	82	49	72	29	133
TUNICATA..................	7	...	...	...	6	...	7
POLYZOA	66	33	59	35	12	2	67
ECHINODERMATA	30	27	21	9	22	5	35
ACTINOZOA	7	2	2	2	...	...	7
FORAMINIFERA............	103	46	69	65	54	83	142
POLYCYSTINA	8	...	...	...	...	...	8
SPONGIDA	5	1	1	1	...	3	8
Total......	339	152	234	161	166*	122	407

TABLE II.—The Crustacea, Polyzoa, and Echinodermata divided into their several orders (the columns corresponding to those in Table I.).

	1.	2.	3.	4.	5.	6.	7.
CRUSTACEA.							
Brachyura	3	3	2	2	1	1	4
Anomura	1	1	1	1	1	...	1
Macrura	11	9	8	2	7	1	12
Stomapoda	...	...	...	...	1	...	...
Cumacea	6	...	2	1	2	1	7
Isopoda	7	1	2	1	3	4	9
Amphipoda	39	12	32	9	18	6	42
Phyllopoda	3	1	2	2	1	...	3
Ostracoda	34	12	27	26	25	15	45
Copepoda	2	0	1	1	6	many	2
Cirripedia	4	3	4	3	2	1	5
Pycnogonoidea	3	1	1	1	5	...	3
	113	43	82	49	72	29	133

* This summary includes the animals collected in Parry's first and third voyages, Ross's second voyage, Penny's, Belcher's, and M'Clintock's voyages; and the Polyzoa recorded by Busk, the Ostracoda by Mr. G. Brady, and the Foraminifera by Parker and Jones.

TABLE II. (*continued*).

	1.	2.	3.	4.	5.	6.	7.
POLYZOA.							
Cyclostomata............	10	8	10	8	2	...	10
Ctenostomata	1	1	1	1	...	...	1
Chilostomata............	55	24	48	26	10	2	56
	66	33	59	35	12	2	67
ECHINODERMATA.							
Holothuroidea	8	7	4	1	3	1	9
Echinoidea	3	2	2	2	1	1	4
Asteroidea..............	8	8	7	3	5	2	10
Ophiuroidea	10	9	8	3	12(?)	1	11
Crinoidea	1	1	?	...	1	...	1
	30	27	21	9	22	5	35

TABLE III.—Showing the total Fauna of Davis Strait as known from *all* sources previously to the 'Valorous' Expedition, and the additions made to it by that Expedition.

	W. Greenland Fauna as known in 1875.	Results of 'Valorous' Expedition in 1875.	Addition to Fauna in 1875.	W. Greenland Fauna as now known.
CRUSTACEA	214	113	35	249
TUNICATA.................	14	7	...	14
POLYZOA	78	66	16	94
ECHINODERMATA	34	30	2	36
ACTINOZOA	8	7	2	10
FORAMINIFERA...........	54	103	63	117
POLYCYSTINA	0	8	8	8
SPONGIDA.................	28	5	...	28
	430	339	126	556

It will be seen from the above that an important addition has been made to the Greenland fauna. The numbers must be regarded as approximate only, since there is still some material to be worked through.

In the following notes, the more remarkable animals in the several dredgings are briefly noticed.

Tow-net.

In the tow-net in Lat. 52° 33′ N., Long. 26° 44′ W., in the North Atlantic, the very rare *Pasiphæa tarda*, Kröyer, was taken. The same mode of collecting also produced *Nautilograpsus pelagicus*, Roux, *Idotea robusta*, Kröyer, *Themisto libellula* (Mandt), *Parathemisto compressa* (Goës), and *Tauria medusarum* (Kröyer), together with many Copepoda, a Campanularian, *Lepralia hyalina*, many Diotomacea, &c. In Davis Strait were similarly taken *Themisto libellula* (Mandt), *Themisto bispinosa*, Boeck, *Tauria medusarum* (Kröyer), and *Onesimus littoralis* (Kröyer), together with the beautifully spinose Copepod recently figured by Buchholz, in the Report of the German North-Pole Expedition, under the name "*Cleta minuticornis*, Müller"*. It is, however, most certainly not the species described by Müller or Baird; and I would therefore propose for this very distinct Arctic form the name *Cleta horrida*.

Holsteinborg Harbour, 7–35 fathoms.

Holsteinborg Harbour produced a rich harvest of Arctic forms. The Crustacea included the great spider crab of the Greenland seas, *Chionecetes opilio* (Fabr.), the fine northern shrimps *Crangon boreas* (Phipps) and *Argis Lar* (Owen), the rare Amphipods *Onchomene minuta* (Kröyer), *Byblis Gaimardi* (Kröyer), *Œdiceros lynceus*, M. Sars, and *borealis*, A. Boeck, *Protogeneia incrmis* (Kröyer), *Antonoë macronyx*, Bruzelius, and *Photis Reinhardtii*, Kröyer, together with many species familiar to us off the British coasts. The Ostracode *Bradycinetus Brenda* (Baird) was in extraordinary abundance, along with *Cytheridea papillosa*, Bosquet, *Sclerochilus contortus* (Norman), *Xestoleberis depressa*, G. O. Sars, *Cytherura undata*, Sars, *Cythere tuberculata*, *emarginata*, *lutea*, and other species. Most of the specially Greenlandic Echinodermata occurred here. The Holothurians *Orcula Barthii*, Trosch. (? or new), *Cucumaria minuta*, Fabr., and *calcigera*, Agassiz, *Psolus Fabricii*, Düb. and Koren, *Chirodota læve*, Fabr., and *Myriotrochus Rinkii*, Steenstrup, the Ophiuridan *Ophioglypha nodosa*, Lütken, and the Asteridan *Asterias albula*, Stimpson, were associated with forms which are also European. Here, too, were such interesting Polyzoa as:—*Leieschara subgracile* (D'Orb.); *Celleporaria incrassata*, Lamk.; *Bugula Murrayana*, Bean, both the typical form and that named by Packard from the coast of Labrador (*B. fruticosa*) and by Lovén from Finmark (*B. quadridentata*). Our common *Lepralia ventricosa*, Hassall, seems to be also the most abundant Greenland Lepralian; it was found here and in all the other inshore dredgings. A new *Cellepora* in Holsteinborg Harbour and other Greenland localities had been previously sent to me by Mr. Whiteaves from the River St. Lawrence, where it was procured in the dredgings of the

* Die zweite deutsche Nordpolarfahrt in den Jahren 1869 und 1870, p. 393, pl. xv. fig. 3.

Canadian Government; it may fitly bear the name of that naturalist, *Cellepora Whiteavesi*. The Shetland *Membranipora sacculata*, Norman, furnished here another link between our fauna and that of the Greenland seas. The more remarkable Foraminifera in the locality were *Trochammina gordialis*, Parker and Jones, *Lituola Canariensis*, D'Orb., *Textularia biformis*, Parker and Jones, and *Bolivina punctata*, D'Orb.

Godhavn Harbour, Disco, 5–20 fathoms.

There are certain common British Invertebrata which are equally abundant on the Greenland coast. These animals are for the most part also circumpolar in their distribution; conspicuous amongst these are *Hyas aranea* (Linn.) and *coarctata*, Leach (of gigantic size), *Eupagurus pubescens* (Kröyer), *Solaster papposus* (Linn.), *Ophiopholis aculeata* (Müll.). With these in Godhavn Harbour were associated *Chionecetes opilio* (Fabr.), *Argis Lar* (Owen), *Hippolyte Fabricii*, Kröyer, and *turgida*, Kröyer, *Ampelisca Eschrichtii*, Kr., and *Haploops tubicola*, Lilljeborg, these two Amphipods being in great abundance. Among many Ostracoda were the rare *Cythere borealis*, G. S. Brady, *canadensis*, G. S. Brady, and an undescribed species of the same genus; and *Cytherura granulata*, G. S. Brady, and *cristata*, G. S. Brady, the two latter species being only previously known as fossil in the Posttertiary deposits of Canada. The great sea-cucumber, *Cucumaria frondosa* (Gunn), was living in company with *C. calcigera*, Agas., *Chirodota læve* (Fabr.), *Asterias albula*, Stimpson, *Ophiacantha bidentata* (Retz.), and *Ophioglypha robusta* (Ayr). Of the Polyzoa may be named:—*Scrupocellaria scabra*, var. *elongata*, Smitt; *Bugula Murrayana*, var. *fruticosa*, Packard; *Lepralia cruenta*, *sinuata*, *ansata*, *acutirostris*; *Cellepora plicata*, Smitt; and *Hippothoa divaricata*, Lamx.,—the true form, and not my *H. expansa*, which is much more abundant in the Arctic seas, and has probably been frequently recorded under the former name. Among thirty-six Foraminifera identified from this locality are *Dentalina consobrina*, D'Orb. (or the form figured under this name by Parker and Jones), *Polymorphina Burdigalensis*, D'Orb., *Pullenia sphæroides*, D'Orb., *Verneuilina polystropha*, Reuss, *Cassidulina obtusa*, D'Orb., *Pulvinulina Karsteni*, Reuss, and *Discorbina obtusa*, D'Orb.

A small quantity of material examined from the harbour consists of a ferruginous mud, which contained large quantities of the tubes of *Pectinaria* and of another more delicate Annelid. The Foraminifera among this mud were specially interesting, as exhibiting a marked parallelism with those recorded by Mr. G. M. Dawson from Gaspé Bay in the Gulf of St. Lawrence*. Of the twenty-eight Lievely-Bay species, twenty-two are also in Mr. Dawson's Gaspé-Bay list; and

* On Foraminifera from the Gulf of St. Lawrence, by G. M. Dawson ('Canadian Naturalist,' 1870).

these include two very marked forms not hitherto found in any other localities, namely *Rhabdopleura abyssorum*, Parker, and *Lituola cassis*, Parker, and also *Nonionina Labradorica*, Parker, and *Bulimina pyrula*, D'Orb. The Ostracoda include *Cythere tuberculata*, G. O. Sars, *C. Canadensis*, G. S. Brady, and *Paradoxostoma flexuosum*, G. S. Brady; the last of gigantic size as compared with the dimensions it attains in our own seas.

Station No. 1. *Off Hare Island, Waigat Strait, at the entrance of Baffin Bay*; 175 *fathoms.*

The chief features in the dredging were the magnificent *Astrophytons* of the two Arctic species *Agassizii*, Stimpson, and *eucnemis*, Müll. & Trösch., and the abundance of luxuriantly developed *Hornera lichenoides*, upon the branches of which were living many other very rare Polyzoa. *Hippomedon abyssi* (Goës), *Pontoporeia femorata*, Kröyer, and *Amphithopsis latipes* (M. Sars) among the Amphipoda, *Pallene intermedia*, Kröyer, *Nymphon grossipes*, Fabr., and *N. hirtipes*, Bell, among the Pycnogonoidea, were the most interesting Crustacea. *Ctenodiscus crispatus*, Retz., was abundant; and the only example of *Antedon Eschrichtii*, Müll. & Trosch., taken in the expedition occurred here. The Polyzoa were many and good; for example, *Eschara elegantula*, D'Orb., *Leieschara subgracile* (D'Orb.), *Idmonea Atlantica*, Forbes, *Alecto diastoporides*, Norman, *Menipea arctica*, Busk, and *Discopora sincera*, Smitt.

Station No. 3. *Lat.* 69° 31′ *N., Long.* 56° 1′ *W.*; 100 *fathoms.*

Among the Crustacea here were the extraordinary Isopod *Munnopsis typica*, M. Sars (which that excellent naturalist elaborately described in the last work published before his lamented death*), *Glauconome leucopis*, Kröyer, *Hippomedon abyssi* (Goës), and *Aceros phyllonyx* (M. Sars). Among the Polyzoa *Flustra membranaceo-truncata*, Smitt, and a new *Lepralia*, which in many respects comes near *L. trispinosa*, but which, in addition to the acute-mandibled avicularia, has very numerous ovoid avicularia with rounded mandible scattered over the cells, while the mouth wants the spout-like projection of the lower margin, and is somewhat different in outline: this form may be named *L. Jeffreysi*. Among the Echinodermata were *Myriotrochus Rinkii*, Steenstrup, *Asterias Grœnlandica*, Stimpson, *Ophioglypha Sarsii*, Lütken, *Ophiocten sericeum* (Forbes), *Amphiura Sundevalli*, Müll. & Trosch., and *Astrophyton eucnemis*, Müll. & Trosch. The Arctic Actinian *Chrondactinia nodosa* (Fabr.) was also procured here, together with an undescribed *Ammothoa*, which Dr. Lütken has kindly given me the means of comparing and identifying with his *Ammothoa arctica*, Lütken, MS.

* Bidrag til kundskab om Christiania-Fjordens Fauna, 1868, pls. vi. & vii. pp. 70–95.

Station No. 4. *Lat.* 67° 50′ *N., Long.* 55° 27′ *W.*; 20 *fathoms.*

Rare Crustacea were met with here which had not been found in the preceding localities:—*Hippolyte spinus*, Leach, *aculeata*, Kröyer, and *Phippsii*, Kröyer; *Socarnes Vahlii* (Rhdt.); *Anonyx lagena* (Rhdt.), and *gulosus*, Kröyer; *Onesimus plautus* (Kröyer); *Vertumnus serratus* (O. Fabr.); *Amphithoë carinata*, Kröyer; *Œdiceros saginatus*, Kröyer; *Podocerus anguipes*, Kröyer; *Caprella septentrionalis*, Kröyer; *Cythere Finmarchica*, G. O. Sars, and *angulata*, G. O. Sars; *Cytheropteron latissimum* (Norman), and *pyramidale*, G. S. Brady; and *Cytherura clathrata*, G. O. Sars; and the only example procured of the circumpolar *Solaster endeca* (Lin.) and of *Pteraster militaris* (Müller) were from this locality.

Station No. 5. *Lat.* 66° 59′ *N., Long.* 55° 27′ *W.*; 57 *fathoms.*

A wonderfully rich dredging. Bottom sand and shells, with an immense profusion of *Rhynchonella psittacea*, Ch. (living and dead, the latter covered with Polyzoa), *Balanus porcatus*, Da Costa, *Cynthia rustica*, L., and *Alcyonidium gelatinosum*, L. Among these animals Crustacea were living in extraordinary numbers, which included almost the whole of the Arctic Palæmonidæ and Crangonidæ, namely, *Crangon boreas* (Phipps), *Hippolyte Gaimardi*, M.-Edw., *gibba*, Kröyer, *borealis*, Owen, and *aculeata*, Kröyer; the Cumacea, *Diastylis Edwardsii* (Kröyer), *D. Rathkii* (Kröyer), and *Campylaspis rubicunda* (Lilljeborg); the Amphipoda, *Vertumnus inflatus* (Kröyer), *Stegocephalus ampulla* (Phipps), *Eusirus cuspidatus*, Kröyer, *Melita dentata*, Kröyer, *Gammaropsis erythrophthalmus*, Lilljeborg; and the Entomostraca, *Nebalia bipes* (Fabr.), *Cythere dubia*, G. S. Brady, *Cytheropteron punctatum*, G. S. Brady, *Cytherideis foveolata*, G. S. Brady (only previously known in the Gulf of St. Lawrence), and *Polycope orbicularis*, G. O. Sars. On *Hippolyte spinus*, Sow., there was the parasitic Isopod *Bopyrus abdominalis*, Kröyer; and on the abdomen of *Hippolyte borealis*, Kr., the parasitic Cirriped *Sylon Hippolytes*, Kr. Altogether there were no less than fifty species of Crustacea in this dredging. It was also very rich in Polyzoa, among which were:—*Escharoides rosacea* (Busk), and *Sarsii*, Smitt; *Leieschara subgracile* (D'Orb.), and *crustaceum*, Smitt; *Eschara elegantula*, D'Orb.; *Lepralia crystallina*, Norman, *labiata*, Busk, *bellis*, Busk, *hippopus*, Smitt, *spathulifera*, Smitt, and many others; *Hippothoa expansa*, Norman; and luxuriant growths of *Celleporaria incrassata*, D'Orb. The Echinodermata included *Asterias polaris*, Müll. & Trosch., *Ophiacantha bidendata* (Retz.), and *Ophioglypha robusta*, Ayr. Of thirty-five Foraminifera, ten belonged to the genus *Lagena*, rarest among which was the Greenlandic *L. striato-punctata*, Parker and Jones; there were also *Lituola globigeriniformis*, Parker and Jones, *Cyclammina cancellata*, H. B. Brady, MS., and *Bulimina elegantissima*, D'Orb.

Station No. 6. *Lat.* 64° 5′ *N., Long.* 56° 47′ *W.*; 410 *fathoms.*

Only a very small quantity came up in the dredge; but every scrap was a treasure, and showed that we as yet knew nothing whatever of the rich fauna which lies hid in the depths of the sea in the Arctic regions. Two actinozoans of the highest interest occurred here. Of these the first is a remarkably fine Gorgonian belonging to the genus *Mopsea*. It differs entirely from the recently described *Mopsea borealis*, M. Sars*, the only previously known northern form, and approaches much more nearly to the character of species from tropical seas. It grows in the form of a thick little bush, 6 inches high (probably, at least, 9 inches when perfect). The main stem continuously divides with verticils of three or four branches each, and the branches thus formed similarly subdivide. The polyps, instead of being short as is the case in *Mopsea borealis*, are very long, longer even than in *Mopsea Mediterranea*, Risso†. The form may be named *Mopsea arbuscula*. In floating the sharp sand of this dredging to separate the Foraminifera and Ostracoda, a tip of a branch of *Antipathes arctica*, Lütken, was procured. Although this fragment was not more than a quarter of an inch long, there can be no doubt of its belonging to the species described by Dr. Lütken‡; and we thus obtain a habitat for this Arctic form of what is otherwise known only as a marked tropical genus, if we except an as yet undescribed species found in the 'Porcupine' Expedition of 1869. The type and only known specimen of *Antipathes arctica*, described by Dr. Lütken, was found in the stomach of a shark (*Scymnus microcephalus*), in Rodebay, about two miles north of Jakobshavn in Greenland, by M. K. Fleischer. The Spatangoid *Schizaster fragilis* (Düb. and Kor.) was also dredged here, and is an addition to the Greenland fauna, to which it is remarkable that no Spatangoid and only one Echinoid, *Toxopneustes Dröbachiensis* (Müll.), was previously known to belong. The few Foraminifera did not include any species worth special notice; but among the Ostracoda were *Cytheridea Sorbyana*, Jones, and *Cythere abyssicola*, G. O. Sars.

Station No. 7.

Nothing received from this station.

Station No. 8. *Lat.* 62° 6′ *N., Long.* 55° 56′ *W.*; 1350 *fathoms.*

The very small quantity of sand from the sounding of this station contained, among many more common Foraminifera, a Nodosarian which incorporates sand and extraneous matter in its shell-substance, and appears

* On some remarkable Forms of Animal Life from the Great Deeps of the Norwegian Coasts (Christiania, 1872), pp. 50–57, pl. v. figs. 1–23.

† Hist. Nat. des principales productions de l'Europe Méridionale, vol. v. p. 332, pl. viii. figs. 43, 44.

‡ Oversigt over det Kongl. Danske Vidensk. Selsk. Forhandl. 1871 (translated Ann. & Mag. Nat. Hist. 1872, ser. 4, vol. x. p. 77).

to be the same as the Tertiary fossil figured by Schlicht from Pietzpuhl* (pl. vi. figs. 29–32), and which has been named by Reuss *Nodosaria Schlichtii*; this sand-incorporating form seems common in the depths of the North Atlantic, as I have observed it not only in many of the 'Valorous' dredgings, but also in several of those of the 'Porcupine,' 1869. *Orbitolites tenuissimus*, Carpenter †, *Pullenia quinqueloba*, Reuss, and *Lituola nautiloidea*, Lamk., also occurred here; the *Orbitolites* seems to have a wide distribution in the deep sea, as I have just received specimens from the Marquis da Monterosato which he dredged in 100–200 fathoms off the Sicilian coast.

Station No. 9. *Lat.* 59° 10′ *N.*, *Long.* 50° 25′ *W.*; 1750 *fathoms.*

This was the last deep-water dredging in Davis Strait. A remarkable new genus of Echinoidea occurred here. In general outline it is almost cylindrical, the length being to the breadth as 5 to 2; and the height, which is greatest in the centre, exceeds the breadth. Viewed laterally the cylindrical form is interrupted anteally by two fifths of the length of the animal being sloped away anteriorly above. This sloped-away portion of the Spatangoid is surrounded by a well-marked fasciole, containing within it the ambulacral system, which is thus excentric and confined to the anterior portion of the animal; the four lateral ambulacra are remarkably short, consisting of only four or five pairs of pores each: the anterior odd ambulacrum is much larger, and consists of nine pairs of pores, which are of much larger size than those of the lateral ambulacra; it is situated in a broad but very shallow depression. The tentacles of the upper portion of the odd ambulacrum are very large and remarkable, of umbrella-like form, supported on flexible columns, which are densely studded and strengthened with fusiform nodulous spicula. The spines are of two forms, battledoor-shaped and of the more usual form. Pedicellariæ of two if not of three kinds. Mouth inferior, at one third the length of the animal from the anterior extremity, not situated in a groove (as is the case in *Pourtalesia*). Anal aperture dorsal, at about one fourth the length of the animal from the posterior extremity, nearly flush with the surface, neither in a deep depression (as in *Pourtalesia*) nor in an anal groove.

In its elongated form this genus shows an approach to *Pourtalesia*, but in mouth, anal aperture, and the condition of the ambulacral system it is altogether different. The nearest approach I know to the general outline of this genus in found in the Chalk fossil *Archiacia sandalina*, Ag.; but in *Archiacia* the anal aperture is inferior. Indeed the conditions of this organ are altogether exceptional; for in those known genera

* 'Die Foraminiferen des Septarienthones von Pietzpuhl,' 1870, pl. vi. figs. 29–32. Reuss, Sitzb. d. k. Akad. d. Wissensch. 1. Abth. Nov.-Heft, 1870, 'Die Foraminiferen des Septarienthones von Pietzpuhl' (separate copy, p. 18).

† Thomson's 'Depths of the Sea,' woodcut, p. 91.

in which it assumes a dorsal position (e.g. *Cassidulus*, *Echinobrissus*, *Clypeopagus*, and other genera) it is always sunk in an anal groove. This new and most interesting form will be named *Aërope rostrata* by Sir Wyville Thomson*. In this dredging were also procured a specimen of *Leucon longirostris*, G. O. Sars† (which was described by him from a fragment procured in the 'Josephine' Expedition), *Leucon serratus*, Norman, a new *Diastylis* (*D. armata*), and five undescribed Isopoda. This dredging was also by far the most important as regards the Foraminifera. The *Globigerinæ* here presented an entirely different aspect from that of those usually met with—so much so that they might have been taken to belong to a different species; the segments have a comparatively compactly compressed appearance, very different from the rounded, swollen outline so characteristic usually of the chambers of *Globigerina bulloides*. The ooze, moreover, has a reddish tinge, and contains a large number of remarkable arenaceous Foraminifera, and more Polycystina than are usually met with in North-Atlantic dredgings. From the peculiar appearance of the *Globigerinæ* and the character altogether of this dredging, it would seem that we have here the commencement of that transition state of the sea-bed between the '*Globigerina*-ooze' and the 'Red Clay‡' which has been termed by Sir Wyville Thomson "Grey ooze," and has been found by the 'Challenger' Expedition to constitute the bottom at depths of about 2500 fathoms in the South Atlantic. I am thus led to infer that the peculiar form of the *Globigerinæ* is dependent partially or wholly upon incipient decomposition. The arenaceous Foraminifera are an extraordinary assemblage. They embrace no less than eighteen distinct and well-marked forms, most of the more conspicuous species found in the 'Porcupine' Expedition, viz. *Rhabdammina*, *Pilulina*, what Carpenter has called 'nodosarine,' 'moniliform,' 'nodosarine No. 2,' 'globigerine,' 'orbuline,' and 'orthocerine' *Lituolæ* §, and other forms. With these there are others which have not been before observed, one of which must not be passed without notice. The genus *Astrorhiza* was constituted by Sandahl ‖ for the reception of a large flat disk-like Rhizopod, having a test which consists of extraneous matter

* When this description was read I had suggested a name for the present species; but having since learnt from Sir Wyville Thomson that it has also been procured in the 'Challenger' Expedition, I gladly adopt the above name, under which I found that he was about to describe it.

† Beskrivelse af de paa Fregatten Josephines Expedition funde Cumaceer, 1871, p. 42, pl. xv. fig. 75.

‡ Proc. Roy. Soc. 1874, vol. xxiii. p. 39 *et seq.*

§ See Carpenter, 'The Microscope,' 5th edit. 1875, pp. 531–535, and woodcuts.

‖ Öfversigt af Kong. Vet. Akad. Förhand. 1857, p. 301, pl. iii. fig. 526. The same species has since been described by Bessels, Jenaische Zeits. für Naturwiss., heraus. von der med.-natur. Gesellschaft zu Jena, 1857, p. 265, pl. xiv., under the name *Haeckelina gigantea*; and by Schultze, 'Jahresbericht der Commission zur wissenchaft. Untersuchung der deutschen Meere in Kiel für 1872–73,' pl. ii. fig. 10, under the name *Astrodiscus arenaceus*.

(pieces of shell, sand, and other materials) roughly cemented together, apparently without any selective power being exercised in the choice of the materials. From the edge of the disk proceed numerous spoke-like radii, giving the whole animal a stellate appearance; pseudopodia are extruded from the end of these radii; and Bessels has shown that in its most perfect state a number of these disks are attached to each other by their radii, so as to form a flat network animal, of which each disk will represent a chamber. This remarkble animal, which I have frequently taken off the British coast, was called by Sandahl *Astrorhiza limicola*. In the 'Porcupine' Expedition of 1869 a Rhizopod was dredged between Shetland and Färöe which had a much less regular outline, being sometimes stellate and sometimes cervicorn, and the test was composed entirely of fine sand-grains cemented together; to this Dr. Carpenter has given the MS. name *Astrorhiza arenaria**. At station No. 8 a beautiful form was found which must also be referred to this genus; the chambers are more or less ovoid, not flattened as in the previously known forms, but equally rounded on the sides and above and below; the spoke-like pseudopodian processes, instead of being all in one plane, as in *A. limicola*, radiate in all directions. Several specimens occurred in which two chambers were united together, a fresh chamber being developed at the end of one of the radiating processes; and it is probable that in its most perfect state the animal would consist not only of a series of chambers extended on all sides, as in *A. limicola*, but of other chambers superimposed on these, so that the whole animal would be of a most complex type. The arenaceous investiture consists of fine sand-grains and sponge-spicules firmly (not loosely as in *A. arenaria*) cemented together, and is of a ruddy hue, but not ferruginous. *Astrorhiza catenata*, n. sp., may be the name to distinguish this animal. Together with several more new arenaceous forms are two calcareous Foraminifera, which though known as fossils are now for the first time met with in a living state; the one is *Cristellaria obvelata*, Reuss†, the other is one of the most beautiful species I have ever seen, and is clearly the same as the fossil described by Karrer in his 'Zur Foraminiferenfauna in Österreich,' under the name *Orbulina Neojurinensis*, Karrer‡. I may add that one of the arenaceous forms is very near to, if not identical with, *Globigerina arenaria*, Karrer, described in the same paper.

Station No. 12. *Lat.* 56° 11′ *N., Long.* 37° 41′ *W.*; 1450 *fathoms*.

A bottom of *Globigerina*-ooze and pebbles. The Crustacea here met

* Since the above was written Dr. Carpenter has published a description of this species, and well illustrated its various forms, though he has not given it a specific name (Quart. Journ. Micr. Science, April 1876, p. 221, pl. xix.). It is to be hoped that Dr. Carpenter will before long give us his anxiously looked-for Report on the Foraminifera of the 'Porcupine' Expedition.

† I am indebted to Mr. H. B. Brady for the identification of this form, and for much kind assistance with respect to the Foraminifera.

‡ Sitzb. d. k. Akad. d. Wissensch. 1. Abth. April-Heft, Jahrg. 1867, pl. iii. fig. 10.

with include *Cyclaspis longicaudata*, G. O. Sars (which was described by him from the Lofoden Islands, where it was found in 150 fathoms), three new Isopoda belonging to the family *Tanaidæ*, and fourteen Ostracoda, for the most part new and very fine species, but including also *Bairdia fusca*, G. S. Brady (only known before from Australia), *Bairdia subdeltoidea*, Von Münster, and *Bairdia Crosskeiana*, G. S. Brady (described from the Levant), *Cythere scabra*, Von Münster, and *Cythere echinata*, G. O. Sars (known before from the Lofoden Islands). The Foraminifera include a fine form of the very rare *Cornuspira margaritifera*, Williamson, *Lagena pulchella*, H. B. Brady, *Nonionina pompiloides*, F. & M., *Pulvinulina pauperata*, P. & J., and *Bolivina plicata*, D'Orb. Here, too, was procured the most interesting sponge of the cruise, being a fragment of what appears to form when perfect a large cup or fan-shaped Hexactinellid, nearest allied perhaps to *Farrea occa*.

Station No. 13. *Lat.* 56° 1′ *N., Long.* 34° 42′ *W.*; 690 *fathoms.*

A bottom of rock and sand. Notwithstanding the difference in depth between this and the last station, out of thirteen Ostracoda found here eight are common to the two localities; and of the remaining five, four are perhaps new, and the last is the *Cythere abyssicola* of Sars. Among about fifty species of Foraminifera are two *Biloculinæ* (which do not seem referable to any of the numerous recent and fossil forms already described), a pedunculate *Planorbulina* (which was also taken in the 'Porcupine' Expedition off Färöe, but is still undescribed), together with *Cyclammina cancellata*, H. B. Brady, MS., *Rheopax scorpiurus*, Montfort, *Gaudryina pupoides*, D'Orb., and *Orbitolites tenuissimus*, Carpenter.

Station No. 14. *Lat.* 55° 58′ *N., Long.* 31° 41′ *W.*; 1230 *fathoms.*

Remarkable among about thirty Foraminifera are a beautiful large variety of *Uvigerina pygmæa*, D'Orb., in which the ribs are elevated into strong plicæ, and the delicate, perfectly transparent, and extremely fragile genus *Cheilostomella*, which is now for the first time recorded as occurring in a recent state. Specimens have, however, been in my collection some years, which I found among sand dredged in 1870 by Dr. Jeffreys's yacht 'The Osprey,' in 112 fathoms, off Valentia Island.

Station No. 15. *Lat.* 55° 58′ *N., Long.* 28° 42′ *W.*; 1485 *fathoms.*

Only a very small quantity of material examined from this locality; and it contains nothing worthy of special remark, except that a fragment of *Orbulina Neojurinensis*, Karrer, gives a second locality for that fine addition to recent Foraminifera.

Station No. 16. *Lat.* 55° 10′ *N., Long.* 25° 58′ *W.*; 1785 *fathoms.*

Among the *Globigerina*-ooze of this the deepest dredging of the 'Valorous' Expedition there was a mutilated specimen of an Echinoderm

belonging to the remarkable abyssal genus *Pourtalesia*. The specimen seems referable to *P. phyale*, Wyv. Thomson. Here, too, was a new Ophiuridan belonging to that section of the genus *Amphiura* which is devoid of tentacle-scales. Of this section it belongs to the subsection* which has the arm-spines simple (that is, not hatchet-formed as in *A. filiformis* and its allies); and it may be distinguished among other characters from *A. Atlantica*, Ljungman, the only other species falling into this subsection, by having only three instead of six arm-spines. The present form may be named *Amphiura abyssorum*, n. sp.

Two young Asteroidea which occurred here, although they unquestionably have not attained their mature form, have characters so distinct that we cannot refer them to any described starfish. Though differing in all details they are alike in general outline, which resembles that of our well-known *Porania pulvillus*. In one case each angle of the disk terminates in a large calcareous plate bearing a large central spine flanked on each side by smaller spines; in the other case each angle bears three spines which project upwards from the dorsal surface. In the organs of the mouth and those of the ambulacra these two Asteridans are far removed from each other. Like station No. 12, this dredging produced several undescribed Isopoda and Ostracoda; and among the Foraminifera are *Glandulina lævigata*, var. *gracilis*, Reuss, a Nodosarian which has been already referred to as apparently identical with the *Nodosaria Schlichtii*, Reuss, *Candeina nitida*, D'Orb., and *Discorbina Parisiensis*, D'Orb.

Annelida.

By W. C. M'Intosh, M.D., F.R.S.E.

The Annelida collected during this expedition were kindly placed in my hands by Dr. Gwyn Jeffreys on his return. They resembled in many respects those recently examined from the Gulf of St. Lawrence, though the series was in neither the same.

Besides the Annelids mentioned in the following list, one Nemertean is abundant. The colour is brownish purple on the dorsum, whitish beneath. The short body and large proboscis distinguish it from *Nemertes Neesii*; but it may be related to the *Amphiporus Grœnlandicus* of Örsted. The empty tubes of some of the Annelids are interesting; thus the *Globigerina*-tubes are bristled with sponge-spicules, and the latter are also used by the *Terebellæ* in forming the processes at the anterior apertures. A remarkable one occurred at a depth of 1785 fathoms, amongst the *Globigerina*-ooze (station No. 16, 'Valorous'). It consists of a slender tube (about the thickness of a stout thread) of fine greyish mud, and having at one end an enlargement. The latter is tufted with what at first sight (under a lens) appears to be minute and

* *Vide* Ljungman's paper on the Ophiuridans procured in the 'Josephine' Expedition, Öfversigt af K. Vet. Akad. Förhand. 1871, p. 643.

finely ramose algæ; but the microscope shows that these finely branched processes are composed of the same elements as the tube, so that the animal probably fashioned it to be in harmony with such structures. One bottle from station No. 9 contained about twenty rich madder-brown tentacles (apparently of a *Medusa*) which were brought up by the sounding-line; they were studded with finely formed thread-cells.

Reference will first be made to the Annelids as they occur in the sequence of their families, and thereafter a note of the collection in its bathymetrical and geographical aspects will be appended.

The Euphrosynidæ are represented by fine examples of *Euphrosyne borealis*, Örst. While no example of the Amphinomidæ or Aphroditidæ occurs, the Polynoidæ, on the other hand, are common. The most abundant of the family, perhaps, is *Nychia cirrosa*, Pallas, the size of the specimens somewhat exceeding those from Shetland and Canada. One has its skin studded throughout with small, firm, whitish tubercles, caused by subcutaneous masses composed of a vast number of minute ovoid bodies with a firm external wall, differentiated from the slightly granular central region. No change was produced by the addition of acetic acid, but sulphuric ether shrivelled them considerably. The structures seemed to be of parasitic origin. *Nychia Amondseni*, Mgrn., occurs likewise in fine condition. *Eunoa Oerstedi*, Mgrn., is represented by specimens an inch and three quarters long, and therefore considerably larger than those from Canada. The ubiquitous *Harmothoë imbricata*, L., is abundant, and there is nothing peculiar in size or coloration; indeed much larger examples exist in my collection from Exeter Bay, Greenland. Some varieties approached *Evarne impar*, Johnst. (a species also present); but they could always be recognized by the position of the eyes, the structure of the bristles, and other points. Many had parasitic Infusoria on the bristles. A fragment of *Eupolynoë occidentalis*, M'Int., with scales, demonstrates that the latter become in spirit of wine of a ferruginous brown hue and smooth. They have rather numerous, long, slender cilia on their outer border, and the tips of these processes are very slightly dilated. The *Lepidonotus sublevis* of Prof. Verrill* may be this or an allied form; but his description is unfortunately too lax for determination. The specimen had several parasitic Pedicellinæ.

The Sigalionidæ are represented by a fragmentary *Leanira* from 1785 fathoms (station 16), and by excellent examples of *Pholoë minuta*, Fabr.

Two species of the family Nephthydidæ occur, viz. *Nephthys cæca*, Fabr., and *N. incisa*, Mgrn., the former having many parasitic Infusoria on its bristles.

Of the Phyllodocidæ are *Phyllodoce Grœnlandica*, Örsted (large specimens), *Eulalia viridis*, O. F. Müller, *Eteone pusilla*, Örst., and *Eteone flava*, Fabr., besides a fragmentary and minute *Phyllodoce*.

* Invertebrate Animals of Vineyard Sound, p. 581.

There is no example of the Hesionidæ or Syllidæ. Good specimens of *Nereis pelagica*, L., and *Nereis zonata*, Mgrn., represent the Nereidæ. The latter shows fine reddish-brown bars in the preparations. Under the family of the Lumbrinereidæ is *Lumbriconereis fragilis*, O. F. Müller, a species tolerably plentiful and of all sizes, though none are so large as those procured in the 'Porcupine' off the Spanish coast. The Eunicidæ and Onuphididæ are each represented by a single species, viz. the former by the *Eunice limosa*, Ehlers, and the latter by very large examples of *Nothria conchylega*, Sars, in tubes of small pebbles.

In the Glyceridæ are *Glycera capitata*, Örst., and *Glycera setosa*, Örst., the latter having been procured at the surface of the sea in Waigat Strait.

Four forms belonging to the Ariciidæ occur, viz. a new species allied to *Aricia Kupfferi*, Ehlers, two small fragments pertaining to different species, and *Scoloplos armiger*, O. F. Müller. In the first-mentioned species the bristles are much more developed than in *Aricia Latreillii*, while no pectinate rows of papillæ exist on the ventral surface, as in the latter, *Aricia Cuvieri*, Aud. & Ed., and *Aricia Kupfferi*, Ehlers. In this respect it agrees with *A. Norvegica*, Sars, a form which extends to the Atlantic, but differs in the structure of the process at the summit of the pectinate rows of the anterior feet. The bifid bristles in the superior division of the foot appear to be characteristic; but as previous authors seem to have overlooked them (they are present in all the species examined), the value of this test is at present not fully available. The specimen is imperfect, and measures about $\frac{3}{4}$ of an inch in length. The snout is elongated and pointed, and has traces of blackish pigment near the posterior border, dorsally. The mouth opens on the ventral surface between the posterior border of the head and the first bristle-bearing segment; and on each side it has two large curved lobes, which pass backward to the commencement of the second bristle-bearing segment. There are sixteen lateral rows bearing hooks anteriorly (2nd to the 17th), and then the form of the foot alters. These rows are for the most part furnished with pectinate papillæ, the superior being in each case longer than the others. The branchiæ commence on the fifth segment. There is no trace of pectinate rows of papillæ on the ventral surface. The forked bristles of the dorsal division of the foot have one of the limbs considerably longer than the other, and peculiarly curved.

The Opheliidæ are represented by good specimens of *Ammotrypane aulogaster*, H. Rathke, numerous examples of *Ophelia limacina*, H. R., a *Travisia*-like Annelid with no other appendage than minute bristles, and two interesting new forms. For the latter and several allied species the name *Tachytrypane* may be given, from their active boring movements through sand and mud. Though in external appearance the new forms somewhat resemble *Ammotrypane*, the structure of the body-wall and other points clearly separate them both from the latter and *Ophelia*.

The first species (*Tachytrypane Jeffreysi*) comes from station No. 9, at a depth of 1750 fathoms. The body is elongate, being upwards of two inches in spirit, and about one tenth of an inch in breadth, rounded in front, but marked by the usual ventral ridges throughout the rest of its extent. There are about thirty segments besides the head and tail, the anterior being short, those in the middle very long. The head forms a short cone, with a minute filiform process at the tip. No bristles are visible under a lens, the body being smooth and iridescent, like that of *Linotrypane apogon*, for the fine transverse lines are not conspicuous. Very minute simple bristles are, however, present: there are no cirri. The caudal process is separated by a well-marked furrow from the rest of the body, and terminates in a slender recurved process bent downward and forward.

In the structure of the body-wall *Tachytrypane Jeffreysi* forms an intermediate link between *Linotrypane** and the other forms connecting both with *Ammotrypane* and *Ophelia*. The cuticle is greatly thickened (as in *Linotrypane*), and a special process passes inward from the median line on the ventral surface towards the nerve-cord. From each side of the latter the oblique muscle slants to the body-wall, cutting off a segment of the longitudinal muscular layer; but the projection of the separated region is much less than in the Connemara specimen figured in the Ann. & Mag. Nat. Hist. 1875, xvi. p. 369, and therefore more closely allied to the condition in *Linotrypane*. In the latter the slender oblique muscle passes on each side from the ventral raphe upward and outward to the body-wall, and the band causes no separation of the comparatively large segment included between it and the ventral raphe. The oblique muscle is much shorter and more powerful in *Tachytrypane*, and the condition leads to that in allied forms which more nearly approach *Ammotrypane*. In the latter the change is much greater, for a single deep narrow muscle (apparently representing the coalesced oblique muscles) occurs at the ventral border, with the nerve-cord beneath; and the two segments, which in *Tachytrypane* only bulge to a slight degree, are here separated from the body by the whole breadth of the deep transverse muscle just mentioned, so that in cross-section each forms a prominent pear-shaped lobe attached by a narrow pedicle. A further differentiation is apparent in *Ophelia*; for the great transverse ventral muscle splits, and a division passes into the ventral pedicle on each side, while the nerve-cord occupies the median line superiorly.

The other species, *Tachytrypane arctica*, is minute, more slender than a young *Ammotrypane aulogaster* of the same length, and is devoid of cirri, but the bristles are prominent and curve backward. The cephalic lobe is less pointed, a pigment-speck exists on each side at the base, and the form of the caudal process is characteristic, being funnel-shaped and terminating in a smooth rim. The cuticle is dense, though less developed

* Proc. Roy. Soc. Edinb. vol. viii. p. 386 (1873-74).

than in the foregoing species. It was dredged at station No. 16 in 1785 fathoms. Both species had the intestinal canal filled with fine *Globigerina*-mud; so that in all probability they live in this easily penetrated medium, after the manner of their congeners in the sand.

There is a single example of the Scalibregmidæ, viz. *Scalibregma inflatum*, H. Rathke; while two forms represent the Chloræmidæ, viz. *Trophonia plumosa*, O. F. Müller, and *Flabelligera affinis*, Sars. The specimens of the latter are large.

Scolecolepis cirrata, Sars, *Prionospio Steenstrupi*, Mgrn., and a fragmentary *Spio* from station No. 3 typify the Spionidæ.

There is an example of the Cirratulidæ (*Cirratulus cirratus*, O. F. Müller), and one of the Halelminthidæ, viz. *Capitella capitata*, Fabr.

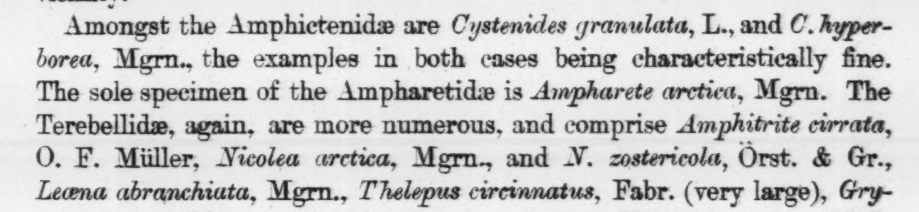
The Maldanidæ comprise *Maldane biceps*, Sars, *Maldane Sarsi*, Mgrn. (a common Canadian form), *Nicomache lumbricalis*, Fabr., *Axiothea catenata*, Mgrn., and *Praxilla prætermissa*, Mgrn.

The family Ammocharidæ is represented by *Owenia filiformis*, D. Ch., the tubes of which closely resemble the stalks of *Alcyonidium* growing in the vicinity.

Amongst the Amphictenidæ are *Cystenides granulata*, L., and *C. hyperborea*, Mgrn., the examples in both cases being characteristically fine. The sole specimen of the Ampharetidæ is *Ampharete arctica*, Mgrn. The Terebellidæ, again, are more numerous, and comprise *Amphitrite cirrata*, O. F. Müller, *Nicolea arctica*, Mgrn., and *N. zostericola*, Örst. & Gr., *Leæna abranchiata*, Mgrn., *Thelepus circinnatus*, Fabr. (very large), *Grymæa Bairdi*, Mgrn., and *Trichobranchus glacialis*, Mgrn., in tubes, chiefly composed of *Globigerinæ*. Certain fragments not yet determined pertain to this family.

The Sabellidæ are represented by *Sabella pavonia*, Sav., *S. vesiculosa*, Mont., *Euchone analis*, Kr., *Chone infundibuliformis*, Kr., and the Serpulidæ by a fragmentary, minute, shining quinquecostate *Serpula**, with four transverse ridges anteriorly and irregularly twisted, *Spirorbis borealis*, Daud., *S. verruca*, Fabr., *S. spirillum*, L., and the tube of an apparently new *Ditrypa*, which may be characterized as follows:—*D. Grœnlandica*. The tube is about half an inch in length, not much thicker than a thread, and curved from end to end like a bow (fig. 1). It tapers

2



1

* *Placostegus quinquecostatus*, Daud., from the Mediterranean may be an allied form.

very gradually from the anterior to the posterior end, contrasting in this respect with the more decided diminution in *D. arietina.* The oval aperture (fig. 2) forms a smooth slightly constricted rim, which is narrower than the tube almost by the depth of the ridges. The latter are eight in number, and run from the anterior to the posterior end of the tube, though, it must be added, none of the specimens were quite perfect. It was obtained from station No. 12 (1450 fathoms). It differs from any other *Ditrypa* known to me in its slender form and the well-marked longitudinal ridges.

A curious form, the affinities of which have not yet been made out, occurs at station No. 6 (410 fathoms). The head forms a blunt cone, with two short tentacles near a large eye-speck on each side. The segments are deeply cut. The dorsal division of the foot has a single large hook (somewhat resembling that figured by the late M. Claparède in Dr. Ehlers's recent paper* on the 'Porcupine' deep-sea species). The ventral bristles taper to a flattened tip with a minute claw. A series of small papillæ occur on the foot and on the conical cirrus beneath the dorsal hook.

Station.	Depth in fms.	Position.	Character of bottom.	Species.
1875. No. 1. July 22 ...	175	70° 30′ N., 54 41 W. Off Hare Island, Disco.	Sandy mud.	Euphrosyne borealis, Eupolynoë occidentalis, Nephthys incisa, Nereis zonata, Nothria conchylega, Lumbriconereis fragilis, Trophonia plumosa, Scolecolepis cirrata, Maldane biceps, Nicomache lumbricalis.
No. 3. July 23 ...	100	69 31 N., 56 1 W.	Sandy mud.	Nychia Amondseni, Nephthys incisa, Nereis zonata, Nothria conchylega, Lumbriconereis fragilis, Prionospio Steenstrupi.
No. 4. July 24 ...	20	67 50 N., 55 27 W.	Shell-sand.	Harmothoë imbricata, and fragmentary Nephthys, sp. n., Phyllodoce Grœnlandica, Eteone pusilla, Nereis pelagica, Glycera capitata, three sp. of Aricia, Scoloplos armiger, Ophelia limacina, Spio, sp., Capitella capitata, Ampharete arctica, Euchone analis, Chone infundibuliformis, Amphiporus.
No. 5. July 26 ...	60	66 59 N., 55 27 W.	Sand & shells.	Harmothoë imbricata and var., Eteone pusilla, Eulalia, sp., Nereis pelagica, Nothria conchylega, Owenia filiformis, Leæna abranchiata, Spirorbis spirillum, Amphiporus as before.
No. 6. Aug. 10 ...	410	64 5 N., 56 47 W.	Sandy mud.	Evarne impar, Nereis pelagica, Lumbriconereis fragilis, Eunice limosa, Aricia Cuvieri, Scolecolepis cirrata, Leæna abranchiata, Grymæa Bairdi, Terebellides Strömi, a remarkable form with dorsal hook, Chætoderma.

* Zeitsch. f. wiss. Zool. xxv. Bd. i. Taf. i. f. 13.

Station.	Depth in fms.	Position.	Character of bottom.	Species.
1875. No. 7. Aug. 11 ...	1100	63° 9′ N., 56 43 W.	Mud.	Lumbriconereis fragilis, Glycera capitata, Scoloplos armiger, Praxilla prætermissa, Terebellides Strömi.
No. 9. Aug. 14 ...	1750	59 10 N., 50 25 W.	Mud.	Glycera capitata, Tachytrypane Jeffreysi, Globigerina-tubes.
No. 10. Aug. 16 ...	400–1400	58 14 N., 46 29 W.	Came up on sounding-line.	Twenty tentacles of a madder-brown Medusa.
No. 12. Aug. 19 ...	1450	56 11 N., 37 41 W.	Globigerina-ooze & stones.	Trichobranchus glacialis, Ditrypa Grœnlandica, Globigerina-tubes.
No. 16. Aug. 23 ...	1785	55 10 N., 25 58 W.	Globigerina-ooze.	Leanira, sp., Lumbriconereis fragilis, Travisia-form, Tachytrypane arctica, Prionospio Steenstrupi, Owenia filiformis, remarkable tube with fine ramose filaments.
Holsteinborg Harbour.	7–12	....	....	Nychia cirrosa, Harmothoë imbricata, Pholoë minuta, Eulalia viridis, Amphiporus as before.
Ibid.	10	....	....	Nychia cirrosa, Harmothoë imbricata, Evarne impar, Pholoë minuta, Nephthys cæca, N. incisa, Eteone pusilla, E. flava, Cystenides granulata, Thelepus circinnatus, Euchone analis, Amphiporus as before.
Ibid.	30	....	....	Nychia cirrosa, Harmothoë imbricata, Scoloplos armiger, Flabelligera affinis, Cirratulus cirratus, Maldane Sarsi, Nicolea arctica, Amphiporus as before.
Ibid.	35	....	....	Nychia cirrosa, Eunoa Oerstedi, Euchone analis.
Godhavn Harbour, Disco.	5–20	....	....	Nychia cirrosa, N. Amondseni, Harmothoë imbricata, Evarne impar, Pholoë minuta, Nephthys incisa, Phyllodoce Grœnlandica, Eteone flava, Nereis pelagica, Ammotrypane aulogaster, Flabelligera affinis, Axiothea catenata, Cystenides granulata, C. hyperborea, Amphitrite cirrata, Nicolea zostericola, Terebellides Strömi, Sabella pavonia, S. vesiculosa, Spirorbis borealis, Amphiporus as before, Priapulus caudatus.
Outside Godhavn Harbour.	80	....	....	Nychia Amondseni, Harmothoë imbricata, Lumbriconereis fragilis, Owenia filiformis, Cystenides granulata, Terebellides Strömi, Sabella pavonia.
At surface, Ritenbenk Kulbrud, Waigat Strait.	surface.	....	Surface.	Glycera setosa.
H.M.S. 'Alert': No. I.	30	65 00 N., 53 00 W.	R. & G. bottom.	Harmothoë imbricata, Nereis pelagica, Thelepus circinnatus.

Dr. Malmgren's catalogue* shows that the majority of the foregoing

* Annulata Polychæta Spetsbergiæ, Grœnlandiæ, &c. (Helsingfors, 1867). The arctic specimens were collected by O. Torell and Amondsen.

species have been found in the seas of Greenland. The cursory examination of the collection made in the 'Valorous,' however, indicates that there are some not previously well known as inhabitants of these waters. Amongst the latter are *Evarne impar*, Johnst., the fragmentary *Leanira*, *Nephthys incisa*, Mgrn., *Eteone pusilla*, Örst., *Eunice limosa*, Ehlers, the new species of *Aricia*, the two new species allied to *Ammotrypane*, the *Travisia*-form, *Prionospio Steenstrupi*, Mgrn., *Maldane biceps*, Sars, *Maldane Sarsi*, Mgrn., *Praxilla prætermissa*, Mgrn., *Ampharete arctica*, Mgrn., *Nicolea zostericola*, Örst. & Gr., *Grymæa Bairdi*, Mgrn., *Sabella pavonia*, Sars, *S. vesiculosa*, Mont., and those formerly mentioned.

The Annelids from the Gulf of St. Lawrence are not yet fully worked out; but so far as observed the following comparison may be made (it will also be seen that the majority are Norwegian forms):—

I. Annelids common to the Gulf of St. Lawrence and the 'Valorous' Collection. The * indicates a Norwegian form.	II. St. Lawrence forms not present in 'Valorous' Collection. *Norwegian.	III. Norwegian forms in 'Valorous' not indicated in Column I.
*Nychia cirrosa.	*Aphrodita aculeata.	Euphrosyne borealis.
—— Amondseni.	*Lætmonice filicornis.	Harmothoë imbricata.
*Eunoa Oerstedi.	*Lepidonotus squamatus.	Evarne impar.
Eupolynoë occidentalis.	*Lagisca rarispina, var.	Nephthys cæca.
*Pholoë minuta.	Malmgrenia Whiteavesii.	Eulalia viridis.
*Nephthys incisa.	*Antinoë Sarsi.	Eteone pusilla.
*Phyllodoce Grœnlandica.	Nemidia canadensis.	Nereis zonata.
*Nereis pelagica.	—— Lawrencii.	Glycera setosa ?
*Nothria conchylega.	Polynoë Gaspéensis.	Flabelligera affinis.
*Lumbriconereis fragilis.	*Sthenelais limicola.	Cirratulus cirratus.
*Glycera capitata.	*Leanira tatragona.	Ampharete arctica.
*Scoloplos armiger.	*—— hystricis.	Amphitrite cirrata.
*Ammotrypane aulogaster.	*Onuphis sicula.	Nicolea arctica.
*Ophelia limacina.	*Goniada maculata.	—— zostericola.
*Scalibregma inflatum.	Ephesia gracilis.	Leæna abranchiata.
*Trophonia plumosa.	*Eumenia crassa.	Grymæa Bairdi.
*Scolecolepis cirrata.	*Praxilla gracilis.	Trichobranchus glacialis.
*Prionospio Steenstrupi.	*Artacama proboscidea.	Euchone analis.
*Capitella capitata.	*Sabella saxicava ?	Spirorbis borealis.
*Maldane Sarsi.	——	—— spirillum.
*Nicomache lumbricalis.	Balanoglossus.	——
*Axiothea catenata.	Sternaspis.	20
*Owenia filiformis.	——	28
*Cystenides hyperborea.	20	——
*Thelepus circinnatus.		48 'Valorous.'
*Terebellides Strömi.		46 Norwegian.
*Sabella pavonia.		9 St. Lawrence, not Norwegian.
*Chone infundibuliformis.		20 'Valorous' and Norwegian, not St. Lawrence.
——		7 St. Lawrence, not Norwegian or 'Valorous.'
*26		
2 not Norwegian.		
——		
28		
——		
Priapulus.		
Chætoderma.		

HYDROZOA.

By Professor ALLMAN, F.R.S., P.L.S.

Among the Hydroids of the 'Valorous' dredgings which I have as yet examined are many new species, and some which I believe I must place in new generic groups. The curious organism dredged in 1450 fathoms, lat. 56° 11′ N., long. 37° 41′ W., is especially interesting; for though it retains none of its soft parts, I have little doubt of its being a second species of *Stephanoscyphus*, found hitherto only in the Mediterranean, unless some dried specimens from the North Atlantic, which I had formerly received from Mr. Gwyn Jeffreys, should be referable to the same genus, a fact by no means improbable.

There is also a little Campanularian in which I can find no point which will specifically distinguish it from a species obtained at Kerguelen Island by Mr. Eaton, the naturalist of the 'Transit' Expedition to that island. I have not yet completed my examination of all the specimens. The work is necessarily slow where every specimen is to be submitted to microscopic examination, and careful drawing made of such as may turn out to be new.

CORALS.

By Professor DUNCAN, F.R.S., P.G.S.

The Corals sent to me were dredged in 690 fathoms, lat. 56° 1′ N., long. 34° 42′ W. They consist of:—

1. A small *Caryophyllia*; a young individual. The species can hardly be determined, but I believe that it is not a new form.

2. *Flabellum laciniatum*; fragments, showing an unusual scantiness of carbonate of lime.

FORAMINIFERA.

By WILLIAM B. CARPENTER, M.D., LL.D., F.R.S.

The *calcareous-shelled* Foraminifera collected during this cruise did not present any novelty either as regards type or distribution, except that the beautiful deep-sea form of *Orbitolites* which I noticed in the first 'Porcupine' Report (1869, § 36), and which I subsequently met with in the Mediterranean, occurred abundantly in the Baffin's-Bay and North-Atlantic dredgings—attaining the same large size in the far north as it does off the west coast of Ireland and in the Mediterranean, and showing here, as elsewhere, a true spiroloculine centre, which indicates the close relation of its *cyclical* to the *spiral* plan of growth. The *Nodosarine* type is peculiarly conspicuous in the size of individual specimens, especially smooth-shelled *Nodosarians*, as also in the number and beauty of its more delicate and beautifully sculptured forms.

But the *arenaceous* Foraminifera furnish a most interesting addition to those collected in the 'Porcupine' Expeditions, and show, by their extraordinary size and relative abundance, that this type is preeminently

adapted to live on the bottom of deep seas—the most productive haul having been the dredging in 1750 fathoms (No. 9, Davis Strait). This furnished, among other well-known types, abundance of the small nautiloid *Lituola canariensis*, and peculiarly large and regular specimens of the "spirilline" *Trochammina irregularis*, the surface of which is finished off with a smooth "plaster" composed of the finest sand-grains worked up with abundance of ferruginous cement.—I shall now briefly notice the more important novelties obtained in the 'Valorous' dredgings.

Retaining, for the present, the general term *Lituola* for "tests" composed of sand-grains firmly cemented together by phosphate of iron, and imitating more or less closely the forms of calcareous-shelled genera, I may specially notice the following as among its most conspicuous types:—

a. A large "nodosarine" *Lituola*, composed of a series of globular segments arranged in linear succession—the axis of growth being sometimes nearly straight, sometimes regularly curved, sometimes irregular, and the segments having no other connexion with each other than that which is afforded by the small tubular neck which projects from each to be received into the next. The wall of each segment, which is of very uniform thickness, is composed of extremely fine sand-grains, laid together so as to constitute a beautifully smoothed surface both interiorly and exteriorly; and the tubular neck has a perfectly circular orifice, surrounded by a ring of somewhat deeper colour, which indicates that the ferruginous cement is here present in greater quantity. The number and size of the chambers varies in different specimens. The largest I have met with, which is about 0·45 inch in length, has only four chambers—the first having a diameter of about 0·08 inch, the second of 0·10 inch, the third of 0·12 inch, and the fourth of 0·15 inch. Another, whose total length would be about the same if the chambers were arranged in regular linear series, has nine chambers—the diameter of the last or largest chamber being somewhat less than that of the first or smallest in the preceding, whilst that of the first is only about 0·02 inch.

b. Another "nodosarine," composed of segments of an ovoid shape, the small end of each being prolonged into a tubular neck with a circular orifice which is received into the next segment, and the prolonged neck of the last segment forming the mouth. The total length of this usually ranges between 0·4 and 0·7 inch, and the number of segments is commonly from seven to ten; its general form closely resembling that of the calcareous-shelled *Dentalina guttifera*, D'Orb., of which unusually large specimens, attaining a length of 0·45 inch, occur in the same 1750 fathoms dredging. But a series of small and very delicate forms, exactly similar in type, also presents itself in this dredging, their total length ranging downwards from 0·25 to 0·15 inch. And in another series, distinguished by the excess of the breadth over the length of the segments, most perfect forms, composed of excessively minute sand-grains, present as many as

twelve segments in a total length of about 0·12 inch. On the other hand, in the 1450 fathoms dredging (No. 12) are fragments which appear to belong to the same type, except that the small end of each segment is elongated into a tubular neck which intervenes between each segment and its successor, so that the whole test would present the aspect of a tube with egg-shaped enlargements at intervals. The length of a single flask-shaped segment with its neck is sometimes as much as 0·16 inch.

c. Another series, which I may distinguish as the "orthocerine," consists of arenaceous tubes, sometimes attaining a length of 0·8 or even 0·9 inch, having an average diameter of about 0·1 inch; they usually vary but little in diameter from one end to the other, but are slightly constricted at somewhat irregular intervals, so as to show an imperfect division into about ten or twelve segments. When perfect, these tubes are usually closed and rounded at one end, which encloses a globose or ovoid chamber, commonly marked off from the rest either by an external constriction or by an internal thickening of the wall. At the other extremity the tube, which is there often somewhat conical, has a circular mouth; but the finish of this mouth, as of the entire tube, is not nearly so perfect as in the types already described. The incomplete segmentation of each tube pretty obviously marks successive additions to its length; and these additions are far less uniform in length than they are in transverse diameter; so that while the latter is pretty constant throughout, the length of a segment may be much less than its breadth, or may be as much as twice as great.—The special interest of this orthocerine test, therefore, consists in its combination of inconstancy of detail with great constancy of general form and proportion, and in the transitional stage it presents between the monothalamous and the polythalamous types. For the whole cavity may in one sense be said to consist of but a single chamber; whilst in another it may be said to be composed of a series of freely communicating chambers. And the component sand-grains are much less firmly cemented together than they are in the preceding types—some specimens approaching, in the looseness of their aggregation, the *Astrorhizæ* and the large polythalamous orthocerine *Lituolæ* of the 'Lightning' collection*, as also the monothalamous *Lituolæ* collected in the 'Porcupine'†, whose claim to relationship to the preceding mainly consists in the possession of a slightly projecting circular mouth, along the border of which the sand-grains are united by the ferruginous cement which is almost entirely wanting in the "test" generally.

The sarcodic contents of these *Lituolæ* have the dark olive-green hue which I have previously noticed as prevalent among the large arenaceous deep-sea Foraminifera. But it is a curious circumstance that many of the "orthocerine" tubes were found to be occupied by a large parasitic

* Proceedings of the Royal Society, vol. xvii. p. 172, § 13.

† *Op. cit.* vol. xviii. p. 443. § 76.

Protozoon of the *Gregarina* type. The most careful examination has failed to detect in this any higher organization than that of ordinary *Gregarinæ*; and its parasitic character may be inferred from the fact that I often found it coexisting in the same tube with the ordinary sarcodic body of the *Lituolæ*, which was then more or less reduced in bulk, indicating that the latter had been partially preyed on by the former. Moreover, I found a precisely similar body coiled up in the midst of a mass of sand that occupied the interior of a large detached and partly broken spherical segment of a "nodosarine."

d. Another series of "orthocerine" *Lituolæ* is of great interest as conducting us towards what seems at first sight an entirely distinct type, the *Rhabdammina* of Sars. These are straight tubular cylindrical rods, nearly uniform in diameter, and distinguished from the preceding by their extreme slenderness. Some of them are nearly an inch in length, while their diameter never exceeds 0·03 inch, being often not half that amount. The larger of these specimens presented themselves in great abundance in the 410 fathoms dredging (No. 6, Davis Strait); the smaller and less numerous examples in the 1750 fathoms dredging (No. 9). Sometimes they are nearly uniform in size from end to end; in other cases there are constrictions at irregular intervals, forming an imperfect segmentation; while sometimes the tube narrows for part of its length, and then enlarges again. The sand-grains, usually rather coarse, of which these tubes are composed are very firmly united by ferruginous cement; and the non-segmented rods bear so exact a resemblance to those which form the extensions of the triradiate *Rhabdammina* (Sars), that they might easily be supposed to be detached arms of that very curious form*. In fact, when we met with similar rods in dredgings containing also large numbers of *Rhabdamminæ*, this was the light in which my colleague was accustomed to regard them. A careful examination of the varietal forms of *Rhabdammina*, however, had led me to the conclusion that the typical triradiate form might graduate into a single long rod; for when, as often happens, one of the three rays is imperfectly developed, the others are not only longer than usual, but diverge at an angle greater than 120°, this divergence increasing in proportion to the suppression of a third ray, until it reaches 170°, so that the two rays come so nearly into the same straight line, that a single very long straight rod may be considered as the representative of two of the three rays of the typical triradiate *Rhabdammina*. And I used to enforce this view by a comparison of the large number of single straight rods which often came up in one dredging, with the small number of triradiate *centra* from which they could be supposed to be detached—the former being often *six* or *eight* times as numerous as the other, whilst they ought not to have exceeded *three* times. Now in the two 'Valorous' dredgings which furnished these straight rods in the greatest abundance

* Proceedings of the Royal Society, xvii. p. 172.

only two or three triradiate *Rhabdammina* centra presented themselves; so that I feel justified in concluding that the long straight rods are not rays of *Rhabdammina* broken off from their centra, but that each represents two elongated and straightened-out arms of a *Rhabdammina* whose third arm is abortive. The gradation towards the orthocerine *Lituolæ*, marked by their occasional imperfect segmentation, is only another case of that general doctrine of intimate mutual relationship which I have on several occasions pointed out as existing in each of the two groups of *perforate* and *imperforate* Calcareous-shelled Foraminifera, and which, when all the new deep-sea *Arenaceous* types shall have been carefully worked out, will be found, I feel confident, to be completely applicable to that third series which is now coming to rival the other two in the variety of its forms.

What is the relation between the Arenaceous and the Calcareous-shelled *Foraminifera* (whether the former are the elder, and the latter the derived forms), is a question on which I shall at present only suggest, reserving what I have to say upon it to some future opportunity.

Diatoms.

By Professor George Dickie, F.L.S.

Mr. Gwyn Jeffreys, during the voyage of the 'Valorous,' collected by means of the towing-net, in lat. 58° 55′ N., long. 34° 18′ W., a peculiar organism having the appearance of a small sponge. It was found to have a very wide range, extending over some thousands of square miles.

The general aspect of a specimen preserved in spirit is such that it might be readily mistaken for a sponge.

Specimens were submitted to Dr. Bowerbank and Mr. Carter. They both reported it not to have the character of a sponge. The latter was more specific in his opinion, and pronounced it be a Diatom, probably a *Synedra*. Before receiving Mr. Carter's report I had arrived at the same conclusion.

The organism is a new species of the genus *Synedra*, and remarkable on account of the large proportion of colloid matter which seems to connect the frustules in masses. The former may be compared to the sarcode, the latter to the spicules of a sponge; but there is mere resemblance only. It is further notable on account of the great length of the frustules as contrasted with their breadth.

The countless multitudes of this Diatom and of others of the same family, and the extent of sea over which they extend, are points of much interest, contributing directly as they do to the support of various smaller marine animals, and these in turn to larger forms, adding also to deposits taking place at various depths. I add a brief description of *Synedra Jeffreysi*:—Frustules greatly elongated, straight, in front view linear, ends subcapitate, no pseudo-nodule, in side view linear rectangular,

striæ marginal. The total length varies from a ninth to a tenth of an inch, and the front view has a diameter about $\frac{1}{4000}$, the side view about $\frac{1}{2500}$ of an inch. The striæ are 40 to 50 in a thousandth of an inch.

From Mr. Jeffreys I also received specimens of an organism found by Mr. Hart, the naturalist of the 'Discovery,' drifting on the surface in lat. 59° 36′ N., long. 39° 10′ W. The same was also found by Mr. Jeffreys in 59° 16′ N., long. 37° 16′ W. It consists of dense fleece-like tufts of a species of *Podosphenia*. It agrees very well with the characters of *P. elongata*, Ktz. The latter species is not uncommon on European coasts growing upon various Algæ. The specimens collected by Mr. Hart are attached to fragments of Algæ, in one case, apparently, to a piece of a species of *Fucus*, and therefore probably drifted from some coast-line. I have compared Mr. Hart's specimens with a Scotch example of *P. elongata*, and I find they agree. The frustules in the latter vary in length and breadth; specimens from the North Atlantic have, for the most part, shorter and broader frustules.

Lat. 59° 10′ N., long. 50° 25′ W., 1750 fathoms.

One Diatom only, viz. *Coscinodiscus radiatus*, Ehb.

Also various Polycystina, Foraminifera, and spicules of Sponges.

Surface-drift. Lat. 59° 36′ N., long. 39° 10′ W.

A tufted Diatom, viz. *Podosphenia elongata*, Ktz.

Station No. 13. Lat. 55° 58′ N., long. 31° 41′ W., 1230 fathoms. Fine adhesive mud, bluish when moist, dirty white when dry.

Diatomaceæ: *Coscinodiscus radiatus*, Ehrb.; *C. lineatus*, Ehrb.; *C. minor*, Ehrb.; *Amphora granulata*, Greg.; *Synedra Jeffreysi*, n. sp.

Along with these, two Polycystina (*Dictyocha fibula*, Ehrb., *D. gracilis*, Ehrb.), with fragments of a Rotalia and sponge-spicules.

Twenty grains of this mud, partly soluble with effervescence in acid, left a residue which, when washed and dried, weighed 14 grains.

Lat. 55° 38′ N., long. 28° 42′ W., 1485 fathoms. A fine dirty-white mud.

Diatomaceæ: *Coscinodiscus radiatus*, Ehrb.; *C. minor*, Ehrb.

Also fine siliceous particles, fragments of sponge-spicules, and numerous Coccoliths.

Twenty grains, partly soluble with effervescence in acid, left a residue which, after being washed and dried, weighed 12 grains.

Lat. 62° 6′ N., long. 55° 56′ W., 1350 fathoms. A grey mud, very adhesive.

Diatomaceæ: *Coscinodiscus radiatus*, Ehrb.; *C. minor*, Ehrb. Foraminifera: *Rotalia globosa*.

Also fragments of sponge-spicules and numerous Coccoliths.

Twenty grains, partly soluble with effervescence in acid, when washed and dried, left a residue weighing 15 grains.

Holsteinborg Harbour: 10 fathoms. Chiefly a blackish sand.

The washings yielded the following Diatoms:—*Coscinodiscus oculus iridis*, Ehrb.; *Amphora proteus*, Greg.; *A. Leighsmithiana*, O'Meara; *A. lanceolata*, Cleve; *Navicula didyma*, Ehrb.; *N. ovalis*, Sm.; *Nitzschia distans*, Greg.; *Grammatophora marina*, Ktz.

From H.M.S. 'Alert.'

Station No. 1. Lat. 65° N., long. 53° W., 30 fathoms.

Diatomaceæ:—*Grammatophora marina*, Ktz.; *Rhabdonema arcuatum*, Ktz.; *Navicula Archeriana*, O'Meara; *N. elliptica*, Sm.; *N. pinnularia*, Cleve; *Coscinodiscus subtilis*, Ehb.; *C. radiatus*, Ehb.; *C. oculus iridis*, Ehb.; *Cocconeis glacialis*, Cleve; *Amphora cymbifera*, Greg.

Catalogue of Pebbles and Minerals dredged by Mr. Gwyn Jeffreys.

28 Jermyn Street, Nov. 8, 1875.

1. 690 fathoms. Amygdaloidal porous decomposing igneous rock, light and spongy, owing to decomposition. *Probably from Iceland.*
2. 690 fathoms. Station 12. Admiralty, No. 9. Lat. 56° 1′ N., long. 34° 42′ W. Aug. 20, 1875. Felstone and quartzite, altered basalt.
3. 410 fathoms. Small pebbles of gneiss and quartzite from No. 5. Aug. 10, 1875.
4. 1450 fathoms. Fine-grained quartzite (many nodules); gneiss; argillaceous limestone (Silurian?); fine-grained sandstone; hornblendic rock, much quartz; fine-grained felstone; pale buff sandstone; hornblende and quartz; amygdaloidal greenstone (vesicular); fine-grained siliceous rock; steel-grey felspathic rock, fine-grained; felstone, grey, close-grained. Rock specimens all rounded and water-worn. *Most likely derived from Iceland.*
5. Minerals same depth, marked 1450 fathoms, No. 11. Aug. 19, 1875. Black mica; obsidian; quartz (crystallized); flint; basaltic rock in same series.
6. 1750 fathoms. Lat. 59° 10′ N., long. 50° 25′ W. Quartz pebbles; ashy matter, igneous; vesicular rock (decomposed amygdaloidal trap?). *Probably Iceland.*
7. 1785 fathoms. Lat. 55° 10′ N., long. 25° 58′ W. Decomposing argillaceous limestone.

All the pebbles associated with the *Balani* from the shore are gneiss; much white quartz in the matrix.

R. Etheridge.

Soundings and Dredgings obtained on Passage from Disco to England.

In Davis Strait.

No. of Station.	Lat.		Long.		Depth in fms.	Nature of bottom.	Bottom-temp.	Remarks.
	N.		W.					
1.	70°	30′	54°	41′	175	Sand, mud.	....	Dredging.
2.	70	27	55	0	85	Gravel, stones.	....	Ditto.
3.	69	31	56	1	100	Mud.	....	Ditto.
4.	67	56	55	27	20	Broken barnacles and shells.	....	Ditto.
5.	66	55	55	30	57	Rock, sand, shells.	....	Ditto.
6.	64	5	56	47	410	Sand, mud.	34°·6	Serial temperature, dredging.
7.	63	9	56	43	1100	Clay, mud.	36·4	Ditto ditto.
8.	62	6	55	56	1350	Mud (blue clay under).	34·6	
9.	59	10	50	25	1750	Ditto.	34·0	Dredging.
In North Atlantic.								
10.	58	14	46	29	1660	Fine sand.	34·3	Serial temperature.
11.	57	50	44	52	1860	Globigerina ooze.	33·4	
12.	56	11	37	41	1450	Glob.-ooze, stone.	36·3	Serial temperature, dredging.
13.	56	1	34	42	690	Glob.-ooze.	38·2	Dredging.
14.	55	58	31	41	1230	Mud.	36·8	
15.	55	58	28	42	1485	Clay, blue mud.	36·5	Serial temperature.
16.	55	10	25	58	1785	Glob.-ooze (blue mud under).	36·7	Dredging.

N.B. In the accompanying Chart (Plate 2) the outward course of the voyage is shown by a plain line, and the homeward or return course by a dotted line.

XXV. "Report on the Physical Investigations carried on by P. Herbert Carpenter, B.A., in H.M.S. 'Valorous' during her Return Voyage from Disco Island in August 1875." By William B. Carpenter, C.B., M.D., F.R.S. Received June 15, 1876.

Introduction.

The despatch of H.M.S. 'Valorous' to Disco Island, in the summer of 1875, with stores for the use of the Arctic Discovery Ships, having afforded an opportunity for the prosecution of a Deep-Sea Physical and Biological Exploration of the North Atlantic and Baffin's Bay, which should be complementary to the work elsewhere carried on by the 'Challenger,' a suggestion for the prosecution of this inquiry on the return voyage of the 'Valorous' was made by the Council of the Royal

Society, and approved by the Admiralty. Dr. Gwyn Jeffreys having undertaken the general charge of the work, named as his assistant my Son, Mr. P. Herbert Carpenter, who had accompanied me in the 'Lightning' and 'Porcupine' Expeditions; and it was arranged that, while aiding Dr. Jeffreys in the Biological work, he should take special charge of the Physical.

A grant from the Donation Fund having been made to Dr. Jeffreys and myself for the expenses of the inquiry, I endeavoured to make such provision for the conduct of the Physical observations as should render them capable of accurate correlation with those of the 'Challenger.' With this view, I obtained two water-bottles, on the construction devised by Mr. Buchanan (the Physicist of the 'Challenger'), from Messrs. Milne, of Edinburgh, by whom they had been supplied to him; and also two Hydrometers, on Mr. Buchanan's construction, from Messrs. Kemp, who had previously made them under Mr. B.'s direction.

Unfortunately, however, my Son was unable to utilize the water-bottles thus provided: for as Captain Nares had not been supplied with any water-bottle, it was deemed right to comply with his pressing request that one of these bottles should be transferred to him; and the other bottle failed in its work, in consequence of some defect in its construction which the armourer of the 'Valorous' was unable to remedy*. No Specific-Gravity observations could be made, therefore, on any but surface-water; these, however, were very systematically carried out; and Mr. Buchanan has kindly undertaken to compare our Hydrometers with his own, and to furnish the formula for the exact correction of the 'Valorous' observations, so that results may be worked out which shall be strictly comparable with those obtained in the 'Challenger' Expedition.

Having been supplied by my Son with the entire series of Deep-Sea Temperature-observations, the results of which have been embodied in Sections prepared at the Hydrographic Office of the Admiralty, I now present a Report upon these, in which I have drawn attention to what seem to me their chief features of interest.

Report.

In the first of the Serial Soundings taken by the 'Valorous' (see Plate 2. Station 6, and Plate 3. No. VI.), nearly in the middle of Davis Strait and on the parallel of Godthaab, the bottom-temperature, at a depth of 410 fathoms, was 34°·6 Fahr.; and the descent to this from a surface-temperature of 40° was nearly uniform—39°, 38°, 37°, 36°, and 35° being met with at almost equal intervals. There was here, therefore, no indication of any contrary movement of different strata of water, or of any special superheating of the superficial stratum. But the

* Mr. Buchanan tells me that the like defect existed in all the water-bottles supplied to the 'Challenger' by Messrs. Milne, the construction of which he had not personally superintended.

case was very different with the next much deeper sounding (Plate 2. Station 7), which was taken about a degree further south, but still towards the middle of Davis Strait: for there was here (Plate 3. No. VII.) a surface-stratum of 45°, but of such extremely small thickness, that the isotherm of 40° was reached in about 15 fathoms; from 40° to 38° the interval was nearly the same as in the previous sounding; but below 38° the descent was so slow that 37° was not reached until nearly 800 fathoms, and on the bottom at 1100 fathoms the temperature was still 36°·4. At the next station (Plate 2. Station 8), Lat. 62° 6′ N., Long. 55° 56′ W. (that is, another degree further south, and at about the same distance from the Greenland coast), a depth of 1350 fathoms was met with; the surface-temperature was still 45°; but the bottom-temperature was found to be 34°·6, as in the 410 fathoms sounding. The next temperature-sounding (Plate 2. Station 9, and Plate 3. No. IX.) was taken nearly 3 degrees further south and $5\frac{1}{2}$ degrees to the west, namely in Lat. 59° 10′ N., Long. 50° 25′ W.; that is, a little to the south of Cape Farewell, but still six degrees to the west of it: here the surface-temperature was still 45°; but the bottom-temperature at 1750 fathoms had sunk to 33°·4. Finally, a set of serial soundings (Plate 2. Station 10, Plate 4. No. X.) was taken before rounding Cape Farewell, about a degree further south and 4 degrees east: the surface-temperature had then risen to 49°; but the isotherm of 40° was reached at about 50 fathoms, that of 39° at about 90 fathoms, and that of 38° at about 160 fathoms; whilst below this the descent of the thermometers was extremely slow down to the isotherm of 37°, which lay at about 1050 fathoms—becoming more rapid, however, beneath this, so that 36° was reached at about 1400 fathoms, 35° at about 1500, and 34°·3 on the bottom at 1660 fathoms.

Now these phenomena seem to me to point very distinctly to the existence (1) of a superheated layer, which is slowly moving up Davis Strait, and gradually losing its excess of temperature as it proceeds north, as shown by the gradual approach of the isotherms to the surface; (2) of a neutral intermediate layer, 1000 fathoms or more in thickness, marked out by the extreme uniformity of its temperature, which indicates its stationary condition; and (3) of a deep cold layer, which as clearly derives its low temperature from a northern source, as the uppermost stratum does from a southern, and which must, therefore, be in movement.

The Temperatures at Station VI. (Plate 3.) seem at first sight rather anomalous when compared with those of Stations VII.–X.—the isotherm of 37° here coming up within 200 fathoms of the surface, whilst at only a degree further south it lies at nearly 800 fathoms; and a bottom-temperature of 34°·6 being found at 410 fathoms at Station VI., whilst at Station VIII. it is only reached at 1350 fathoms. But the anomaly disappears when the rapidly increasing depth and the tendency of the coldest water to gravitate to the bottom are taken into account: for

it appears, from the temperature-soundings taken further north towards Disco Island by the Swedish ship 'Ingegera' (Plate 3. Nos. I.–V.), that water as cold as this, and even much colder (31° being recorded in one instance), is there found at depths varying between 58 and 185 fathoms; and it can scarcely be doubted that the water which is chilled by the more severe cold of Baffin's Bay is here flowing down the slope of Davis Strait. Again, it is at first sight an anomaly to find at Station VIII. a bottom-temperature of 34°·6 at 1350 fathoms, while the bottom-temperatures both to the north and to the south of it are 34°·6; but this only shows that the coldest Polar water is flowing south through some deeper channel, perhaps in the western half of Davis Strait*. And the same explanation applies to the yet more remarkable fact that a bottom-temperature of 33°·4 was met with near the mouth of Davis Strait, when no such water was met with further north. But that even this does not carry down the coldest water of the Arctic basin, is obvious from the fact brought to light by the 'Porcupine' temperature-soundings in the "Lightning Channel" (between the north of Scotland and the Färöe Islands), over a large part of whose bottom we found the temperature to range two degrees, or even more, *below* 32°.

The next temperature-sounding (Plate 2. Station 11, Plate 4. No. XI.), taken on the 17th of August almost exactly in the meridian of Cape Farewell, and not quite two degrees to the south of it, gave, like No. IX., a bottom-temperature of 33°·4 at 1860 fathoms; so that it seemed pretty clear that this is the temperature of the coldest water that can find its way into the North Atlantic along either the west or the east coast of Greenland. And from the depth at which the isotherm 35° was found to lie in the 1660 fathoms serial sounding, it is obvious that the stratum between 35° and 33°·4 must be here a very thin one; whilst the upward slope which is indicated by the next sounding shows that it must rapidly die out towards the east.

The course of the 'Valorous' having then been kept at first nearly due East, and afterwards S.E., another serial temperature-sounding (Plate 2. Station 12, Plate 4. No. XII.) was taken on the 19th of August in Lat. 56° 11′ N., and Long. 37° 41′ W. The surface-temperature had here risen to 53°,—about the same as we had encountered in the "Lightning Channel," at the same time of the year, rather further to the north; but the warm upper stratum was here thinner, a reduction

* As I pointed out on a former occasion (Proc. Roy. Soc. vol. xx. p. 624. § 144), any water moving from either Pole towards the Equator will have a *westerly* tendency in virtue of its *deficiency* of easterly momentum; just as water moving from the Equator towards either pole will have an *easterly* set, in virtue of the *excess* of easterly momentum which it carries with it.—The later temperature-soundings of the 'Challenger' in the South Atlantic have given the explanation of the temperature of 32°·4 observed under the Equator in the first year of her voyage, but not encountered in any of the earlier temperature-soundings taken in the South Atlantic, by showing that the coldest Antarctic underflow is met with on the *westerly* part of its sea-bed.

to 45° taking place within 50 fathoms, and to 40° within 300; whereas in Lat. 59° 35′ N., Long. 9° 11′ W. we had found the isotherm of 45° lying below 500 fathoms, while the bottom at 767 fathoms was still 41°·4. It is obvious moreover, from the regularity of the descent of the isotherm of 40° in this part of the North Atlantic, that *easting* has more influence on the rate of that descent than *southing*—thus confirming the view formerly expressed as to the tendency of the warm upper flow towards the *eastern* side of the basin*. The isotherms of 39° and 38° slope downwards towards the east at about the same rate; but those of 37° and 36° still nearly keep their parallelism to the surface, confirming the previous suggestion of the "neutrality" of the deep stratum which they underlie.

Between the last station and the next (Plate 2. Station 13), taken in Lat. 56° 1′ N. and Long. 34° 42′ W., in the line of the channel between Iceland and Greenland, but considerably to the south of it, the sea-bed was found to have shallowed most remarkably (Plate 4. No. XIII.), bottom being struck at 690 fathoms, and the bottom-temperature rising again to 38°·2. This elevation may be regarded with great probability as a continuation of that which was encountered by Sir L. McClintock in the line of temperature-soundings which he took several years ago across the North Atlantic between Rockall and Cape Farewell; for almost exactly in a line between the 'Valorous' Station 13 and Iceland, Sir L. McClintock met with bottom at 743 fathoms, between 1260 fathoms on the east and 1159 fathoms on the west.

The course being now again kept nearly due east, another temperature-sounding (Plate 2. Station 14, Plate 4. No. XIV.) was obtained in Lat. 55° 58′ N., Long. 31° 41′ W., which, on a bottom of 1230 fathoms, gave a bottom-temperature of 36°·8, the surface-temperature being 54°·5. Three degrees further east, and on the same parallel (Pl. 2. Station 15), another set of serial temperatures was taken (Plate 4. No. XV.) which indicated a further increase in the upper warm stratum, the isotherm of 40° descending to about 380 fathoms; but the depths of the isotherms of 39°, 38°, and 37° show little change; and the bottom at 1485 fathoms was 36°·5, as at the corresponding depth on the other side of the ridge. Still further to the east (Plate 2. Station 16, Plate 4. No. XVI.), in Lat. 55° 10′ N., Long. 25° 58′ W., the depth was found to have still further increased to 1785 fathoms; but the bottom showed no lower a temperature than 36°·7, although in the 1750 fathoms sounding on the other side of the ridge the thermometer fell to more than *three degrees lower*.

Bad weather having come on, it was not considered prudent, in the disabled condition of the ship, to attempt further scientific explorations; and the course was accordingly shaped for Cork.

The Temperature-Section prepared from the serial soundings taken in the 'Valorous' after quitting Davis Strait has been continued towards Va-

* Shearwater Scientific Researches, 1872, §§ 144, 148 (Proc. Roy. Soc. xx. pp. 624, 626).

lentia (Plate 4. No. XVII.) on the basis of the serial soundings taken off the coast of Ireland in the first cruise of the 'Porcupine' in 1867, a sounding (No. 22) in 1263 fathoms, Lat. 56° 8′ N., Long. 13° 34′ W., being taken as the principal guide. This being almost on the same parallel with the last serial sounding of the 'Valorous' (the difference of latitude being only half a degree), and the seasonal difference being rather in favour of the 'Valorous' temperatures, it is extremely striking to find in this Section the most remarkable contrast yet brought out between the thermal condition of the eastern and the western sides of the North Atlantic: for the descent of all the isotherms as they pass from west to east, which has been already pointed out in the 'Valorous' portion of the section, continues at an even more rapid rate; so that the isotherm of 40°, which lay at Station XVI. at 380 fathoms, lies at 900 fathoms at Station XVII., 15 degrees to the west; whilst the isotherm of 45°, which at the first of these stations lay at 80 fathoms from the surface, lay in the second at 640 fathoms. This difference in the thickness of the whole stratum above the isotherm of 40° is much more remarkable than the difference of surface-temperature, the increase of which between the first and the second station was only from 55° to 59°·6.

It is clear, therefore, that the heating power of the warm flow which comes up from the S.W. towards the western shores of the British Isles, and which proceeds onwards to the N.E., so as to ameliorate the climate of the Orkneys and Shetland Islands, but still more markedly to affect that of the coast of Norway (as has been shown by Prof. Mohn), depends upon *its great depth*. Any such superheated film as the Gulf-stream has been found to be when last recognizable as a current (as was long since urged by Mr. Findlay, and has since been confirmed by Capt. Chimmo's observations) must lose its excess of warmth long before it reaches our shores. Hence, as I have urged on a former occasion (Proc. Roy. Soc. vol. xx. pp. 621, 637. §§ 137, 163), the prolonged heating power of the N.E. flow depends much more upon the thickness of its moderately warm stratum than upon its bringing with it a high surface-temperature. A layer of 50 fathoms at 60°, flowing N.E. over a bed of ocean-water at 40°, and exposed above to an atmosphere of 40°, would be cooled down to that standard in two or three weeks. But a layer of 900 fathoms thickness, ranging from 40° to 55°, would retain an excess of temperature far longer.

The advocates of the doctrine that the *vis a tergo* is the Gulf-stream, which cannot be traced as a current by any distinctive feature further to the N.E. than the parallel of 40° and the meridian of 30°, have to show in what way it can raise the temperature of so thick a stratum of ocean-water as we have seen to be affected in the Western portion of the North Atlantic by a warm flow of some kind. Whether, as Prof. Wyville Thomson maintains, the approximation of its boundaries between the British Islands on one side and Labrador and Greenland on the other

can possibly produce this result, is a point on which it is for Hydrographers to decide. For myself, I cannot regard it as probable that a spent stream of 50 fathoms thickness can give motion to a vast layer of 900 fathoms depth.

On the other hand, the doctrine I advocate that a thick upper stratum of the North Atlantic is slowly moving Polewards, to fill up the void left by the gravitation-underflow of the coldest water towards the equator, and that this stratum will also have an Easterly tendency in virtue of the excess of easterly momentum which it brings with it from a lower latitude, seems adequately to account for the facts now brought to light. The progressive closing in of the boundaries of this poleward upper flow will obviously tend to deepen it, so as to give it a more persistent heating power*. In the South Atlantic and Southern Indian Oceans, on the other hand, the progressive opening-out of the ocean-boundaries, as we pass Southwards from the Equator, will tend in the same measure to reduce the thickness of the Poleward upper flow, thus diminishing the persistence of its heating power. And in this, as it seems to me, we have the true explanation of the marked difference between the climate of Kerguelen's Land (Lat. 50° S.), for example, or that of Heard Island (Lat. 53° S.), and that of Ireland (lying between the parallels of $51\frac{1}{2}°$ and $54\frac{1}{2}°$ N. Lat.), the summer temperature of the former being but little above the winter temperature of the latter.

The 'Challenger' temperature-sections have most conclusively shown that the entire warm upper stratum in the South Atlantic is very much thinner than that of the North Atlantic; and while I fully admit that a part of this difference is due to the fact that a far larger portion of the Equatorial current is deflected into the latter than into the former, I cannot see that the Gulf-stream by any means accounts for the descent of the isotherm of 40° in Lat. 56° N. to a depth of 900 fathoms.

The 'Valorous' temperature-soundings seem to me to be of peculiar interest and value, in furnishing a satisfactory explanation of the comparatively high bottom-temperature of the North Atlantic. I have always attributed this to the comparative narrowness of the channels of communication between the Arctic and the North-Atlantic basins, which restrict the flow of the coldest Polar water from the former into the latter; and long before the 'Challenger' Expedition sailed, I had ventured the prediction that the South Atlantic, on account of the perfect

* This position may seem inconsistent with the objection just taken to the doctrine of Sir Wyville Thomson. But the inconsistency is only apparent. I cannot conceive that after the Florida Current has spread itself out like a fan over the Mid-Atlantic, it can retain enough *vis a tergo* to give a N.E. movement to a mass of water nearly 2000 miles wide and 700 or 800 fathoms deep, the impelling force being progressively weakened by the obstacles to that movement. On the other hand, the force which (on the doctrine of a Thermal circulation) acts as a *vis a fronte*, grows stronger as the water which it puts in motion approaches the Polar area, and thus is fully competent to deepen the poleward stratum in proportion to the reduction of its breadth.

freedom of its communication with the Antarctic, would have a colder bottom, and that the influence of the Antarctic underflow would probably extend to the north of the Equator. By Sir Wyville Thomson, on the other hand, it was argued from the commencement that the whole cooling of the deep stratum of the *North* Atlantic is due to the *Antarctic* underflow; and this conviction he repeats in his last utterance on the subject, on the ground of the continuity of the isotherms from the South into the North Atlantic*. The question arises, however, why the deep stratum of the North Pacific, which is undoubtedly fed from the Antarctic, should be so decidedly colder, as the 'Challenger' and 'Tuscarora' soundings show it to be, than the deep stratum of the North Atlantic; and this question appears to me to find an entirely satisfactory answer in the indication furnished by the Second Section (Plate 4), that the Arctic Basin is for the most part separated from that of the North Atlantic by an intervening ridge, which (like many similar ridges discovered by the 'Challenger') allows water of about 36°, but *not colder water*, to pass from the former into the latter. The limited contributions of colder water furnished by Baffin's Bay and the "Lightning Channel" would help to reduce the deep temperature of the North Atlantic generally to the 35°–36° shown in the 'Challenger' Sections; but it is only when, on approaching the Equator, a bottom-temperature below this first shows itself, that I can recognize the influence of the Antarctic underflow.

I forbear, however, to discuss this subject more fully at present, the Admiralty not having yet published the final instalment of the 'Challenger' temperature-sections. And I shall confine myself to an expression of my earnest hope that the ship to be sent next year to communicate with the Arctic Expedition may have, as part of its work, the completion of that which the 'Valorous' was disabled from performing—namely, the obtaining a continuous temperature-section between Iceland and Greenland, and another across Davis Strait.

DESCRIPTION OF THE PLATES.

Plate 2.

Chart showing the track of the 'Valorous' outward and homeward. The latter is the dotted line. The tints represent different depths.

Plate 3.

Soundings I.–IX. and isotherms between Disco and Davis Strait.

Plate 4.

Soundings X.–XVII. and isotherms between Davis Strait and England.

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* Proceedings of the Royal Society, vol. xxiv. p. 632.

"On some Thallophytes parasitic within recent Madreporaria." By P. MARTIN DUNCAN, M.B. Lond., President of the Geological Society, F.R.S., &c. Received March 17, 1876 *.

[PLATES 5, 6, 7.]

CONTENTS.

I. *A Notice of the Discoveries of previous Observers.*

During some investigations into the nature of the sclerenchyma of the Stony Corals (Madreporaria) I was impressed with the importance of examining the method of entry, growth, and distribution of the minute unicellular parasite which so constantly penetrates and ramifies in minute tubular excavations in the solid structures.

The interest of the subject struck Quekett in 1851; for he described the minute tubules of a parasite in his Lectures, which he subsequently published in 1854. He wrote as follows:—"Confervoid growths are also very frequently met with in the skeleton of corals, as all those bodies possess animal matter, which, decomposing after death, becomes a nidus for the development of Confervæ; and hardly a section can be examined without exhibiting such an appearance as shown in fig. 78"†. This figure exhibits almost straight canals, of great tenuity and of different lengths, cutting across the normal structures at different angles.

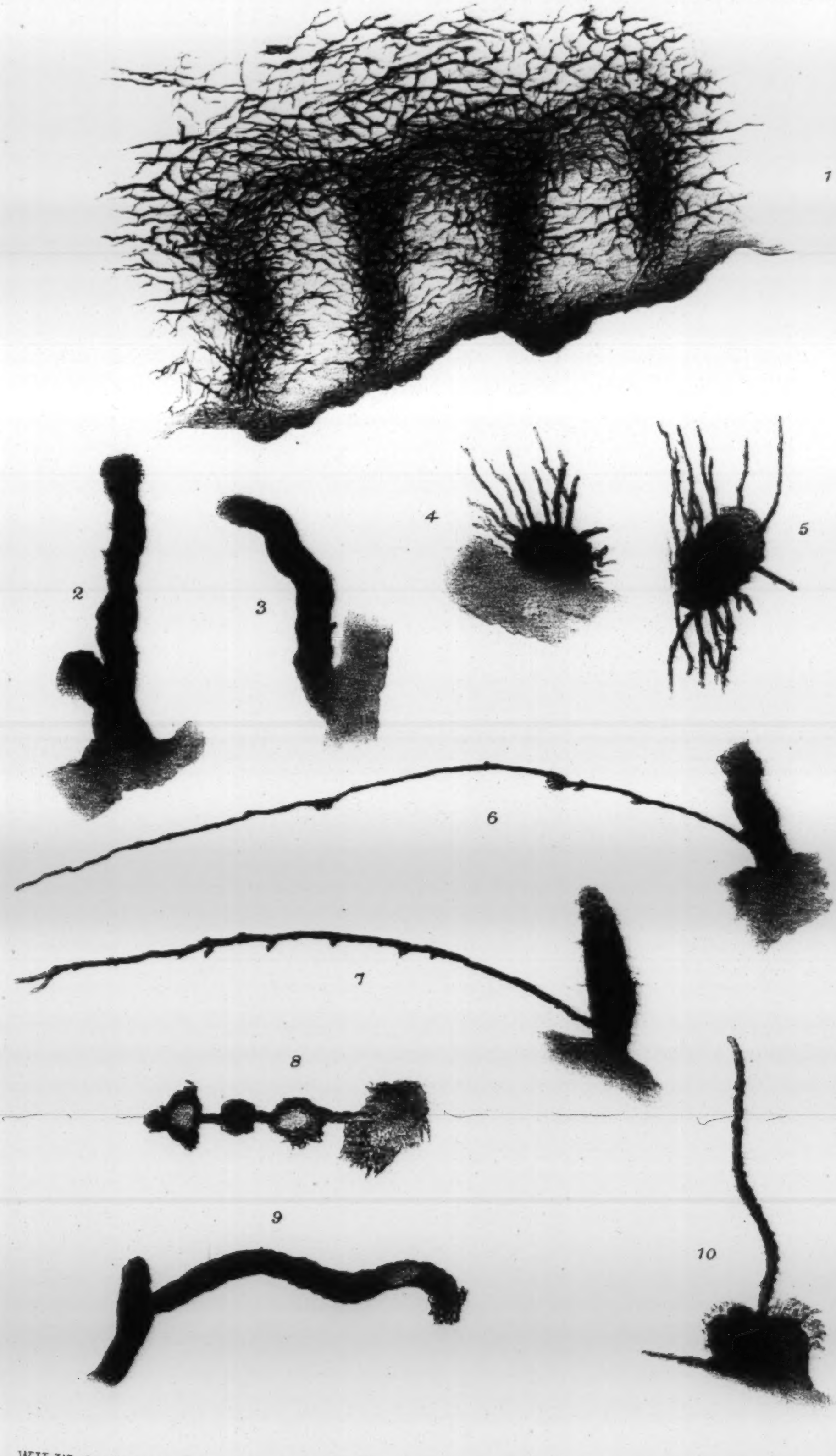
Before this time, Carpenter‡ and Bowerbank§ had shown that molluscous shells contained tubular structures; and Quekett, in his work, agreed with them as regards their facts; but he believed (as, indeed, Carpenter did also at that time) that, whilst some of the tubules

* Read May 11, 1876. See *antè*, p. 17.

† John Quekett, 'Lectures on Histology,' vol. ii. p. 153 (1854).

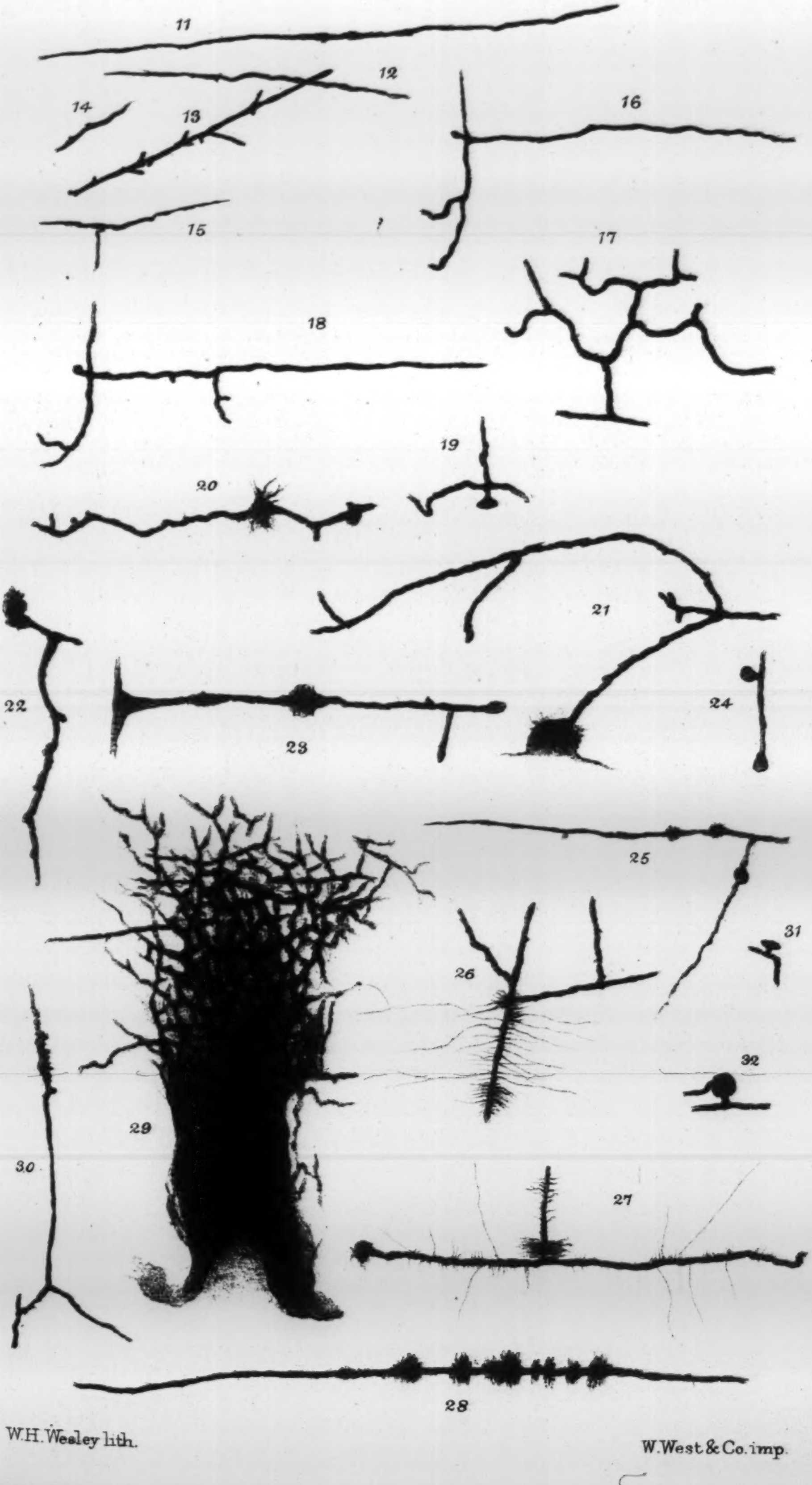
‡ Carpenter, in Todd's 'Cyclopædia of Anatomy and Physiology,' article "Shell," vol. iv. p. 562 (1852); Ann. & Mag. Nat. Hist. Dec. 1843; Report Brit. Assoc. 1844, 1847.

§ Bowerbank, Trans. Microsc. Soc. i. (1844).



W.H. Wesley lith.

W. West & Co. imp.



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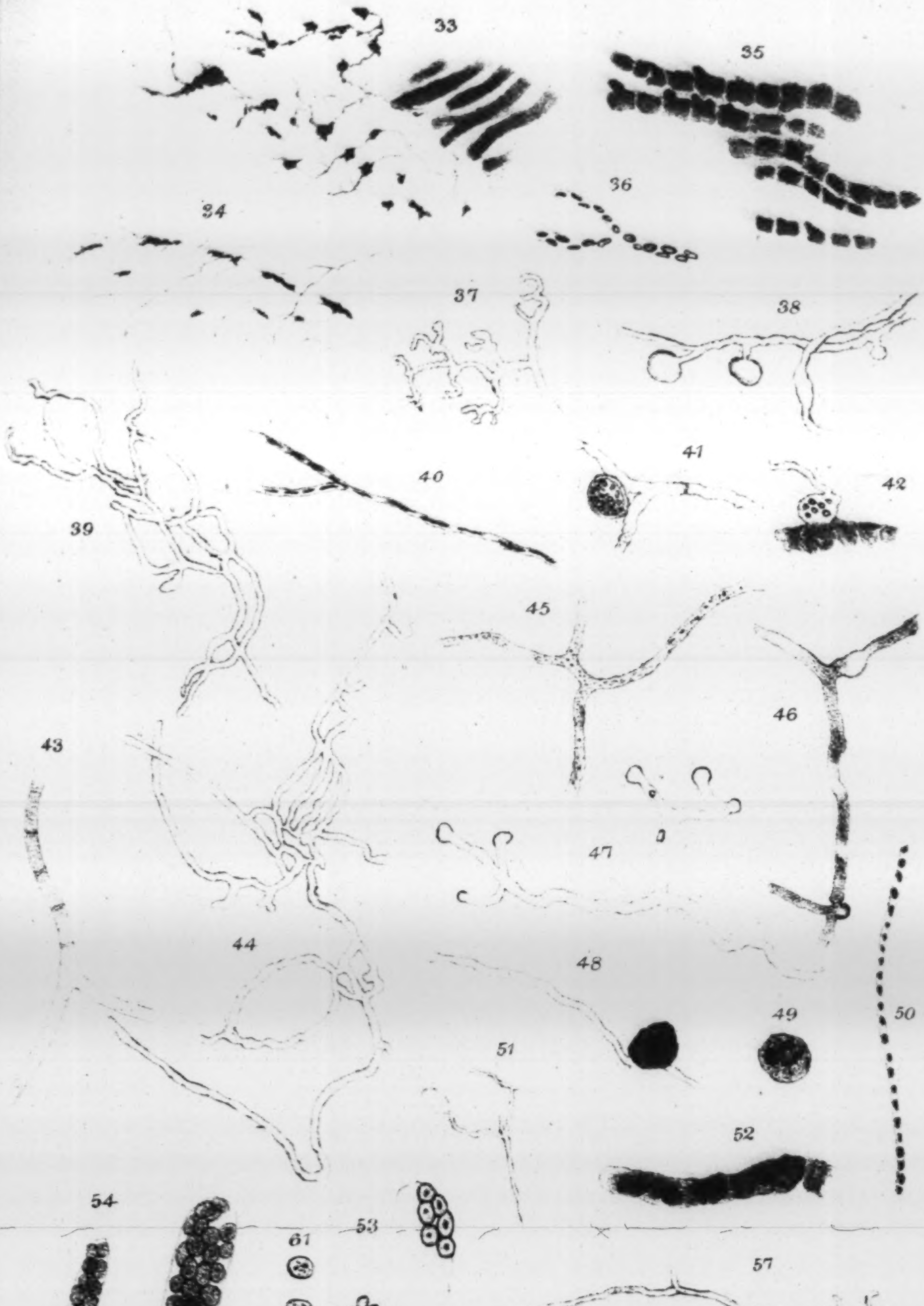


W.H. Wesley lith.

W. West & Co. imp.

were normal to the shells, others were the product of "Confervæ, Spongida, or of minute boring animals."

In 1851 Mr. C. B. Rose, F.G.S., found minute tubular borings in fossil and recent fish-scales; and he published his admirable and short researches in 1855, illustrating them with great care and truthfulness. He noticed that it required a magnifying-power of a $\frac{1}{4}$-inch-focus object-glass to trace the course of the tubes with any degree of distinctness; and he pointed out that their minute diameter of $\frac{1}{2000}$ to $\frac{1}{4000}$ inch distinguished them from the results of boring sponges. Mr. Rose did not attribute the tubes to Confervæ, but to the operation of "infusorial parasites"*.

In 1858, C. Wedl† contributed a paper on the subject of these canals in shells. He described and delineated them; and some of his drawings agree remarkably with those of Mr. Rose. He carried the examination of his specimens much further. He investigated the parasitic canals in recent shells and in fossil Gasteropoda, Bivalves, and Brachiopoda, discovering them even in a *Leptæna* from the Devonian formation. Moreover he exposed a recent shell to the action of dilute hydrochloric acid, and by dissolving the carbonate of lime exposed the vegetable organism. He considered that the parasite was *Saprolegnia ferax*, Kütz., which he termed a confervan. He did not examine any corals.

About the same time, Kölliker‡ contributed a paper to the Royal Society, which contained notices of his having seen the parasitic tubes in a great many shells, Sponges, Foraminifera, and recent Corals; but he did not advance further than C. Wedl in their description. He noticed, however, that the parasite was unicellular in its construction, and wrote:—"I may further add that the frequent anastomoses of the parasitic tubes remind one of the anastomoses observed in the mycelium of some unicellular fungi; whereas such connexions have not yet, so far as I know, been observed amongst the Confervæ." He stated that the sporangia were quite of the same kind as those of unicellular fungi, and that it seemed probable "that the parasites dissolve the carbonate of lime of the hard structures into which they penetrate, by means of exudation of carbonic acid, which secretion would seem to take place only at the growing ends of the fungial tubes, as they never lie in larger cavities, but are always closely surrounded by the calcareous mass." He considers it possible for the parasite to bore its canal by mechanical force in the horny fibres of sponges, in the way that similar parasites make their way through the cell-membranes of Confervæ and other plants.

Finally, this short history would be incomplete without the interesting notice by Mr. H. N. Moseley which was contributed to the Royal Society

* Rose, Quart. Journ. Microsc. Soc. 1855, no. x. p. 7, "On the Discovery of Parasitic Borings in Fossil Fish-scales" (read June 1854). (Plate in vol. iii. plate i.)

† Sitzungsb. d. k. Akad. d. Wiss., December 1858, Band xxxiii. no. 28.

‡ Proc. Roy. Soc. June 9th, 1859, vol. x.

after I had become engaged in these investigations. He writes*:—"*Vegetable Parasites.* The corallum of both *Millepora* and *Pocillopora* is permeated by fine ramified canals, formed by parasitic vegetable organisms of the same nature as those described by Dr. Carpenter and Prof. Kölliker as occurring in the shells of mollusks &c. The organisms were found in abundant fructification; they were *green*, but otherwise appear to be fungi, as are the parasites of shells &c. Similar parasites are to be found in various coralla from widely distant parts of the world."

The paper communicated by me to the Geological Society "On some Unicellular Algæ parasitic within Silurian and Tertiary Corals, with a Notice of their Presence in *Calceola sandalina* and other Fossils," was read Jan. 19, 1876 (Quart. Journ. Geol. Soc. vol. xxxii. p. 205).

II. *The Range of the Parasites, and a List of Species examined.*

Both Kölliker and Mr. Moseley have shown that corals from different localities and belonging to widely remote seas are affected by the parasitic organisms.

Having found the ramifying tubules and their contents in the corallum of *Flabellum laciniatum*, Edw. & H., from the neighbourhood of Davis Straits, at a depth of 380 fathoms, and in *Balanophyllia verrucaria*, Pallas, from the Mediterranean, at different but not great depths, the previously known range may be thus increased. All the deep-sea corals examined from the bed of the North Atlantic are more or less affected by the parasite, and the littoral forms also. Thus in *Caryophyllia clavus*, var. *borealis*, from a depth of 1 to 30 fathoms, in *Lophohelia prolifera*, from 422 fathoms, and in *Solenosmilia variabilis* (nobis), from 1095 fathoms, the tubes were found in greater or less abundance.

The specimens examined by Kölliker and Mr. Moseley were mostly from seas with a high temperature; and the species of coral to which reference will be made in this communication were located during their lifetime in temperatures from 39°·7 to 55° Fahr. The bathymetrical range of the unicellular parasite is therefore from low-water mark to 1095 fathoms in the northern seas; and the temperature to which it is subject varies from 39°·7 to that of the surface water. But it is evident that it is the most destructive when the pressure is least and the warmth of the sea is greatest; and therefore corals of genera such as *Caryophyllia* and *Balanophyllia*, which are grown in aquaria, exhibit the parasite in perfection.

The range of the coral-parasites in time, whilst admitting that there may be and may have been more than one species, is very great. They may be found in Upper-Silurian corals and in those of later date down to the Tertiary times; and tubular excavations corresponding in their appearance may be found in some calcareous fossils of the Lower

* Proc. Roy. Soc. Nov. 25, 1875, vol. xxiv. p. 64.

Silurian rocks *, filled more or less with the fossilized vegetable matters, the cell-wall being even preserved in the Tertiary forms.

List of Species of Corals examined.

†*Caryophyllia clavus*, Scacchi, var. *borealis* and var. *Smithi*. Range of specimens, low spring-tide to 90 fathoms.
†*Flabellum laciniatum*, Edw. & H. 380 fathoms.
†*Lophohelia prolifera*, Pallas. 90 to 422 fathoms.
‡*Solenosmilia variabilis*, Dunc. 1095 fathoms.
§*Balanophyllia verrucaria*, Pallas. Littoral.
‖*Millepora alcicornis*, Forsk. Littoral.

III. *Method of Investigation.*

The parasitic growths which occur in the dense sclerenchyma of the Madreporaria are best seen by examining thin transverse and longitudinal sections of recently dead corals, and also by submitting whole or parts of specimens to the action of very dilute hydrochloric acid and thus obtaining the remains of the organisms. The upper parts of the corals, which are covered with the soft tissues just before death, are comparatively free from the ravages of the destructive tube-matters; but the lower portions of the corallum (which have, especially in species with endotheca, been long uncovered by living tissue) are usually crowded with the borings of the parasites. Age and the length of time which has elapsed since the removal of the corals from the sea have no influence on the preservation of the canals bored within; for they may be traced in fossil specimens, and also in recent forms which have been half a century or more out of the sea. Moreover, so lasting is the peculiar organic basis on which the parasite depends and in and about which the granules and spicula of carbonate of lime are deposited during coral-growth, that it may be extracted more or less perfectly by weak acids from the oldest corallites which have not undergone fossilization; and in the instances of some mid-Tertiary reef-building forms I found it to be distinguishable. The most complete organic films are to be obtained from corals recently dead. The continued preservation of the vegetable cell-wall and its cytioplasm appears to depend upon the same causes which determine that of the organic film. Sometimes the delicate tubular cell-wall may be traced, in old specimens decalcified, amidst the organic matter; and I have noted its preservation in a Miocene coral ¶ as a transparent and probably mineralized structure; but usually age appears to affect the cell-wall, which is commonly found in a very indifferent condition and more or less imperfect in old and dry specimens. In some fossils the spores, either oospores or conidia, are found in a wonderful state of pre-

* P. M. Duncan, Proc. Geol. Soc. (abstract of communication) for Jan. 19, 1876.

† From the North Atlantic. ‡ From the Spanish coast.

§ From own aquarium. ‖ From Bermuda.

¶ Quart. Journ. Geol. Soc.

servation; their shape is perfect and they have been carbonized. Moreover, the cytioplasm, so transparent or minutely granular in most recent specimens, is to be recognized in old corals after they have been decalcified, and in fossil corals also, in the form of dark linear masses *.

The method of entry of the parasites may be studied by examining the outside of the coral, and then making thin sections, both transverse and longitudinal and radial and perpendicular to the surface. In doing this the nature of the minute thread-like green filaments in and on the coral should be noted, and some specimens should be decalcified with them attached. Perfect septa of recent corals, which have not become dry or which have been lately soaked, may often be examined satisfactorily without sections being made, and the network of ramifying tubes may be readily observed in them; for a coral structure well permeated by its natural medium is much more transparent than the dry section which may be made from it.

The sections of the hard structures may be examined after being placed on slides (a small quantity of Canada balsam being used to cause them to adhere) without any covering; and the decalcified specimens, after being washed in water, may be mounted in cells with glycerine.

Reflected light may be used satisfactorily with a low magnifying-power in examining the dry section; for it exhibits the remarkable silvery appearance of the bundles of minute filaments as they enter the theca of the coral, each filament being tubular and refractive.

A magnifying-power of 400 diameters and an achromatic condenser are the most useful appliances in examining transparent specimens and the decalcified structures by transmitted light; and the ability to see the long tubes of very different calibres perfectly and well-defined, and distinguishable from the spicula and their intermediate dark edges, is only possible under a well-corrected object-glass.

The following order of examination should be followed:—(1) The examination of the canals on the inside and in the septa of corals; (2) the examination of corresponding decalcified specimens; (3) the examination of the outer structures, so as to determine the mode of entry of the parasites, hard and decalcified specimens being used; (4) observations should be made in and about those parts of corals where there is much organic basis amidst the sclerenchyma and between the two laminæ of a septum, and the large size of the associated filaments noticed in solid and decalcified specimens.

IV. *The usual appearances of Typical Parasitic Canals.*

On examining a thin dry section of a coral, made at a little distance from the outside, or on looking through a transparent septum, the para-

* The process of carbonization in these delicate filaments and spores may be imitated very significantly by placing some of them under the influence of slight heat and pressure. A thin glass cover being put over a mass, and a spirit-lamp flame being held beneath, it will be found that blackening of some of the vegetable structures will ensue, without the application of much or continuous heat.

sitic borings, when present, usually resemble long dark lines with a longitudinal and central transparent space. The lines may branch here and there, and usually at a considerable angle, and they often dip out of and come within the focus of the microscope in their more or less long course. They are singularly persistent as regards their calibre, which, always small, is unchanged even in the branches and branchlets. The commonest tubes (for such are these linear and longitudinally luminous appearances) are about from $\frac{1}{5000}$ to $\frac{1}{8000}$ inch in diameter. They are simple excavations, tubular in shape, and they have no special hard tubular wall. Each contains a vegetable filament, consisting of a tubular cell-wall and contents. They are cylindrical, and the breadth of the longitudinal light line depends upon the amount of vegetable material within the tube, upon the shape of the perforation, and the nature of the surrounding hard structures. When the cytioplasm of the filament which is within the continuous cell-wall is simply glairy matter, the tubes are often difficult to distinguish, as their whole lumen is transparent; but when the tubular cell-wall is crowded with granules, the light does not pass at all, and the whole tube appears as a dark line. Between these conditions are many, and which refer to the amounts of granular cytioplasm here and there in the same tube. The aggregation of granules determines the clearness of the longitudinal light line, its loss here and there and its replacement by a kind of moniliform appearance of alternate light and shade (Plate 6. figs. 11–17).

The parasitic canals, although they often branch out and ramify widely, rarely inosculate with others.

Another very common kind of canal is seen in the same situations, and also throughout the whole coral; it rarely pursues a straight course, but bends and curves first on one side and then on the other, and branches, either perfect or stunted, come off from the convexity of the curves, usually directly tangential to them. The stunted branches are short and linear, and give a very marked appearance to the canals, especially when they terminate in a spherical end with or without a branch from it. One of the numerous appearances is that of a straight canal bifurcating at right angles, and the continuation of the original canal assuming the form of a short stunted end just beyond the branching. Sometimes this abrupt termination is enlarged and, moreover, less globular in shape (Plate 6. figs. 16–19).

Swellings or enlargements of the calibre of the canals are not infrequent, and they are usually impervious to light. It will be noticed in most specimens of long canals that there is a peculiar wavy outline of their path, the excavations not being absolutely in a right line but in a series of minute and continuous curves.

In some specimens the canals are very long; in others they are short, and every variety may be seen in the same section. The direction which they take, and often their length, depend upon the minute structure of the hard parts of the coral.

The number of the canals varies also in different specimens, age and bathymetrical range regulating the parasitic growth more or less; they are more common in the corals with lax textures like the *Perforata* than in the *Aporosa*.

In some of the inner parts of the hard deep-sea corals the canals are few in number and are very long and narrow. These interior canals often give off, either from their whole surface, from parts of it, or only from the swollen parts, long and very delicate tubes of from $\frac{1}{10,000}$ to $\frac{1}{20,000}$ inch in diameter; but this appearance, which is very common in such species as *Balanophyllia verrucaria* and *Caryophyllia clavus*, var. *borealis*, is extremely rare in others (Plate 6. figs. 20–28).

When parts of corals corresponding to these interior sections and septa are decalcified, a repetition of the appearances of the canals is seen in the arrangement of their contents. Each canal contains and has its interior lined by a homogeneous transparent tubular cell-wall, and this contains fluid and more or less solid contents here and there.

The cell-wall is continuous through all the ramifications, and ends abruptly in the stunted branches, and it lines the globular ends and all the swellings. It can be traced to give off the extremely delicate tubules to the minutest ramifications just noticed. The cell-wall structure appears to lie against the sclerenchyma of the coral, but not in a perfectly smooth canal; for there are minute pits and roughnesses on the inside of the canal, into which the tube itself does not fit (Plate 5. fig. 7, and Plate 7. figs. 56 & 57).

Cross partitions in these tubular cells are very rarely seen; and even in the majority of instances in which one might feel disposed to admit their occurrence, it is possible that the dissepiments are more apparent than real, being the result of light passing up between two closely approximated masses of granules (Plate 7. figs. 58–60).

This coalescing of granules in masses, with spaces between them, is commonly seen; and a moniliform appearance is also given to some minute tubes by a corresponding arrangement of the cytioplasm (figs. 36 & 40).

No starch can be detected with the usual tests; but in many filaments there is evidently a bright sap-green tint, which is increased when light is transmitted through the refractive cytioplasm.

Throughout, the resemblance of the canals and their filaments to a mycelium is very striking.

Occasionally a long and wide canal, crammed with granular cytioplasm, passes far into the substance of the coral and may be seen with those just described. It can be traced outwards to the surface of the coral, whence all the others come—some entering from without directly, and others being branches of canals situated close to the outside or offshoots of what appear to be cavities filled with oospores (Plate 5. figs. 2–5).

V. *The Parasitic Canals near the exterior and their Methods of entry: Reproductive Elements.*

The finer canals and those of all diameters except the very smallest may be occasionally traced to the outside of the hard coral-structure, so that the method of entry of the parasite can be determined. This is greatly assisted by decalcifying, after noting the character of the vegetation, which is inseparable, except by tearing, from the wall of the specimen.

Methods of entry.—1. Rarely a long typical canal may be seen opening out through the coral-wall without any increase of calibre.

2. Dark globular or short cylindrical-shaped cavities exist in the very outside of the theca, and usually in such positions where the external ornamentation or where the intercostal spaces admit of substances resting readily. The cavities are large, and vary from $\frac{1}{500}$ to $\frac{1}{1000}$ inch in diameter; their contents give a dark and opaque appearance to them, and they give off many very fine short canals, canals of larger calibre which pass more or less inwards, and the common long ramifying and non-inosculating canals, there being one or many of these (Plate 5. figs. 1–7 & 10).

3. Long, straight, and also curved canals of large calibre, $\frac{1}{500}$ to $\frac{1}{1000}$ inch, usually constricted here and there, and in some instances having hemispherical projections. They sometimes pass far inwards (Plate 5. fig. 8), and then their contents are usually not so crowded as to prevent light being transmitted; and, indeed, in one specimen long portions of the canal were deficient in granular, and were filled with homogeneous and clear cytioplasm. Sometimes the wide canal ends in a *cul-de-sac*; but in most instances smaller canals pass off irregularly from it, and even some of the minutest.

4. Irregular excavations occur on the surface of the coral of no great depth, into which shallow cup-shaped depressions enter; and these are either with sharp edges and give entry to a typical tube or to many fine and short tubes in addition (fig. 10).

There is in a specimen of *Flabellum laciniatum* a tunnel reaching inwards from one of these irregular excavations which has three more or less globular enlargements on it, the last being continuous with a very short prolongation of the tube.

5. Great numbers of very branching canals form a close network and extend into the coral-structure, usually from the top of a costal or septal ornament, in *Balanophyllia verrucaria* for instance. These glomeruli arise from a depression in the outside of the coral, or from a decided large penetration, and it appears as if a mass of oospores had collected therein and germinated (Plate 5. fig. 1).

Transverse sections, of necessity, cut through these masses of branching and anastomosing tubes at different angles; and it is possible, therefore, by comparing numbers from the same coral to estimate the length of a mass and to recognize the typical canals which eventually arise from it.

Longitudinal sections made parallel with the septal ends often exhibit these glomeruli to perfection; and under a low power they may be seen as bright refractive tube-masses entering the coral at stated intervals. This appearance is also presented in old corals in transverse sections (fig. 29).

Observations on Decalcified Specimens of the Outer Parts.—After submitting sections or pieces of corals known to present the appearances just enumerated and described to dilute hydrochloric acid until the hard parts are destroyed and the organic basis-structure remains free, they should be washed in distilled water; and portions of the remaining matter may be put up in thin cells with glycerine slightly diluted.

The organic tissue is usually preserved in films, but occasionally, and especially in the semiperforate *Balanophylliæ*, masses of it may be obtained conforming to the widely reticulate structure of the exterior of the coral. It is transparent, almost homogeneous, and only granular here and there; but the paths of many fine parasitic filaments may be traced in it. Usually the occurrence of larger canals determines the raggedness and breaking-up of the organic film as a whole, as they have drilled through and along it in all directions.

Surrounding this tissue, and usually inseparable from it, are the vegetable parasites, now freed from their calcareous covering.

Reproductive Elements.—Oospores, zoospores (non-ciliated), separate or in masses, and the latter often within filaments, large confervoid-looking filaments, large unicellular tubules crowded with dark cytioplasm, and filaments of different lengths and diameters and with numerous or few branches (all bearing a definite relation in point of size to the canals whence they came) are readily seen and distinguished.

The extremely fine canals, $\frac{1}{10,000}$ inch, do not usually yield any filaments after decalcifying has been even very carefully done; but I have been able to draw a few, which are all the more interesting because they are evidently extremely delicate utricles arising from the fusiform or roundish zoospores which in this parasite, as in *Saprolegniæ*, germinate before expulsion and before having reached their true zoospore or mobile condition. In Plate 7. fig. 55, zoospores are seen with filaments, and in fig. 61 separate spores are delineated.

It is evident, after the examination of these filaments and their associated zoospores, that the cause of the furry appearance of some larger canals (Plate 6. figs. 21, 22, 23) is due to the development of corresponding growths from germinating cells within and their penetration of the hard parts after having perforated the parent cell-wall and the dense structure surrounding.

Rarely a minute tube may be seen passing off at right angles from one of the largest kind; they are usually short, and do not appear to come from within, but to be offshoots of the parent cell-wall.

Oospores are to be found closely adherent to the organic basis-structure and amidst the branching thread-like tubes which are inseparably attached

to the outside of the corals. They are also to be distinguished in fossils, crowding the cavities on the outside of the coral and some of the dissepimental spaces; but I have failed to see them in corresponding numbers and positions in recent forms.

The oospores are large, and reach $\frac{1}{1000}$ inch in diameter. They are usually globular, but sometimes slightly lobular; and in the latter case there is an evident internal grouping of granules which makes the mass look like a tetraspore. They are dark in colour, and there is an external cell-wall. Becoming fixed to the coral-wall, they sometimes flatten out and remain as circular black spots, a long filament being continuous with them and passing into the hard structure; but as yet I have not seen a satisfactory tubular prolongation of a globular spore, except in the Silurian *Goniophyllum*, where they present beautiful tubes.

VI. *The large confervoid-looking Filaments within the Organic Basis.*

The septa of all Corals are in two longitudinal parts, which are separated more or less perfectly in the median line. Hence transverse sections show in the septa of recent corals two cut surfaces, each representing the top of a plate or lamina separated by a linear space or by a corresponding organic tissue. In some species this intermediate structure reaches the surface at either the calicular or the costal edge of the septum. It is the relic of an involution of the dermal structures in and around which the sclerenchyma was deposited. Moreover the organic structure thus intermediate really passes into the septal laminæ in all directions, as can be proved by decalcifying them. It would appear that the younger the coral is, the more distinct are these intermediate structures, and that with age they become more or less filled up with carbonate of lime, and may finally thus become obliterated. Certain genera of rapidly growing forms appear to have the spaces more distinctly developed than others.

These organic structures, coming to the surface of the coral, of course are, under favourable circumstances, soon attacked by penetrating parasites; and the possibility of entry and of growth is so great in some instances, that wide layers of very confervoid-looking growths flourish, and present a very peculiar appearance under the microscope (Plate 7. figs. 33–35). The filaments forming one or more layers are nearly parallel, are large (from $\frac{1}{1000}$ to $\frac{1}{2000}$ inch), rarely branched, and exhibit transverse markings, which are not true articulations, although the filament frequently breaks off at them. The filaments are tubular, and have a distinct cell-wall, which is swollen out here and there; and their cytioplasm is aggregated more or less regularly, and in some places resembles conidia. Little processes project from the wall of the filament here and there, and are of about half the calibre of the parent tube. In some filaments the cytioplasm is well separated from the wall, but usually it fills the whole tube.

The jointed or articulate appearance is particularly visible when the fila-

ments are only in the organic substance or are only surrounded by very thin films of carbonate of lime; for when these large tubes penetrate the solid parts of the septa their diameter appears to be slightly less, their cell-wall ceases to be distinguishable, and their contents are usually undivided. But occasionally it appears that some burrow very successfully, and a long and wide tube results, which, in the majority of instances, is full of granular cytioplasm and rarely sufficiently empty to exhibit a central bright band of transmitted light.

The Minute Filaments from the large kinds.—Although the broad filaments found within the septa (especially of *Caryophyllia clavus*, var. *borealis*, of *Flabellum laciniatum*, Edw. & H., and of *Balanophyllia verrucaria*) do not often bifurcate or ramify, yet they now and then give off long ramifying but not often anastomosing filaments of extreme tenuity. They are found in crowds in some spots of the septa, and their diameter is about $\frac{1}{20,000}$ inch, their general distribution and method of branching being a very exact counterpart of some of the larger growths already noticed as occurring so frequently in the other hard structures of corals.

Usually these very fine canals retain the same diameter throughout their length. In some corals they do not run a long course, but bend soon or even ramify abruptly, and in both cases pass into irregularly shaped cavities, emerging from them at the opposite end or absolutely terminating in them. These cavities are not unlike ill-developed bone-lacunæ in appearance, and are frequently found in series on the periphery of masses of long, radiating, spicular structures. Usually of very small size, and more or less irregular in shape or elongate, they sometimes are the cut ends or tubes of irregularly shaped parasitic borings which have been formed by another and larger filament.

The resemblance of these fine canals to those which pass off so frequently from the larger parasitic tubules in the thick wall or columella of the same or other species of coral is very exact, and probably they have the same cause of origin.

Large confervoid-looking Filaments in the Body of Corals.—In transverse sections of some corals, especially in the genera *Lophohelia* and *Solenosmilia*, some long rows of the very large confervoid-looking filaments are often seen cut across more or less obliquely. These rows are formed of numbers of large tubes placed side by side and in one or more layers, and they occur in those situations where involutions of the dermal structures took place during the growth of the coral. These tubes give out fine ramifying tubes such as those just mentioned, and which usually terminate in irregular-shaped cavities.

But usually the contents of the large tubes are crowded and not separated off, and it is rare to see spaces in them through which light can penetrate. They branch, and usually produce ramifications which are of the same diameter as the parent filament; and where they are cut across they present an irregular granular appearance, which is partly due to

the cytioplasm and partly to the existence in the canal-wall of minute more or less perfect perforations. It is evident that the canals of these large parasitic filaments may be correctly compared with the larger long and short penetrations on the outside of the corals, which are the means of entry of the bulk of the parasitic growths.

VII. *Method of Entry and Growth of the Parasite.*

It is evident from the examination of the sections of the outside of corals that the parasite obtains entry below the living dermoid tissues of the coral at spots where there is usually a crowd of competitors for attachment, shelter, and also for boring. Some minute cavities on the outside, either the result of the operations of other organisms or the product of the ornamentation of the coral, evidently and constantly contain masses like large conidia or oospores or shapeless masses of granules. The boundaries of these cavities often relate to the intermediate spaces between columns of spicula peculiar to the coral sclerenchyma; the organic basis of the hard structures comes in abundance close to the outside of the theca in such positions, and it is the particular food of the mycelium about to enter. The tubule of the ingrowing parasite comes from a conidium, oospore, or from a granular mass which probably is a zoospore; and the entry can only be by growth-force, and by the assimilation and removal of the organic basis, together with the dissolution of the carbonate of lime of the coral by the development of carbonic acid from the end of the tubule. The existence of movement in the cytioplasm and possibly in the cell-wall may be reasonably inferred; and this would tend to drive out fluid between the hard walls and the soft internal tube. The solution appears, however, to be only active at the growing end of the tubule; and this growth is clearly often stopped by a hard and solid mass of spicula, there being an insufficient quantity of the organic film there for the nutrition and *vis viva* of the parasite. It is evident that the entry must be made during the life of the corallite or very shortly after death.

There is one manner in which the parasite reaches the outside of the coral and becomes fixed so as to penetrate, which is very remarkable and also suggestive of the group of Thallophytes to which it belongs.

Species of *Bryopsis* and *Cladophora* were living in the aquarium with the *Balanophylliæ* which were afterwards cut and, in some instances, decalcified. These reticulate and dark green forms grew upon the lower parts of the corals, where the bright orange animal matter was no longer existing; and, as the corals grew weak in their nutrition and the tentacles rarely expanded, there was an evident struggle between the vigorous plants and the dying Cœlenterate. At last extremely fine filaments of *Bryopsis* appeared on the septa, and, as the thin films of living tissue grew smaller, they encroached more and more.

I thought at first that the penetrating parasite was either a "rootlet"

of *Bryopsis* living under extraordinary circumstances, or that it was a modified form of it, especially as I noted instances where the filaments of this plant had penetrated and perforated through projecting nodules and ridges of the hard parts of the coral.

The adhesion of the *Bryopsis* would give the delicate filament about to penetrate a *point d'appui*. But on placing some tubes of *Bryopsis* which had short filaments on their outsides, like those obtained by decalcifying, in glycerine, this liquid speedily entered what appeared to be the tubular structure of the plant, filled with dark green granules and cytioplasm. It filled the tube and made the cell-wall sufficiently transparent to show that there were no cell-contents, but numerous filaments of an Achlyan-looking parasite. These had penetrated the *Bryopsis*, had grown at the expense of its cytioplasm, and, finally, they were making their way out through the wall and coming in contact with the coral.

I have not been able to trace the mobile zoospores of the Achlyan or the origin of parasitic filaments from them; but it is evident that the parasite exists on the outside as well as within the coral, and in the tubular or vegetative form, and that the external filaments contain immature zoospores which develop tubules which penetrate the parent wall, impinge against the coral, and penetrate (Plate 7. fig. 55).

VIII. *Structure of Reproductive Elements and Classificatory Position.*

The parasites, whether enclosed within their tubular perforations, which assimilate to their shape, or rendered visible by the action of dilute acid, or when free on the outside of the corals, present the appearance of the mycelium of fungi of such orders as the Hyphomycetes and Physomycetes. The filaments, whatever may be their diameter, are furnished with a continuous cell-wall, and dissepiments are extremely rare, being only recognized once or twice in hundreds of specimens. The filaments rarely inosculate, but branch either rarely or with great frequency. There are often secondary and other branchlets; and the width of the calibre of the whole tube is usually maintained.

It is very usual for branching to take place at right angles to the parent filament, and for a small rounded continuation of the last to form beyond the ramification.

More or less globular or hemispherical swellings occur on the side of the filament, and occasionally it is swollen out into one or a succession of spherical enlargements. The filaments terminate in *culs-de-sac* usually of the same diameter as the rest of the tube; but globular or irregular-shaped enlargements are by no means uncommon at their ends. In all this the parasite resembles many fungi. It is towards the ends of some smaller filaments that a cell-dissepiment occurs, separating a terminal portion, only filled with dark cytioplasm, from the rest of the filament, which often contains a refractive fluid with large granules here and there (Plate 7. figs. 49–61).

The cell-contents of the filaments are:—

1. Glairy transparent fluid; this renders the canals often difficult of distinction, and staining with carmine renders them usually visible.

When removed from their canals the filaments, without any other cell-contents than this glairy matter, usually show a very distinct cell-wall with occasional refractive granules close to it in a few places. Usually there is no colour present, but in some specimens the glairy fluid is tinted a faint yet bright sap-green.

2. Dark-coloured, brown or black, cytioplasm collects here and there in the filaments, with interspaces where the clear fluid mentioned above is present; or the whole tube may be crammed with the structureless and dark mass. This cytioplasm often aggregates in regular and consecutive portions of the tube, and the intervening colourless fluid gives the appearance of cell-dissepiments.

In most tubes the dark cytioplasm, small in amount, is situated close to the cell-wall, and, there being some structureless clear fluid in the axis, the filaments are refractive.

Granules, excessively minute, form in the coloured cytioplasm, and conidia gradually develop here and there by their aggregation in all parts of the filaments. Small ovoid bodies with two or three minute dark molecules within them, besides a refractive fluid, are formed out of this cytioplasm within the filaments, and in the enlargements, and on them, and at their ends.

No special terminal cell containing these reproductive conidia and sporidia (zoospores imperfectly developed) appears to exist; but probably the rounded and elongated ends of some filaments are the analogues of the terminal fructification-cells of their congeners.

The passage of extremely minute tubes from larger filaments, through whose walls they penetrate, and the presence of small ovoid bodies giving out minute filaments within the parent cell-tube, are very suggestive phenomena. They coincide remarkably with some parts of the life-cycle of the *Saprolegniæ* *; and this resemblance is enhanced by the presence in the coral parasites of the terminal filaments cut off by a cell-diaphragm from the rest. Moreover the globular endings to many of the filaments, or the spherical offshoots of many, greatly resemble some of the parts of species of *Saprolegniæ* †. But whether a cell-wall cuts off the globular cell in the coral parasite, I have not been able to determine; and all the evidence I have is against this being the case.

Outside the coral are long, branching, inosculating filaments, very rarely divided by partitions, and crammed here and there with zoospores, which often produce filaments when still within the cell-wall. Finally the oospores are large, spherical, and apparently arise from compound masses or oogonia. All these details connect the parasite with the

* Thuret, Ann. des Sci. Nat. t. xiv. pl. 22. fig. 8.

† Thuret, *op. cit.* figs. 10, 11.

Saprolegniæ; but a specific identity with *S. ferax*, Ktz., is wanting. As in another work I have absorbed *Saprolegnia* in *Achlya**, I propose to classify this parasite in that genus, and to name it *Achlya penetrans*.

But in thus provisionally classifying these parasites, their resemblance to the filamentous rootlets of *Codium* and to very delicate specimens of *Bryopsis* must not be forgotten. The large confervoid-looking filaments found within the interlaminar structure of the septa closely resemble those of the *Bryopses* which cling in a close reticulation to the outside of many corals. *Bryopsis*-filaments do penetrate projecting parts of the coral. The tint of the clear cytioplasm of the parasite is often pale sap-green; but there is no other evidence of the existence of chlorophyl in the contents of any of its very variously shaped filaments. The *Bryopsis* is, of course, crowded with green granules, and doubtless chlorophyl is present. The reticulate rootlets of *Codium* are often colourless, the larger and upright portions of the plant being green. The probability of the presence of chlorophyl being determined by the action of light upon colourless or dark brown cytioplasm should therefore be considered before an arbitrary line is drawn between the *Achlyæ* and such very remarkable forms as those included in the genera mentioned above.

From the results of my examination of Upper-Silurian corals and of Lower-Silurian arenaceous Foraminifera, it is evident that a parasite closely resembling *Achlya penetrans* lived within them during those remote ages. Corresponding in shape with the Silurian form of parasite are others which are fossil within the corals of later ages. The main differences between the ancient and the modern forms consist in the larger calibre of some of the filaments of the first, their long, often unbranching course, and the frequent development of *Conidia*-looking bodies within them, and the spherical shape of the spores; but it is quite possible that these are not distinctions which are of a specific value.

The modern coral-parasite is evidently the descendant, with slight or, possibly, no modification, of those which flourished during successive world-wide changes in floras and external conditions. Hence it would, in all probability, have had its life-cycle made complicated, and a metamorphosis involving vegetative and mobile stages has been superadded. It is not an assimilator of putrescent or rotten animal matter, but of the nitrogenous and undecomposed organic basis of the coral; and in this it resembles the organisms which destroy some living Diptera and other aërial insecta. Moreover this resemblance in function is possibly caused by continuance of individuality; and if this be true, it adds vastly to the difficulty of placing the parasite in a philosophical scheme of classification. *Empusina* (the fly-killer) certainly is an aërial form of *Achlya*; and *Empusina muscæ* turns into *Achlya prolifera*. It is, then, quite within the range of possibility, and, indeed, it is extremely probable, that the

* Micrographic Dictionary, 1875, 3rd edit., Articles *Achlya* and *Saprolegnia*. See also *Sporendonema* and *Empusina*, and Article on Confervoideæ.

coral *Achlya* is the aquatic form of some aërial "fungus" which, like it, devours and increases upon organic matter.

Some of the most perfectly developed *Achlyæ* parasitic within corals were obtained from specimens of *Balanophyllia verrucaria* which came from the Mediterranean, and which I kept for many months in an aquarium. The vegetation in the aquarium consisted of species of *Cladophora* and *Bryopsis*, and they grew not only on the rockwork, but also on the bases and sides of the corals which had been left uncovered during growth by the orange-coloured ectoderm. As some of the corals became weak, their colours becoming pale, the organic or living tissue being thinner and the tentacles less expanded, the weed encroached and, finally, in one or two instances appeared on the septa, the living ectoderm having become abraded or dead on those spots.

After a while a bulky "mould" spread over the whole calice of the coral, and decomposition soon set in. This mould consisted of extremely crowded filaments with occasional dissepiments, and resembled a *Botrytis*. It lived in the water, and grew with great rapidity.

This fact renders Berkeley's statement that *Achlya* may be an aquatic form of *Botrytis* very probable; and certainly the filaments of many of the internal parasites of the corals greatly resemble those of *Peronospora*. If, then, the coral-parasite follows the life-cycle of its congeners, it may live under different conditions in various organisms, and receive as many generic titles and specific names.

Doubtless there is a motile stage as a freely swimming zoospore in one of the life-cycles; and in this this feeder on organic matter relates to its remote ancestry amongst the Amœboids.

If the arbitrary nature of all the classifications of organisms which assume different shapes and habits under different external conditions be admitted, the position I have assigned to the parasite as *Achlya penetrans* appears to be correct. But it may be more philosophical to state that it belongs to a group of interchangeable forms, and that it is the marine and parasitic expression of the arbitrarily separated genera *Achlya*, *Saprolegnia*, *Botrytis*, *Peronospora*, and probably *Bryopsis*.

IX. *On the Occurrence of* Achlya (Saprolegnia) ferax, *Ktz.*, *in* Caryophyllia Smithi.

On submitting an old specimen of the common broad-based *Caryophyllia Smithi*, from the Devonshire coast, to the action of dilute hydrochloric acid, a vast amount of internal parasitic growth was obtained. This growth in some respects resembles that of *Achlya penetrans*; but in its close reticulations of long and rarely branching filaments, of $\frac{1}{8000}$ to $\frac{1}{10,000}$ inch in diameter, it assimilates to the well-known parasites of *Anomia* and *Ostrea*.

The resemblance of the fructification to the drawings in Kützing's

'Physiologia Generalis' of *Saprolegnia ferax* is very close (Plate 7. figs. 36, 37, 38, 40, 41).

There is an interesting point about this Achlyan from the English littoral zone, and that is its resemblance in tint to those of the deeper sea. The delicate sap-green of the homogeneous viscid granuleless refractive cytioplasm is evident enough. Many filaments, however, are colourless.

X. *Summary.*

Quekett, Rose, Wedl, Kölliker, and Moseley have noticed and described the borings of vegetable parasites in molluscan shells, fish-scales, and corals; but no special attention has been paid to the filaments penetrating the last-mentioned organisms.

Corals from the littoral zone down to 1095 fathoms are frequently the seat of the parasitic growth of two kinds of *Achlyæ*, whose horizontal range is from Davis Straits to the tropics and 15° S. lat.

Fossil corals of Silurian age were also affected by closely allied, if not specifically identical, growths.

The method of investigation is by making thin sections of the sclerenchyma, and also by dissolving out the carbonate of lime.

The parasites are filamentous, and fill up the canals which they form; they resemble a mycelium, and penetrate the coral, living upon the organic basis, and having their length, breadth, and straightness, or branchings, dependent on the peculiar nature of the arrangement of the spicula in the different species of the Madreporaria. The entry is made from oospores, zoospores, and by the accidental contact of the parasites whilst perforating algæ situated on the wall of the coral; and the penetration and growth appear to be the combined results of the formation of a soluble bicarbonate of lime by the action of carbonic-acid gas evolved from the growing end of the tubular filament, of the pressure incident to growth, and of the movements of the cytioplasm and the cell-wall.

The vegetative life of the parasites is accompanied by reproductive efforts within the corallite; for the aggregation of granules within the viscid transparent cytioplasm can be detected, and their formation into large conidia and into small unciliated zoospores also.

Following the peculiar physiological habit of the Saprolegnian group of *Achlyæ*, the reproductive elements germinate and produce either large or very small tubes which, after penetrating the parent cell-wall, get through the solid investment, and become indistinguishable from the filaments derived from spores attached to the outside.

The diameter of the largest canals containing filaments in which there is occasionally a doubtful dissepiment, and which flourish in the organic matter between the laminæ of a septum, is $\frac{1}{500}$ inch; that of the typical and ordinary tubes is from $\frac{1}{1500}$ to $\frac{1}{3000}$ inch; and the finest tubes are as small as $\frac{1}{20,000}$ inch in diameter.

The canals and included filaments in some instances increase in calibre at certain spots, and even form globular expansions, but usually the same diameter is retained; the enlarged portions relate to the reproductive process. The cell-wall of the filament is in close contact with the sclerenchyma of its canal.

In a littoral species (*Caryophyllia Smithi*) the parasite is identical with *Saprolegnia ferax*, Ktz.; but there is a manifest distinction between it and those of the other forms. The parasite of the littoral coral greatly resembles those of the shells of Mollusca and of the scales of fish. Although it is quite possible that all the parasites of the corals described may be referred to one species, their type being altered by the peculiar conditions surrounding them, still it is thought advisable to regard them as members of two species. The classificatory position of the parasites is in the midst of a group of forms which have complicated life-cycles, such as the Achlyans (proper), the *Saprolegniæ*, and *Empusinæ* and Botritidæ, and the filamentous false-root bearing genera *Codium* and *Bryopsis*—forms which are more or less the expressions of one organism under different conditions and age.

EXPLANATION OF THE PLATES.

Plate 5.

Fig. 1. *Balanophyllia verrucaria.* A longitudinal section of the coral close to the end of a septum, showing masses of the tubules of *Achlya penetrans* close to their entry. × 40 diameters.

Fig. 2. *Caryophyllia clavus*, var. *borealis.* A large tubular excavation opening out at the surface of the coral. × 350 diameters.

Fig. 3. Another excavation.

Fig. 4. A large tubular excavation cut across and exhibiting tubes of the parasite coming from it. × 350 diameters. The same coral as figs. 2 & 3.

Fig. 5. A transverse section of a tube found in the midst of the same coral; it shows the porose condition of the tube-wall and some branches. The coral-structure around is not shown. × 350 diameters.

Fig. 6. The origin of a long parasitic tubule from a large entry-tube. × 350 diameters.

Fig. 7. The porose condition of the wall of the tubular cavity, and very minute tubules coming off from the perforation, and a long and larger tube are shown. × 350 diameters.

Fig. 8. A series of dilatations in a large entry-tube. × 300 diameters.

Fig. 9. A large tube branching. × 300 diameters.

Fig. 10. A partly normal and partly parasitically formed concavity at the edge of a coral, with minute tubules and a larger tube coming from it and penetrating the coral-structure. × 300 diameters. All these views from fig. 2 inclusive are from the same coral.

Plate 6.

Fig. 11. *Flabellum laciniatum.* Typical parasitic tube of *Achlya penetrans* filled with cell-wall and cytioplasm.

Figs. 12–15. Various shapes of typical tubes.

Figs. 16, 18, 19. Tubes branching at right angles and terminating in a blunt or globular head.

Fig. 17. A ramose tube. These drawings (figs. 11–19) are from one coral, and are magnified 300 diameters.

Fig. 20. *Balanophyllia verrucaria.* A parasitic tube showing minute furry-looking collections of lateral branches. × 350 diameters.

Fig. 21. Same subject. The tube entering and branching.

Fig. 22. Glomerulus and ragged tube. × 350 diameters.

Fig. 23. A parasitic tube entering, having glomeruli and ending. × 350 diameters.

Fig. 24. Globular terminations. × 350 diameters.

Fig. 25. A tube with glomeruli. × 350 diameters.

Figs. 26, 27, 28. Long and very minute tubules coming from a typical tube and some conglomeruli. × 350 diameters.

Fig. 29. Entry of tubes. × 40 diameters.

Fig. 30. A tube ending in an enlargement with commencing offshoots. × 350 diameters.

Fig. 31. A globular termination. × 350 diameters.

Fig. 32. A termination with a tube springing from it. × 350 diameters. All the views from figs. 20 to 32 inclusive are from the same coral.

PLATE 7.

Fig. 33. *Lophohelia prolifera.* Large tubes nearly filled with cell-wall and cytioplasm within the interlaminar space of a septum; minute tubules come off, and occasionally end in spaces or lacunæ. × 350 diameters.

Fig. 34. Minute tubules and lacunæ. × 350 diameters.

Fig. 35. Large interlaminar tubes. × 300 diameters.

Fig. 36. This and all the following figures are taken from decalcified specimens put up in glycerine. Some swelling of the parasitic supports occurs. A fine ending of a tube, showing wall of parasite (*Saprolegnia ferax*), unicellular and ovoid bodies with one or more granules in them. × 350 diameters. From *Caryophyllia clavis*, var. *Smithi.*

Fig. 37. A ramose filament of *Saprolegnia ferax*, very characteristic. The cytioplasm is colourless. × 400 diameters.

Fig. 38. Filamentous tubes with globose endings. × 400 diameters. Figs. 37 & 38 are from the same coral as fig. 36, and refer to *Saprolegnia ferax.*

Fig. 39. Filaments of *Achlya penetrans* from *Balanophyllia verrucaria.* A mass of them. × 350 diameters.

Fig. 40. A small filament of *Saprolegnia ferax* in *Caryophyllia clavis*, showing cell-wall and cytioplasm collected more or less in regular spots with vacant interspaces. × 350 diameters.

Fig. 41. A filament of *Saprolegnia ferax* with a dissepiment and ending in branches, and a globular mass filled with granules. × 350 diameters.

Fig. 42. A globular part of a filament with several refractive granules. × 400 diameters.

Fig. 43. A dissepiment in a filament of *Achlya penetrans* from *Balanophyllia verrucaria.* × 400 diameters.

Fig. 44. Filaments of *Achlya penetrans* from the same coral. × 400 diameters.

Figs. 45, 46, 47. Filaments of *Achlya penetrans* from the same coral. × 350 diameters.

Fig. 48. Oospore and filament of *Achlya penetrans* from surface of *Balanophyllia verrucaria.* × 300 diameters.

Fig. 49. Oospore.

Fig. 50. Moniliform appearance of cytioplasm in a filament from the same coral. × 300 diameters.

Fig. 51. A filament with a globular end and branches of *Achlya penetrans.* × 400 diameters.

Fig. 52. A large filament close to the entry of the *Achlya*, with conidia-like masses in the cytioplasm. × 400 diameters.

Fig. 53. A tubular filament ending and having numerous ovoid bodies close to the cell-wall (zoospores). × 600 diameters.

Fig. 54. Interlaminar tubes of *Achlya* crowded with cytioplasm. × 400 diameters. (See figs. 33–35.)

Fig. 55. Zoospores sending out filaments when within the parent filament. × 600 diameters.

Fig. 56. A filament like fig. 53.

Fig. 57. A typical filament of *Achlya penetrans*.

Figs. 58–60. Endings of filaments with dissepiments and granules. × 400 diameters.

Fig. 61. Zoospores, non-ciliated. × 600 diameters (high eyepiece).

Appendix to a Communication on Thallophytes in Recent Corals. By Professor DUNCAN, F.R.S. &c. Received May 11, 1876.

Since my essay on the Thallophytes in Recent Corals has been sent to the Royal Society, I have become aware, after the examination of some deep-sea corals (depth 363 fathoms), that thread-like dark green organisms of a vegetable nature ramify on their surface and penetrate it. These filiform organisms are visible to the naked eye, and, when examined under the microscope, are shown to be unicellular and to contain green colouring-matter. They leave linear depressions on the surface of the coral which correspond with them in diameter and outline, and they penetrate and dip under the surface sometimes to reappear above. Their course may often be traced in *Amphihelia oculata* just below the surface without a high magnifying-power being used. From the stain which is often seen on the coral on either side of these superficial filamentous organisms, it would appear that they are sometimes broad; but the excavating filaments do not appear to have been broader than they were when they first penetrated or covered the corallum. I have traced the ramifications of these large filaments within the coral by dissolving in weak hydrochloric acid, and they resemble those described by me. They appear to be the same as those which are found in the interlamellar tissue of the septa, and the difference is only in size.

Having had the opportunity of examining some large Foraminifera from the Indian Ocean, I can testify to the presence in them of multitudes of small *Saprolegnia*-looking filaments, but which, like those described by Mr. Moseley, have green contents.

Finally, I have lately discovered that, besides the penetrating planta and spongida of corals, there are long tubular organisms which end in bag-like terminations so greatly resembling some of the calycles of Hydroida that they demand careful investigation. These filaments penetrate and also exist in the previous channels of *Cliona*.

XXVI. "On Volta's Experiment of the Electricity produced by the contact and separation of different Metals." By WARREN DE LA RUE, M.A., D.C.L., F.R.S., and HUGO W. MÜLLER, Ph.D., F.R.S. Received June 7, 1876.

A short time since Prof. Tyndall asked one of us to lend him our Thomson-Becker quadrant electrometer for a lecture illustration of the electricity developed by metallic contact; as the bifilar electrometer in question had been purposely rendered much less sensitive than usual by the wide separation of the suspension-threads, it was thought advisable to test the instrument before sending it. This was done with a disk of copper and a plate of zinc, each fastened to a stick of sealing-wax; the action even with this rough appliance was so great that it seemed desirable to pursue the experiment further.

With this object we had constructed a simple piece of mechanism by Messrs. Elliott Brothers, which enables us to bring together and to separate two disks, one of copper and the other of zinc, each 6 inches in diameter, 400 times in a minute, and after each separation to make the zinc plate touch a spring attached to an insulated conductor; moreover, by means of cams, to make earth-connexion with either disk, or with both, previous to their being brought again into contact.

20 cells of a rod of chloride-of-silver battery charged up on May 27 (part of the 8040 cells now in work) and in perfect action were connected with the quadrant electrometer, so that the silver pole was in metallic contact with the quadrants to be charged, while the zinc pole and the other two quadrants were connected to earth. The deflection (say to the left), three times repeated, was each time 95 divisions of the scale.

When the contact-apparatus was now substituted for the battery, and the insulated conductor of the zinc disk was connected with the same quadrant of the electrometer with which the silver pole of the battery had been connected, and the apparatus worked steadily so as to make 320 makes and breaks of contact in a minute, then to make earth-connexion with the copper disk after each separation of the plates and during the time that the zinc was in metallic connexion with the electrometer, the deviation was to the left as before, and amounted to

	150	divisions of the scale.	
	150	,,	,,
	145	,,	,,
	145	,,	,,
	140	,,	,,
	150	,,	,,
Mean	146·7	,,	,,

so that the tension of the electricity as compared with a chloride-of-silver cell is as

$$\frac{146{\cdot}7 \times 20}{95} = 30{\cdot}88 \text{ to } 1.$$

The copper disk, after its separation from the zinc, acts as a condenser to the latter; and as soon as the copper disk is connected automatically with earth the bound electricity of the zinc is set free, and the needle of the electrometer makes a sudden jump. To observe this effect it is necessary to work the apparatus slowly.

When earth-connexion was made with the copper disk, and also subsequently with the zinc disk after the charge had been given off from it, and before a new contact, no sensible difference was observed in the deflection of the electrometer.

With an ordinary Elliott tangent-galvanometer, and indeed with one twice as sensitive as those generally made, not the slightest deflection of the needle was manifested; with, however, a Thomson galvanometer a deviation of the needle was obtained of 35 divisions in one direction and 35 divisions in the other, according as the zinc conductor was connected with one or other end of the galvanometer and the other end with earth.

The current, though feeble, is quite manifest nevertheless. To form a rough notion of the electromotive force, a piece of copper wire 0·5 inch long and 0·03 inch diameter was connected with one end of the galvanometer, and a piece of zinc 0·25 inch diameter and 0·5 inch long with the other, and the one held between the right-hand finger and thumb, and the other between the left-hand finger and thumb, using $\frac{1}{99}$ shunt in the galvanometer or only $\frac{1}{100}$ part of the current; this produced a deviation in the scale of 50 divisions with dry fingers, and 150 divisions when the fingers and thumbs were moistened; so that the quantity of electricity developed by the contact of dissimilar metals is consequently extremely small when the area (28·27 inches) of each disk is taken into account.

XXVII. "Note on the Mycelium described in my Paper on Smallpox of Sheep." By Dr. E. Klein, F.R.S. Received June 7, 1876.

In the above paper, which was printed in the Philosophical Transactions (vol. 165. pt. 1), I described and figured, in Section iv. part c, the presence, in the lymphatics of the skin of the pock, of what I regarded to be the mycelium of a fungus which I termed *Oidium variolæ* (see figs. 9, 10, and 11 of that paper). Similar features were described and figured in the cavities of the primary and secondary pustules.

My attention has been drawn by Dr. Charles Creighton to appearances, in many respects similar to those described by myself, which he found in

preparations of tissues* altogether removed from the suspicion of containing fungoid growths of that or any other character.

A comparison of the two kinds of specimens convinced me that the appearances represented in my figures 18 and 19 are not due, as I supposed, to a mycelium in the cavities of the primary pustules, but are products of coagulation of some albuminous or kindred material by the reagent that had been employed for hardening the object in question (dilute chromic acid and spirit).

The vegetable nature of the other structures—viz. those represented in figs. 9, 10, and 11 (*i.e.* the supposed mycelium in the lymphatics of the skin of the pock) as well as those in figs. 16 and 17 (*i.e.* the mycelium in the cavities of the secondary pustules)—becomes therefore very doubtful. My doubt as to these being also produced by coagulation is based partly on the similarity between the last-named features and those undoubtedly non-vegetable objects in Dr. Creighton's specimens and also in my figures 18 and 19, and partly on the following circumstances:—(1) I have lately ascertained that blood, especially in febrile conditions, which is contained in blood-vessels of tissues that had been subjected, in a fresh condition, to the hardening fluid (*e. g.* chromic acid) presents appearances very similar to branched mycelium-threads to which are attached numerous conidia; the presence of more or less unaltered blood-corpuscles proves their true character†. (2) I have likewise seen that blood-plasma containing globulin or parts of blood-corpuscles, when in lymphatic vessels or kindred spaces, show sometimes in the course of coagulation similar appearances. Whether the greater number of the thread-like structures is due to fibrin or to blood-corpuscles I cannot determine as yet; but it seems to me that both is the case.

In the case of *Variola ovina* it is therefore probable that the supposed mycelium in the lymphatics is due to coagulation of some substance directly connected with blood. Whether the appearances in the cavities of the pustules, however, owe their origin to the same or to certain mucous substances, as appears in Dr. Creighton's specimens, and under what conditions these substances present the fungus-like characters, is a subject which I intend to investigate more fully.

* Sections through hardened mammary glands.

† Blood-corpuscles, or only portions of them, become fused so as to form longer or shorter thread-like structures, to which are attached smaller or larger particles of blood-corpuscles.

XXVIII. "On the Forms assumed by Drops of Liquids falling vertically on a horizontal Plate." By A. M. Worthington. Communicated by R. B. Clifton, M.A., F.R.S., Professor of Experimental Philosophy in the University of Oxford. Received May 17, 1876.

My attention was first drawn to the subject of this paper last spring, by Mr. H. F. Newall, of the Rugby School Natural-History Society, who showed me the mark made by drops of water and mercury falling on a smoked glass plate, the lampblack being swept away in concentric circles and radial striæ. The patterns thus left were generally symmetrical and beautiful, and varied with the height of fall of the drop. I have since sought to investigate the cause of these appearances in Prof. Helmholtz's laboratory in Berlin.

My first care was to obtain a series of what I will call "patterns" left by drops of various liquids, of various and measured diameters, falling from various and measured heights on horizontal smoked glass plates.

I experimented with water and mercury as types of liquids which do not wet the lampblack, and with alcohol as a type of those which do. Drops of a constant size were obtained with water and alcohol by allowing the liquid to fall, drop by drop, from the end of a vertical capillary tube. In the case of mercury, a narrow vertical glass tube was provided at its upper end with a closed caoutchouc tube; the pressure of the hand on this expelled the air, and a column of mercury, about 60 millims. in length, was drawn up by suction to be expelled when required, drop by drop. The caoutchouc tube was found preferable to a hollow caoutchouc ball, as by wrapping it round the finger the internal volume could be diminished more gradually and regularly than with the ball, and thus the danger of expelling more than the normal-sized drops, whose diameter should depend only on the internal diameter of the tube, was diminished. This danger was, however, only completely avoided in later experiments by using an inclined glass tube, to the lower end of which an open caoutchouc tube was attached, whose upper end could be raised or lowered at pleasure, and the mercury in the U-shaped tube thus formed brought to the level of the mouth of the glass tube, and made to fall over drop by drop. The drops thus obtained were found to be very constant in magnitude. The diameters of the drops were calculated from the weight of 10, 15, 20, or 30. The height of fall was taken as the distance between the plate and the bottom of the vertical or the lower edge of the inclined tube. The glass plates were smoked in the flame of a stearine candle: in cases where the height of fall was great, the adhesion of the smoke was increased by dipping the plate in petroleum or turpentine, and gently wiping before smoking. I thus obtained a large number of patterns; and examination of them showed the extreme difficulty of explaining

from them alone the movements of the liquid which gave rise to them, and the probable uselessness of seeking, while ignorant of these movements, a quantitative connexion in the case of any given liquid between the size or number of rings of the pattern and the constants at my disposal, viz. the height of fall and diameter of the drop.

I preferred to endeavour, by means of the electric spark, to see the forms through which the drop passed in the act of making its pattern. In this I have been tolerably successful.

The principle of the method was to make the drop fall in comparative darkness on the plate, and at the moment of incidence itself to break an electrical circuit, by which means a spark was produced in its neighbourhood sufficiently bright to illuminate the drop and enable it to be seen in the form which it had at that instant; to see the consecutive stages it was necessary to postpone the appearance of the spark for excessively short but increasing intervals of time after the first contact of the drop. The accompanying sketch of the apparatus will explain the details of the method.

Fig. 1.

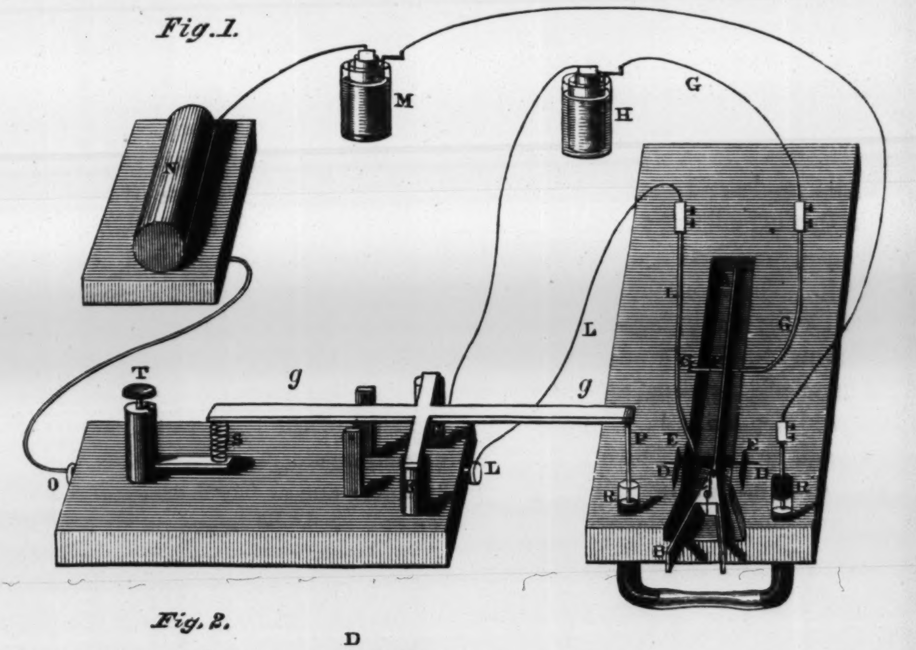


Fig. 2.

A B (fig. 1) is a light wand of cedar wood with a forked end; it is $13\frac{1}{2}$ millims. long, 1 millim. wide, 4 millims. deep. The end B bears the glass plate on which the drop falls, the plate being kept in its place by a spring C, as shown in fig. 2. This wand is fixed on a horizontal

axis D D, made of a fine sewing-needle working in small triangular holes cut in the copper plates E, E; underneath the wand and along it is bound to it a platinum wire, one end of which is bent vertically under the axis, and dips into mercury contained in a hollow in the deal board in which E, E are fixed. The other end of the platinum wire rests, when the wand is horizontal, on a strip of platinum foil F, wound round a copper wire G G. A Bunsen's element (H) sends a current through the coils K, K of a relay, along the wire L L to the mercury in the little trough, along the platinum wire, platinum foil, and wire G G back to H.

The current which passes through the relay comes from 1 or 2 Bunsen's elements, M, through the inducing spiral of a Ruhmkorff's coil, N, into the relay at O, down the platinum wire P, which, when the iron bar *g g* is held down by the magnet, dips into mercury contained in the U tube R R′, so that the current passes out of the mercury at R′ up a thick immersed copper wire and back to the pile M.

The plate of thin glass having been placed on B, the balance of the wand is so adjusted, with a small counterpoise at the other end A, that the slightest downward pressure, even the breath of the observer, is sufficient to raise the end A, and to break the connexion between the platinum wire and foil. Accordingly the moment the drop touches the plate, the current of the pile H is broken at F, the core of K K ceases to be a magnet, and the point of the platinum wire P is pulled out of the mercury at R by the force of the spring S, and the strong primary spark obtained at the surface of the mercury is sufficient to illuminate the drop on the plate. The stage at which it was required to see the drop could be altered at pleasure by altering the depth of immersion of the platinum wire at R, which was done by plunging the connecting wire at R′ more or less deeply into the mercury in that branch of the U tube. This gave a rough adjustment; a finer was obtained by regulating by means of the screw T the tension of the spring S, and so changing the rate of withdrawal of the platinum point.

To secure the complete illumination of the drop and plate, the end R of the U tube and the plate were surrounded with a white cardboard box, with slits to allow of the motion of the wand, the wire P, and the admission of the drop, and open in front, so that the plate could be seen. Complete darkness was found by no means necessary for the experiments; light just sufficient to allow the plate to be seen and the eye easily directed on it was found the most convenient. The results I have obtained have been with mercury and milk. Mercury, from its high reflecting power, is easy to see; water, from its transparency, even when coloured with indigo, very difficult to see; and I substituted milk, which has the advantages of appearing white on a black ground, of being semitransparent, and of showing blue or darkish where it is spread thinly over the black plate, and thus allowing an estimate to be made of the relative thickness of the drop in various places, and especially the advan-

tage of diffusing light through its mass. The mercury, on the contrary, allows no light to penetrate its interior, and for this reason the form of the drop is less easy to ascertain; for the contrast between the brightly illuminated raised or convex parts and the parts that are hollowed or in shadow is so great that it is often uncertain whether the dark portion of the figure is black plate or unilluminated mercury. This was at first a serious cause of error, as in some earlier arrangements which I tried, the light of the spark was reflected laterally from some distance by means of a concave mirror, when the deception was very great, as will be seen by reference to fig. IIα, Set 6. By means of the cardboard box, however, from the surface of which light was diffused from all directions on the plate, and by having the spark close to the plate, I became quite certain of the figures. Owing to the appreciable amount of time required for the demagnetization of the electromagnet, I was not able to obtain figures of the first portion of the spreading out of the drop.

Owing also to accidental causes difficult of control, such as the variations of the contact at F, the variations of the pressure with which the iron bar of the relay was initially pressed down by hand on the ends of the electromagnet, and to the oxidation and irregularities of the surface of the mercury at R, the time of appearance of the spark after the first contact of the drop varied slightly, so that the stages seen with a given depth of immersion of the wire P and a given tension of the spring were not always the same, but varied between narrow limits. But a little judgment enables the observer to tell whether the stage seen is before or after the mean stage most frequently seen with that arrangement of level of mercury and velocity of withdrawal of the platinum wire.

Sets 1, 2, and 4 are figures of milk;

Sets 5, 6, 7, 8, and 9 of mercury.

Explanatory notes are attached where necessary.

The figures suggest a few general observations.

The existence of the radial arms is a deviation from perfect symmetry of figure round the vertical axis; and some slight initial disturbance of symmetry must be required to determine the formation of arms.

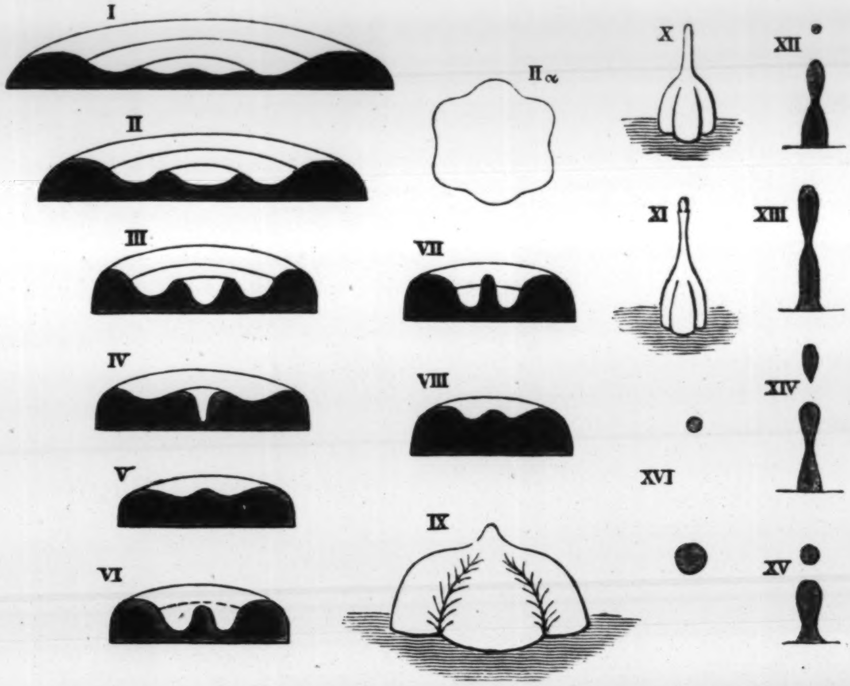
Such a disturbing cause is always at hand in the irregularities of the surface of the glass or of the layer of smoke, which allow the drop to spread with less frictional resistance in one direction than in another, and also in the oscillations of the drop about its mean figure while it falls through the air. The occurrence of such a figure as III, Set 6, confirms this view, as it is an approximation to perfect symmetry when the disturbing cause has been probably very small.

The fact, too, that a very slight dirtiness of the plate was sufficient to cause great irregularities of figure is confirmative of this, as is also the fact that the tendency to form radial arms increases with the height of fall.

In seeking the explanation of the fact that the arms which are first

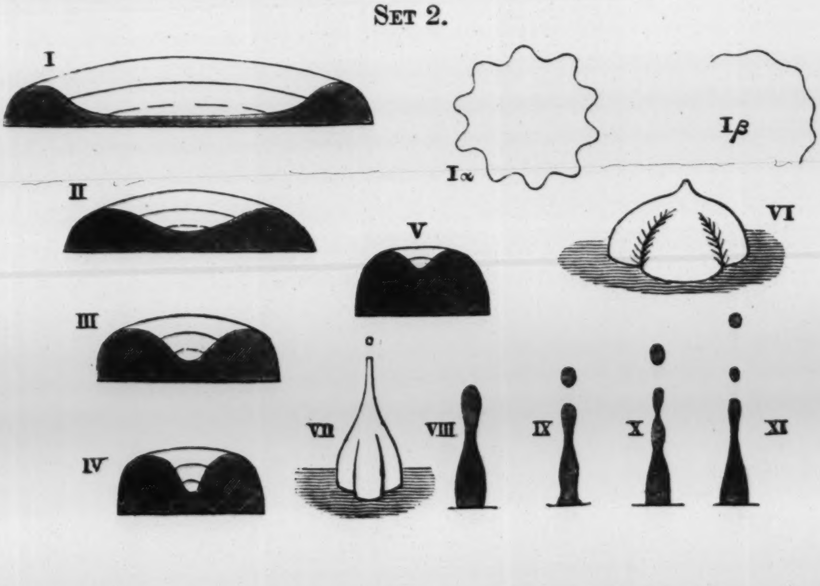
seen when the drop is nearly at its maximum spread contract more slowly than the central part which joins them, we may, I think, leave

SET 1.



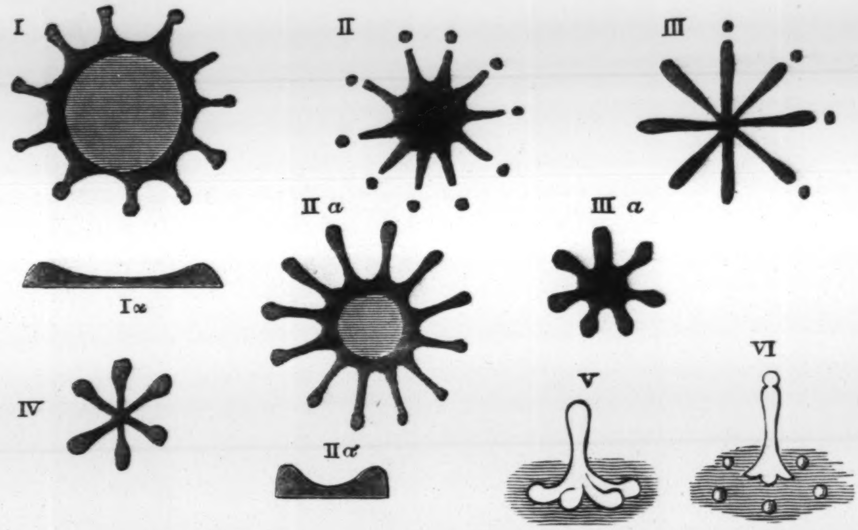
out of consideration the action of gravity on the liquid contained in them, as the duration of their existence is so short that this will not have time to produce an appreciable change of form. The arms, con-

SET 2.



sidered as free cylinders of liquid, will be in equilibrium till the length bears a certain proportion to their diameter, after which they will tend to split each into a row of drops.

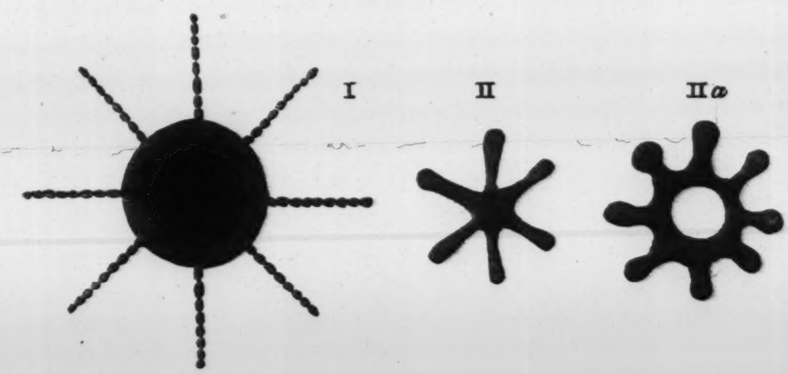
SET 3.



M. Plateau, in his 'Statique expérimentale des Liquides,' has shown that a cylinder of mercury lying on a horizontal plate breaks into drops, whose number depends on the friction between the liquid and the plate.

The pressure of the convex surface at the end of the cylinder will tend to drive the liquid into the cylinder and diminish its length while increasing its thickness, but only so long as the sum of the reciprocals of the principal radii of curvature at any point of the convexity are greater than the reciprocal of the radius of the cylinder.

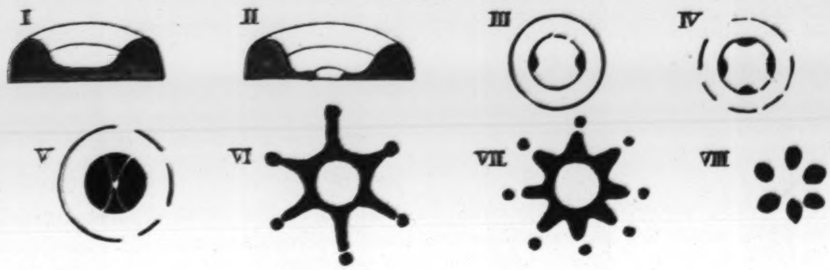
SET 4.



This may, however, always be the case, for the thickness and radius of the cylinder is continually increased by the supply of liquid from the

centre, which keeps contracting under the pressure due to the curvature of its limb.

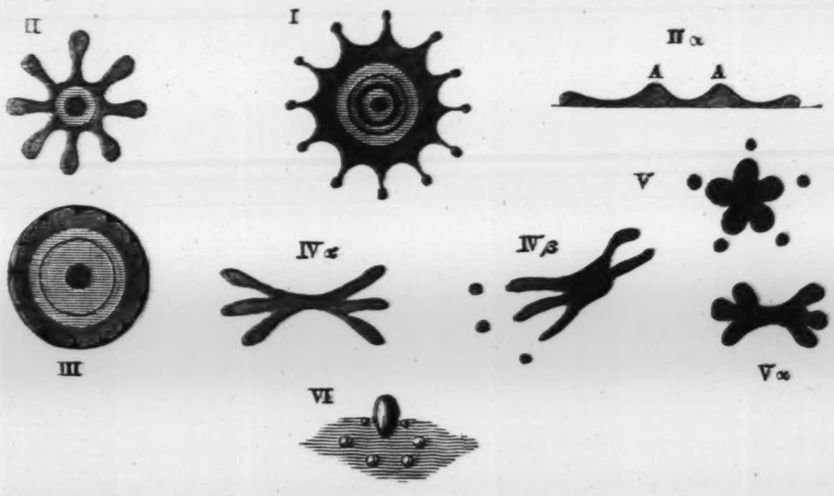
Set 5.



Thus as the centre contracts the arm increases in length.

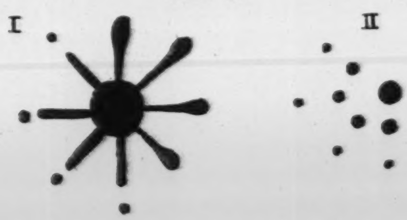
This increase in length causes, after a certain point, a tendency to split into drops. This tendency is counteracted by the thickening of the cylinder from the injection of liquid at the inner end.

Set 6.



This thickening gives efficiency to the curvature of the end of the cylinder, and the arm contracts slowly.

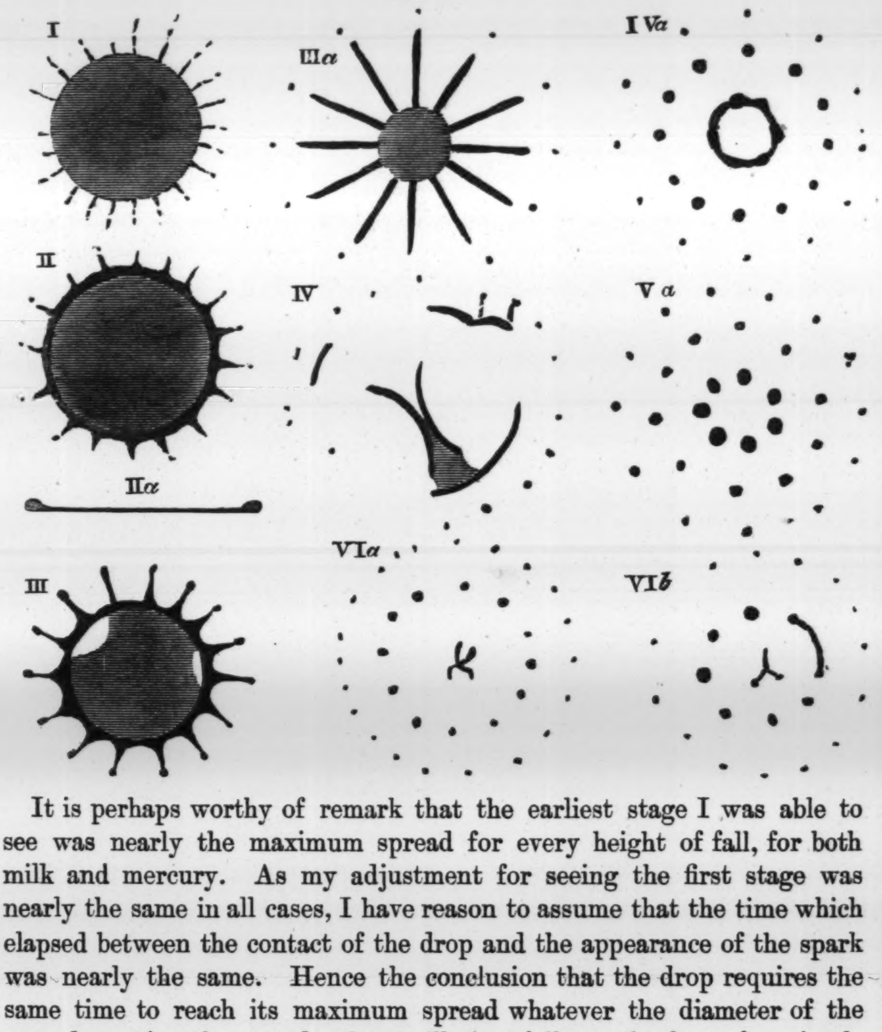
Set 7.



This accounts for our seeing arms whose length is very great compared with their diameter, but which do not succeed in splitting into drops.

When the thickening does not keep pace with the lengthening of the cylinder, drops will split off. That in such case the arms split at the ends rather than in the middle is, I think, to be attributed to the fact that the thickening spreads from the centre to the ends, which receive their additional liquid later.

SET 8.



It is perhaps worthy of remark that the earliest stage I was able to see was nearly the maximum spread for every height of fall, for both milk and mercury. As my adjustment for seeing the first stage was nearly the same in all cases, I have reason to assume that the time which elapsed between the contact of the drop and the appearance of the spark was nearly the same. Hence the conclusion that the drop requires the same time to reach its maximum spread whatever the diameter of the spread; or, in other words, the oscillation follows the law of a simple elastic force. The limits of error are, however, here wide.

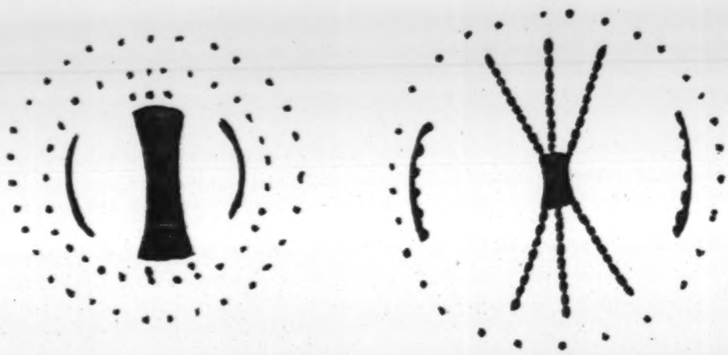
The difficulty of counting the arms was considerable. Most frequently there were 6, sometimes 8 or 12, sometimes more; and I am inclined to think their number was always even.

The number of the drops left from the first spreading out in the case of higher falls was hard to count. In the case of mercury on unsmoked glass, these were left on the plate in a more or less complete circle; and

their number, when the height of fall was 150 millims., was generally 24; but two or three having sometimes run together, made the estimate

SET 9.

I II



uncertain. Incidental disturbing causes seem to alter the number of arms, and to determine the tearing, regularly, irregularly, or not at all, of the central patch in the case of higher falls.

It may be objected that any results with so variable a substance as milk must be unsatisfactory. My object, however, was to study the type of form before proceeding to quantitative measurements. For these I am not likely for some time to have time or opportunity, which is my excuse for presenting my paper as it is, in the hope that other and abler observers may be attracted to the investigation. To the kindness of Prof. Helmholtz, in giving me help and suggestions, much of the success of my experiments is due.

EXPLANATION OF THE FIGURES.

The shaded parts throughout are sections.

SET 1.

Milk on smoked glass. Height of fall 37 millims. Diameter of drop 6·012 millims. The shaded parts are vertical central sections, seen at an angle of about 30°.

I. Rises sometimes wavy, as in IIα.
III. Smaller; central hollow deeper; edge always wavy.
IV. The central hollow now fills up.
IX. The wavy edge visible in the form of lobes as the drop contracts.

SET 2.

Milk on smoked glass. Diam. of drop 6·012 millims. Height of fall 50 millims.

I. More spread out than from lower height. Edge wavy, as Iα or Iβ. Irregular on unevenly smoked glass.
VII. The small detached drop flies upwards, while the remainder rises and splits again as in the remaining consecutive phases.

SET 3.

Milk (boiled) on smoked glass. Diam. of drop 6·012 millims. Height of fall 100 millims.

I. Number of arms uncertain.
Iα. Vertical section of the same between the arms.
IIα & IIIα are phases of an alternative course, sometimes taken instead of II & III.
IV. This phase is consecutive on either III or IIIα.
V. The centre rises before the arms have come in.

SET 4.

Milk on smoked glass. Diam. of drop 6·012 millims. Height of fall 200 millims.

I. Very much spread out; arms beaded, tending to split into rows of drops.
IIα is an alternative phase sometimes seen instead of II; the centre having been torn, the arms contract into the ring thus formed.

Falling from a height of 280 millims. on a smoked glass plate, the same sized drop of milk went through phases similar to those of mercury from 150 millims. (see Set 8).

The succeeding figures represent the forms assumed by a drop of mercury 4·05 millims. in diameter falling on smoked (sometimes on unsmoked) glass from the heights stated.

The first height of fall was 34 millims.

The phases in this case were very similar to those of milk falling from 50 millims. (Set 2), the main difference being that the arms were not so long and did not split off into drops, while the centre was deeply hollowed as in fig. IV. Set 2; after which the central part filled up and rose before the arms came in, as in the case of milk.

From a height of 60 millims. on unsmoked glass the consecutive phases were very similar to those of 34 millims. fall. On smoked glass, however, the arms were longer, and the resemblance to the forms seen in the case of milk was closer than on unsmoked glass.

An alternative course was often taken by the drop, of which the phases are given on

SET 5.

I. The thickness of the central portion is very slight.
II. The thickness diminishes, till the central membrane of liquid tears in the centre and flows to the circumference; or more frequently the tearing takes place under the edge of the outer band, leaving such forms as III, IV, & V, where the white represents the mercury and the black the plate. After this the succeeding stages were as in VI and
VII, where the arms contract into the ring, which splits into drops, as in VIII. These run together. The final stages are like those of milk.

SET 6.

Mercury on unsmoked glass. Height of fall 100 millims.

I. Slight waves on the central patch.
II. This, when illuminated by reflection from a concave mirror at the side, looked like two concentric circles of drops, showing probably that the raised circular band A A seen in the section IIα was lobed where the arms joined it.
III. Sometimes the contour was almost circular, being very slightly lobed.

If the plate was in the least dirty, irregular forms were seen, as in IVα, IVβ, & Vα; otherwise the final stages were very similar to those of milk, such deviations as V and VI being seen.

The same height of fall on smoked glass gave a rather wider spreading out than in I; after which the phases differed only from those depicted in the arms being thinner in the necks, with bulby heads, and the drop finally breaking into three when it rose vertically.

SET 7.

Unsmoked glass. 150 millims.

I. The phase I was seen, after which drops disposed as in fig. II were left on the plate, indicating that the arms from which the small drops split, split a second time, while the rest did not. The figures were of much the same type as when the fall was 100 millims.

A later stage of II, Set 6, was seen, more contracted, and with a complete circle of small drops round it, left from the first spreading out.

SET 8.

Smoked glass. 150 millims.

I. Very much spread out, flat, and uniform, with tendency to irregular small drop-forming arms.

IIα is the central vertical section from rim to rim of II.

III. The central patch begins to tear.

IV. The ring splits off, and the torn central patch runs together into arms; or the alternative course indicated by the next five figures is taken.

IIIα. Sometimes the centre contracted till the arms met. (In the case of milk from 280 millims., whose forms, it has been remarked, were similar to these, the centre invariably contracted till the arms met; the arms were also beaded, as if tending to split into groups along their whole length.)

IVα. The central patch tears open into a ring, into which portions of the arms contract.

Vα. The ring splits into drops.

Later stages showed a general distribution of drops over the plate, rather hard to remember with certainty, even immediately after they were seen, with occasional small arms remaining somewhat as in the figs. VIα & VIβ.

From 255 millims. on unsmoked glass the forms were much the same as from 100 and 150 millims. The arms of fig. II, Set 6, were seen, 6, 8, and 12 in number, and rather longer than there drawn. Occasionally the centre tore, and concentric rings of drops were formed.

SET 9.

Smoked glass. 250 millims.

The phases were generally the same as from 150 millims. (Set 8), with the variations of figs. I & II.

Later, the whole mass of central arms, or thin layer of liquid, split up into fine drops, which rose in a splutter from the plate.

It is to be observed that while on unsmoked glass the type of forms hardly changes while the fall increases from 100 to 250 millims., the same increase of fall on smoked glass is accompanied by very marked alterations in the behaviour of the drop, and that generally the wider spreading out of the drop on smoked glass indicates much less friction than on unsmoked.

XXIX. "On the Behaviour of the Fixed Elements of the Connective-tissue of the Tongue in Inflammation." By GEORGE DOWDESWELL, M.A. Cantab. Communicated by J. BURDON-SANDERSON, F.R.S., Professor of Physiology in University College. Received June 14, 1876.

Presents received, June 15, 1876.

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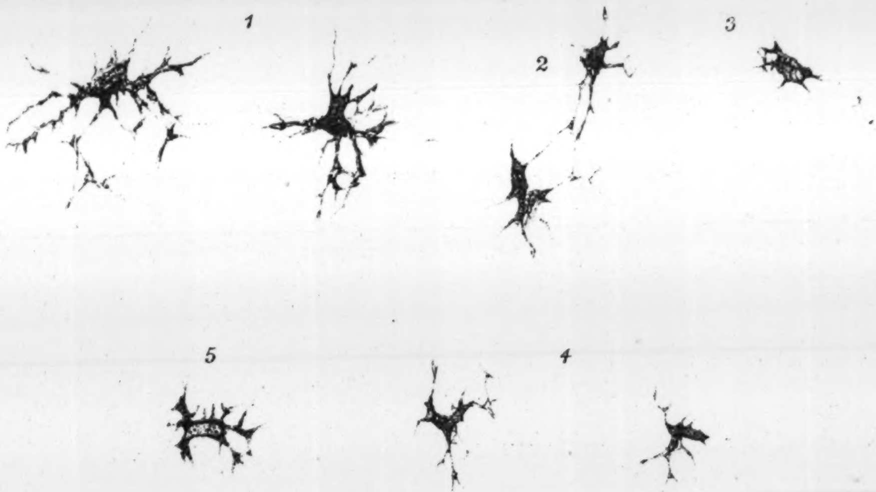
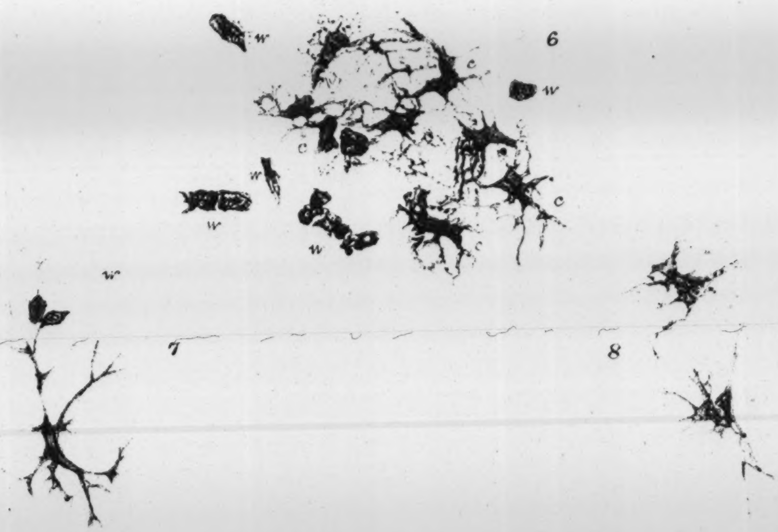
Group I.



Group II.



W. West & Co. lith.

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"On the Effect of Heat on the Chloride, Bromide, and Iodide of Silver." By G. F. Rodwell, F.R.A.S., F.C.S., Science Master in Marlborough College. Communicated by Professor Tyndall, F.R.S. Received March 10, 1876*.

In a former paper† I endeavoured to prove that the iodide of silver is capable of existing in three allotropic modifications, that it possesses a point of maximum density at or about 116° C., and that if a mass of the molten iodide be allowed to cool the following effects may be observed:—(α) At the moment of solidification a very considerable contraction takes place; (β) the solid on further cooling undergoes slight and regular contraction, after the manner of solid bodies in general, until (γ) at or about

* Read May 4, 1876. See *ante*, p. 4. † Proceedings, vol. xxiii. p. 97.

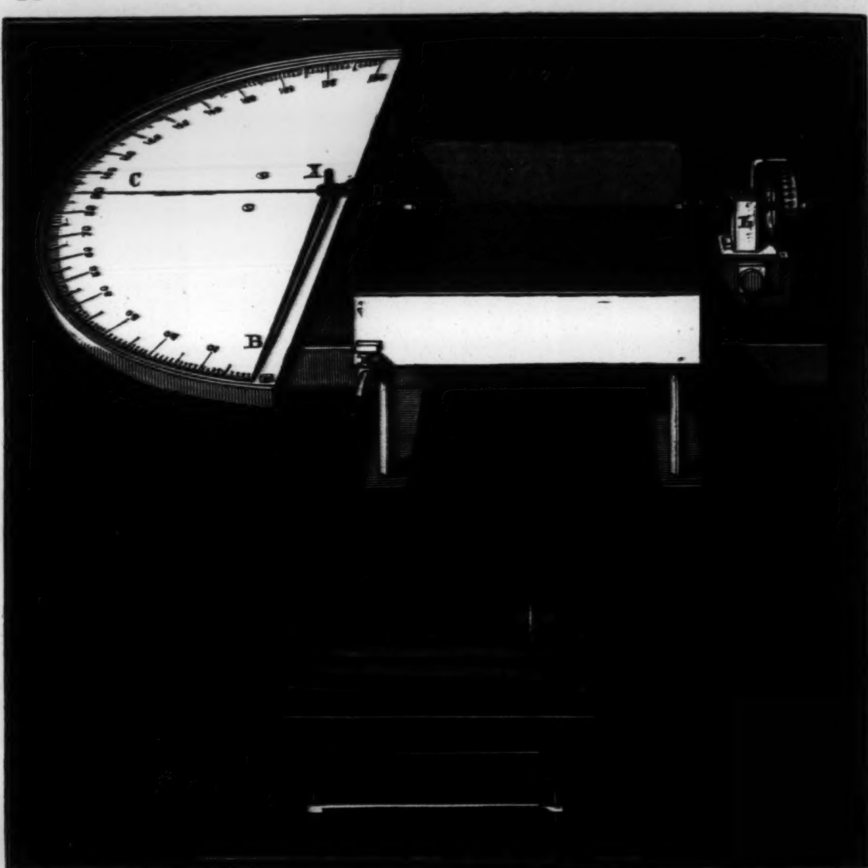
116° C. it undergoes sudden and violent expansion, passing from the amorphous into the crystalline condition; (*d*) after undergoing this expansion the mass, on further cooling, undergoes slight expansion, and (*e*) the coefficient of contraction diminishes as the temperature decreases (or otherwise expressed, the coefficient of contraction augments with the temperature). The following experiments were made in order to examine these effects more minutely, to determine the coefficients of contraction and expansion of the iodide, and to determine the coefficients of expansion of the chloride and bromide of silver between as great extremes of temperature as should be found practicable.

1. *Apparatus employed for the determination of coefficients of expansion or contraction.*

As mercury attacks the iodide of silver at a temperature far below its boiling-point, and as the beautiful optical method of Fizeau does not seem to be capable of application at high temperatures (his own experiments in no case embracing a higher temperature than 100° C.), it was considered advisable to seek for some special method by means of which the degree of expansion and contraction of the iodide could be accurately determined. I finally decided to enormously multiply a small motion by a system of levers, to cause short bars of the iodide, chloride, and bromide to act on these levers while being heated between various temperatures, and to measure the dilatation by a micrometer-screw.

The following apparatus was devised in order to carry out this idea. The box A, fig. 1, p. 282 (drawn to scale), contains the levers, shown in plan and section in figs. 2 and 3; B is an index attached to the axis X, which is in connexion with the final lever, B moving through 180° of arc over the graduated half-circle C. D is a sliding bar, one end of which bears upon the first lever, while the other is in contact with a short rod of glass E, which moves freely, but water-tight, in a metal stuffing-box. The rod of glass thus enters a brass trough F, which can be filled with water or melted paraffine, and which can be heated by means of a Bunsen's burner. The water or paraffine is allowed to run off at the end of an experiment by the tap G. H is a rod of glass similar to E, which moves freely but water-tight in the metal stuffing-box I. Between E and H is placed the rod of substance the expansion or contraction of which we desire to determine. The end of H external to the trough F rests against the end of the micrometer-screw K, which is tipped with agate, and which moves steadily in the support L firmly clamped to the iron bar M, which is screwed to the base of the instrument N. The head of the micrometer-screw, O, is graduated into 250 divisions, and is figured to 500; the graduations are read off against the cross arm P. A plan of the levers is shown in fig. 2, where D (the end of which is seen in fig. 1) is the movable bar, sliding in a socket R and bearing against the first lever S, which is pivoted at T.

The lever S bears against a second lever U, pivoted at V. From the opposite extremity of U a very fine steel chain W, such as is employed



in watches, passes to a vertical axis X, which carries the needle B. The needle is brought back to zero by the pressure of the spring Y against the lever U, and also by a fine steel mainspring Z, which is fixed to the vertical axis X. *a* is a small pin to stop the lever U as soon as the needle has passed the zero-point by a few degrees.

The levers are shown in section in fig. 3; the lettering is the same throughout. The framework *b* is of brass, as are also the levers; the pivots are of steel. Pieces of glass, *c c*, are let into the levers at the bearing points to diminish the friction. The micrometer has threads $\frac{1}{100}$ of an inch apart; hence one turn of the micrometer-head is equal to $\frac{1}{100}$ of an inch, and the movement of the head through one division is equal to $\frac{1}{25000}$ of an inch; but it is quite easy to read to half a division, and hence to $\frac{1}{50000}$ of an inch. The rods inserted between the micrometer and the lever are six inches long, and from one quarter to half an inch in diameter; they rest on light glass rollers placed in the trough F. A rod of the substance to be examined is inserted between E and H;

perfect continuity of the parts is established, and the micrometer-head is turned until the needle stands at zero; the reading on the micrometer is registered, and the head is then turned until the index B has passed to 180° of arc; by again reading the micrometer, the relation between the micrometric divisions and the divisions on the scale of C is established. By this means it was found that a movement of ·0035 inch in the micrometer moves the index through 180° of arc. Now since the index is six inches long, in moving through half a circle its extremity passes over 18·84 inches, and this motion is produced by a movement of ·0035 inch; hence the levers multiply any motion communicated to them 5382 times. The precise value of this index-scale having been ascertained, the determination of the expansion of a body by heat becomes an easy matter, provided that we are careful to secure perfect rigidity in all the fixed parts of the apparatus (by no means an easy task), and are further careful to prevent the conduction of heat from the short glass rods (E and H) to the interior of the apparatus. The coefficient of expansion of E and H must also be ascertained, and allowed for in all determinations. If, on the other hand, we wish to determine the contraction of a body, the index is pushed up to 180° of arc by means of the micrometer-screw, and is pushed back to zero as the bar contracts by the spring Y and the helical mainspring Z. The precise value of the return movement must be, of course, ascertained by means of the micrometer-head. It is obvious that in moving from 0° to 180° of arc the spring Y acts against the motion of the expanding body; while in moving from 180° to 0° it moves with the motion of the contracting body.

A bar of fine homogeneous silver was used in order to test the delicacy of the apparatus. It was placed between the short glass rods (E and H), and cold water was poured into the trough. The temperature was indicated by two thermometers reading well together, placed near the opposite extremities of the silver bar, and the needle was brought to zero. The water was then heated, being constantly stirred to produce uniformity of temperature throughout the whole mass, until the needle had been forced round to 180° of arc. The number of degrees of heat being read off, showed the heat necessary to produce a certain known expansion, and from this the coefficient of expansion was easily ascertained. Six results gave 10° C., 10°, 9°, 10°, 10°, 9°·5. The coefficient deduced from the mean of the determinations was for 1° C.

·0000193083.

Now Lavoisier gives it as ·00001910; Daniell ·00001951; Fizeau (determined by the same method as that which he applied to the iodide, chloride, and bromide of silver) gives ·00001921; and more recently Matthiessen (Phil. Trans. 1866) found it to be ·000019436.

Rods of other metals gave the following results:—

	Expansion-apparatus.	Various observers.
Iron..........	·000011025	·00001182 ·00001194
Copper........	·0000174433	·00001666 ·00001678 ·00001722
Lead...........	·0000302121	·00002924
Zinc	·0000288761	·00002918

It is thus abundantly evident that the apparatus is capable of very considerable accuracy, especially when we remember that we are dealing with rods of metal only six inches long. I venture to think that such an apparatus would prove a useful adjunct to a Physical Laboratory; for it would not only afford a means of determining coefficients of expansion with rapidity and accuracy, but it might be used for determining thicknesses in thousandths and ten-thousandths of an inch. For if the substance were introduced between the agate face of the micrometer-screw and the glass rod H, and if the index were then driven round to a certain point (say 20° of arc), and so with the other substances tested, an exact uniformity of pressure of the face of the screw on the substance would be secured, and minute thicknesses might thus be measured with accuracy.

In constructing such an apparatus, extreme rigidity of the fixed parts is a main necessity; the base should be of thick marble, and the micrometer-screw support should be deeply let into it and firmly secured. Massive supports, apparently fixed with the utmost firmness, sometimes yield with surprising readiness to the extent of one five-hundredth of an inch. The elasticity of the metal sometimes causes great inconveniences. The levers must be firm and strong, but not unnecessarily heavy; they should work very smoothly. Agate should be let into the brass at the points of contact. Care should be taken to avoid any transference of heat from the hot trough to other parts of the apparatus. By the use of a paraffine of high boiling-point, the temperature of a bar submitted to heat in the trough (F) can be raised to 300° C. Under such circumstances great care is requisite to avoid heat from being radiated or otherwise communicated to other parts of the apparatus.

2. *Means employed to determine the expansion in passing from the solid to the liquid condition.*

The determination of the amount of expansion which iodide, chloride, and bromide undergo in passing from the solid to the liquid condition was effected in the following manner:—A conical tube of platinum weighing 44·844 grammes was carefully filled with mercury at a known temperature. It contained 105·43 grammes of mercury. It was then filled with the substance under examination at its melting-

point; the substance was allowed to solidify, and the whole was weighed. The cavity due to the contraction of the mass in solidifying was then filled with mercury to the level of the mouth of the conical tube, and was weighed. Then, knowing the capacity of the tube, the specific gravity of mercury and of the substance under examination, the temperature of the fusing-point, and the coefficient of expansion of the platinum cone, we have all the data requisite for the determination. The real difficulty is the determination of temperatures above the boiling-point of mercury; and until a trustworthy method of general application has been devised, such determinations must be regarded as approximations. The expressions "below a red heat," "a dull red heat," &c. are still common in text-books and in memoirs; but the very definitions of what is meant by a "dull red heat" vary, as also do the temperatures assigned to it by different writers.

3. *Effects of Heat on the Iodide of Silver.*

Iodide of silver, carefully prepared by precipitating the nitrate by means of iodide of potassium and subsequent drying*, was fused in a porcelain crucible and cast in warm tubes of glass. The cylinders thus cast were from $\frac{3}{8}$ to $\frac{1}{2}$ an inch diameter; they were levelled at the end by a fine steel saw, and were reduced to 6 inches in length. Every precaution was adopted in order to secure homogeneity; but the rifts, both longitudinal and horizontal, produced during the sudden expansion of the substance in passing from the amorphous to the crystalline condition sometimes rendered the bar so weak and brittle that it had to be recast. I have before pointed out that as a bar of molten iodide cools it contracts regularly to its point of maximum density, then expands considerably just below the temperature of maximum density, passing simultaneously into its crystalline condition, and finally continues to slightly expand as the temperature falls. Its behaviour in the expansion-apparatus may consequently be readily inferred; and experiment justified the inference.

A bar of the iodide was placed between the movable glass rods (E H, fig. 1), resting on glass rollers; water was placed in the trough, and the thermometers were put in their places. The index was placed up to 180° of arc, and the water was heated. As the temperature rose the needle gradually returned towards zero as the bar contracted; and the amount of contraction was measured and compared with the range of temperature. When the needle reached zero, the bar was allowed to cool, and the needle now moved forward, indicating expansion. The water was now replaced by a paraffine of high boiling-point, and the heating was recommenced. The index (at 180° of arc) began to retreat as before, showing that the bar was contracting; as 140° C. was approached the

* I must express my great indebtedness to Mr. Valentin for allowing me to have a quantity of the iodide, bromide, and chloride prepared at South Kensington, and to Mr. Greenaway for preparing it.

contraction became more rapid; and between 142° and 145°·5 C. very rapid contraction took place, and the temperature was maintained as steady as possible. A slight fall of the temperature below 142° C. caused the needle to stop, and the contraction was resumed as soon as the temperature again rose above that point. When the temperature was kept steadily between 142° and 145°·5 C., the contraction after a while ceased at the latter temperature; and on raising the temperature the mass now expanded slowly up to 300° C., the limit of the apparatus, as beyond this the paraffine ignited. In cooling these effects were exactly reversed: the index moved from 180° of arc to zero between 300° and 142° C., that is, the mass contracted like an ordinary solid; then at 142° a considerable expansion occurred, after which the mass slowly expanded as the temperature fell.

Fizeau, "by a method depending on the accuracy with which extremely minute movements can be appreciated, by observing the changes they produce in a system of Newton's rings," determined the coefficient of cubical contraction of iodide of silver between + 70° C. and − 10° C., and found it to be ·00000417. The determinations made with the expansion-apparatus were higher than this; but as M. Fizeau's method is undoubtedly one of great accuracy, and as, moreover, the determinations of the expansion of silver, copper, lead, zinc, and of the chloride and bromide of silver made by means of the expansion-apparatus agree very closely with those made by M. Fizeau by his optical method, I have preferred to adopt his coefficient for temperatures between − 10° C. and + 70° C. Between 70° C. and 142° C., at which temperature the considerable expansion occurs, the coefficient was found to be ·00001749; the determination is apt to be vitiated by the sudden increase in the coefficient as the temperature of rapid contraction is approached. Between 142° C. and 145°·5 C. the mass contracts to the extent of ·004500; probably this contraction occurs at one precise temperature within even a less range than 3°·5 C.; but it is practically impossible to ensure an absolutely uniform temperature in a bar of iodide in a bath of paraffine under the conditions of the experiment. Between 145°·5 C. and 300° C. the coefficient was found to be ·00002844. The iodide fuses at a temperature which I estimate at 450° C. By fusion in the platinum cone, and the application of the method described above, the volume in the liquid state at 450° C. was found to exceed the volume at 450° C. in the solid state to the extent of ·046331 on volume at 450° C. in the solid state = 1·008659. On raising the temperature of the platinum cone and its contents to a temperature estimated at 750° C. (a cherry-red heat visible in broad daylight), the molten iodide did not appear to expand more than the platinum; the coefficient between 450° C. and 750° C. has been hence taken as that of platinum as an approximation. As no method could be devised for determining the coefficient of expansion between 300° C. and 450° C., the same coefficient as that

determined for the range between 145°·5 C. and 300° C. has been adopted. Finally, as to the coefficient of contraction between − 10° C. and − 60° C. Fizeau asserts that he believes the point of maximum volume or minimum density of the solid iodide to be at −60° C. He does not give the coefficient below −10° C., and presumably could not apply his method to so low a temperature as −60° C. Neither did an attempt with solid carbonic acid and ether prove satisfactory in the expansion-apparatus. Hence, as an approximation, the coefficient has been taken as ·00000104, calculated on the presumption that the coefficient between −60° and −10° C. decreases in the same ratio as the coefficient between + 70° C. and 142° C. increases, in reference in each case to the coefficient for temperatures between −10° C. and +70° C. Summarizing the above results, we obtain the following as an approximate statement of the changes in volume undergone by a mass of iodide of silver in cooling down from 750° C. to −60° C. More than this we fear we cannot say until high temperatures can be satisfactorily measured, and until certain experimental difficulties in connexion with the determinations can be overcome.

Volume at	750° C.	(liquid)	=	1·052946	Contraction on cooling, expansion on heating.
„	„ 450	(liquid)	=	1·044990	
„	„ 450	(solid)	=	1·008659	
„	„ 142	(maximum density) ...	=	1·000000	
„	„ 145·5	(after sudden expansion)	=	1·015750	Expansion on cooling, contraction on heating.
„	„ +70	....	=	1·017009	
„	„ −10	....	=	1·017342	
„	„ −60	(? minimum density) ..	=	1·017394	

In my former paper I mentioned 116° C. as the temperature at or about which the iodide undergoes its sudden change of volume, and at which it possesses its maximum density. But this assertion was founded on the fact that a rod of iodide in cooling in a glass tube breaks the tube at or about that temperature. It is obvious, however, that the tube would yield before breaking; and the expansion-apparatus clearly proved that the change takes place at a higher temperature—a temperature at or very near to 142° C. At this temperature, both in cooling and heating, the index reversed its motion. Wernicke (Pogg. Ann. cxliii. p. 560) mentions the fact that prisms of fused iodide of silver when cooled to 138° C. exhibit a sudden alteration of colour and transparency.

Molten iodide solidifies to a perfectly transparent, very flexible claret-coloured solid; as it cools it becomes amber-coloured, and just above 142° C. it becomes pale yellow; at 142° C. the change from the amorphous to the crystalline condition takes place, the body simultaneously becoming crystalline and opaque and undergoing considerable expansion. It now exists as a brittle pale green solid. On heating this to a temperature of 300° C. it recovers some of its plasticity, and may again be

bent, but it cannot be made transparent without re-fusing. By rapidly casting a bar and introducing it into the trough of the expansion-apparatus filled with paraffine above 200° C., the expansion of the transparent amorphous iodide was determined, and this gave ·00001206. Wernicke considers that at a high temperature "part of the iodine is separated from its combination with the silver, and is absorbed by the remaining substance in the liquid state, for the spectrum, like that of solid and liquid iodine, contains no blue or violet light. In the normal state, below 138° C., silver iodide gives a spectrum less bright, but twice as long, and particularly developed in the blue-violet portion."

It is well known that red, transparent, plastic sulphur in becoming yellow, opaque, and crystalline gives out a good deal of heat. Transparent plastic iodide of silver was examined at the moment of change, and was found to give out heat. A mass of the plastic iodide at 200° C. was placed in paraffine at 200° C., and the cooling was observed. When the iodide began to change from the amorphous to the crystalline condition the rate of cooling (previously determined) was far less rapid, proving clearly that heat was given out.

The iodide appeared to become more brittle with successive fusions; but this did not seem to materially affect its specific gravity. Thus a specimen which had been many times fused gave a sp. gr. of 5·675, while another specimen after a single fusion gave 5·66. Taking the former as the true sp. gr., we find that the sp. gr. at the maximum density (142° C.) will be 5·771, and at the minimum density (— 60° C.) 5·673; while the sp. gr. of the molten iodide at 450° C. will be 5·522.

It was noticed that the platinum cone was altered in capacity when fused iodide of silver was allowed to cool in it; in fact it became more or less deformed. This was found to be due to the considerable expansive force exercised by the iodide in expanding. We did not succeed in breaking an iron bottle by this means, but a thick porcelain tube was easily broken by the passage of the iodide from the amorphous to the crystalline condition.

An attempt to determine the relative conductivities of the iodide, bromide, and chloride of silver was made in the following manner:—Homogeneous rods of each of these substances were taken in weights corresponding to their specific heats. Any one rod whose conductivity was to be determined was fixed in the bottom of a copper trough, the projecting end dipped into a small reservoir of mercury in contact with a delicate thermopile and galvanometer. Great care was taken to surround every thing with non-conductors of heat, and to insulate the rod from contact with the copper trough. Paraffine at 300° C. was poured into the trough, and thus came into contact with the exposed end of the bar. The time which elapsed before the movement of the galvanometer took place was noted in seconds. The results were not satisfactory; but they appeared to prove that the bromide of silver conducts heat better

than the chloride, and more than twice as well as the iodide. We should naturally expect the iodide from its comparatively loose crystalline nature to conduct heat less well than either the bromide or chloride.

4. *Effects of Heat on the Bromide of Silver.*

Precipitated bromide of silver was fused, and cast in warm glass tubes. The mass when warm was found to be somewhat tenacious; the surface of the fused rod was smooth and brilliant, the fracture crystalline. Repeated fusion seemed to render the substance more crystalline. Although crystalline the substance was very compact, and altogether unlike the crystalline condition of the iodide; the rod contracted a good deal in cooling, and easily came out of the tube in which it was fused. Rods 6 inches long by $\frac{1}{4}$ inch diameter had their ends levelled by means of a fine steel saw, and were placed in the expansion-apparatus, and tested as described in the case of the iodide bars. The coefficient of cubical expansion for 1° C. was found to be

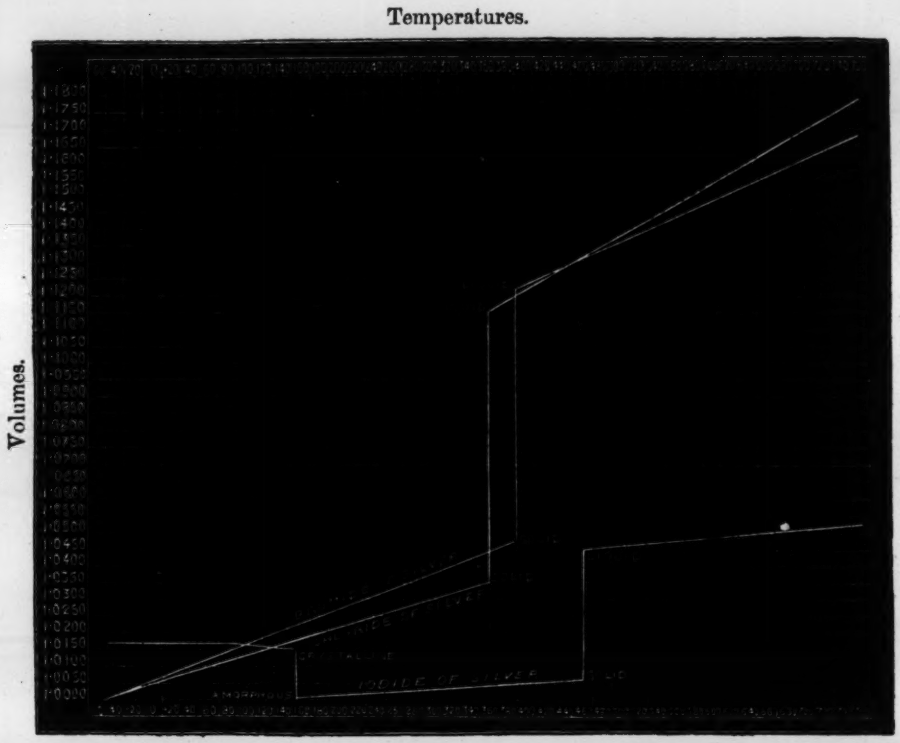
·00010500.

Fizeau by his optical method found the coefficient to be ·000104061. The coefficient increases with the temperature, at least to the extent of ·000004 for each 100° C. increase of range. The expansion is very considerable in passing from the solid to the liquid condition, and between 380° (which I believe to be near its melting-point) and a temperature estimated at 750° C. The volume at − 60° C. has been taken as unity in order for better comparison with the iodide in the accompanying Curve Table (p. 290), and an addition of ·000004 to the coefficient has been made for each 100° C. of temperature. Then if we suppose a molten mass of bromide of silver to be cooling down from 750° C. to − 60° C., the following is an approximation to the volumes of the mass at the various temperatures indicated:—

Volume at	750° C.		....	= 1·167940
„ „	380	(liquid)	....	= 1·122840
„ „	380	(solid)	....	= 1·048120
„ „	300		....	= 1·038760
„ „	200		....	= 1·027460
„ „	100		....	= 1·016560
„ „	0		....	= 1·006060
„ „	−60		....	= 1·000000

The specific gravity at 7° C. was found to be 6·245 in the case of a specimen which had been often fused, and 6·293 in the case of a specimen which had been only once fused. This would give 5·595 as the sp. gr. of the molten bromide at the fusing-point.

Table showing approximately the Action of Heat on the Chloride, Bromide, and Iodide of Silver, between $-60°$ C. *and* $750°$ C.



The bromide fuses to a reddish brown liquid, not unlike bromine, and it solidifies to a bright yellow transparent solid when seen in thin layers. In thick layers it appears to be brownish yellow. It is brittle even before it is quite cold. During the process of cooling loud cracking noises, like the cracking of a piece of porcelain or thick glass, are produced. The solidified solid produces a metallic ring when struck; but this is less marked than in the case of the chloride of silver.

5. *Effects of Heat on the Chloride of Silver.*

Precipitated chloride of silver was fused and cast in warm glass tubes. It had been very carefully prepared in the dark, and the fused mass in thin layers was colourless and quite transparent. When warm, a rod of the chloride was very flexible, and to some extent malleable; when cold the bar did not bend without fracture, and repeated fusings seemed to render it more liable to fracture. The rod contracted a good deal on cooling, and easily came out of the tube in which it was cast. Rods of 6 inches long by $\frac{1}{4}$ inch in diameter had their ends levelled by means of a fine steel saw, and were placed in the expansion-apparatus and tested as

described in the case of the iodide bars. The coefficient of cubical expansion for 1° C. was found to be

·000095454.

Fizeau by his optical method found the coefficient to be ·000098814. The coefficient increases with the temperature, at least to the extent of ·000003 for each 100° C. increase of range. The expansion is considerable in passing from the solid to the liquid condition, and between 350° C. and a temperature estimated at 750° C. I believe 350° C. to be near the melting-point of the chloride. (Can the 260° C. which we see in books be a misprint for 360° C.? The chloride may be kept for any length of time in a paraffine-bath at 300° C. without showing signs of fusion.) The volume at — 60° C. has been taken as unity, in order that a better comparison between the chloride and the iodide may be made in the curve table, and an addition of ·000003 to the coefficient has been made for each 100° C. of temperature. Then, if we suppose a molten mass of chloride of silver to be cooling from 750° C. to — 60° C., the following is an approximation to the volumes of the mass at the various temperatures indicated:—

Volume at	750° C.	....	= 1·177135
„	„ 350	(liquid)	= 1·116427
„	„ 350	(solid)	= 1·040302
„	„ 300	....	= 1·035082
„	„ 200	....	= 1·024937
„	+100	....	= 1·015092
„	„ 0	....	= 1·005547
„	„ —60	....	= 1·000000

The specific gravity was found to be 5·505 in the case of a specimen which had been often fused, and 5·405 in the case of a specimen which had been once fused. This (former) would give 4·957 as the sp. gr. of the molten chloride at the fusing-point.

The chloride fuses to a dull light-red liquid, and becomes faintly yellow when solidified, in thin layers colourless and quite transparent. When cold it forms a pale dirty greenish-yellow solid, transparent in thin layers. No sounds were emitted during cooling. The fracture is crystalline. In thin layers the chloride is very flexible when cold; and when hot thick masses of it are very flexible. When cold it has a decided metallic ring when struck.

"On the Effects of Heat on some Chloro-brom-iodides of Silver." By G. F. Rodwell, F.R.A.S., F.C.S., Science Master in Marlborough College. Communicated by Frederick Guthrie, F.R.S., Professor of Physics in the Royal School of Mines. Received April 13, 1876*.

In a recent communication to the Society I have given the approximate coefficients of expansions of the chloride and bromide of silver, and the coefficients of contraction and expansion of the iodide of silver. It was thought that some interesting results might be obtained by alloying these bodies together, and thus forming various chloro-brom-iodides of silver, and by investigating the physical properties of such bodies and the effects of heat upon them. Accordingly these bodies were fused together in the proportions requisite to form the following compounds:—

$$Ag\,I,\quad Ag_2\,Br_2,\quad Ag_2\,Cl_2.$$
$$Ag\,I,\quad Ag\,Br,\quad Ag\,Cl.$$
$$Ag_2\,I_2,\quad Ag\,Br,\quad Ag\,Cl.$$
$$Ag_3\,I_3,\quad Ag\,Br,\quad Ag\,Cl.$$
$$Ag_4\,I_4,\quad Ag\,Br,\quad Ag\,Cl.$$

Dr. Matthiessen ("On Alloys," Chem. Soc. Journ. 1867, p. 201) states that he believes "in nearly all cases the two metal alloys may be considered as solidified solutions of the one metal in the other;" and he continues as follows:—"By the term solidified solution I mean a solution of two substances which has been allowed to solidify, as, for instance, if a mixture of ether and alcohol were made, and sufficient cold could be produced to solidify it, we should produce a solidified solution of these two substances in one another. Again, if the chlorides of potassium and sodium, say in equal parts, be melted together and allowed to solidify, the solid thus produced is a solidified solution of the chlorides of potassium and sodium in one another. Glass is also a good example of a solidified solution; to produce it, different silicates are fused together and allowed to solidify. There is, however, an important point in the definition of the term 'solidified solution' which must not be overlooked—namely, that the components are most intimately mixed together; in fact they are homogeneously diffused in one another, and to that extent that, even in the most powerful microscope, it would not be possible to distinguish the components of a solidified solution. As examples of this fact glass may be quoted, which presents under high magnifying-power a homogeneous mass; the silver and gold in the gold-silver alloys cannot be distinguished by the same test from one another."

Accepting Dr. Matthiessen's definition, we must regard the chlorobrom-iodides of silver as solidified solutions of chloride, bromide, and

* Read May 4, 1876. See *ante*, p. 4.

iodide of silver in one another. Such bodies are found native: *embolite* (ἐμβόλιον) is a chloro-bromide of silver in which the ratio of the chloride to the bromide varies indefinitely. Minerals having respectively the composition $Ag_3\,Br\,Cl_2$, $Ag_5\,Br_2\,Cl_3$, $Ag_4\,Br_3\,Cl$, $Ag_9\,Br_4\,Cl_5$, and $Ag_4\,Br\,Cl_3$ have been analyzed by Domeyko, Field, Müller, Richter, and others. They occur chiefly in Chili, and constitute the principal ore of the silver-mines of Chañarcillo. They are described as possessing specific gravities which vary between 5·75 and 6·2; and according to Dana the colour is "greyish green, and asparagus-green to pistachio or yellowish green, and yellow; often dark, becoming darker externally on exposure." Dana further states that an iodobromide of silver is found native in Chili; but of this I am unable to find any description.

In examining the following results, we must bear in mind that we are dealing with bodies which are very differently affected by heat. For while the chloride and bromide of silver have higher coefficients of expansion than the most expansible metals (such as zinc), the iodide of silver contracts slightly when heated to a temperature of 142° C., while between 142° and 145°·5 C. it undergoes considerable contraction; then expands to the melting-point, undergoes considerable increase of volume in passing from the solid to the liquid condition, and expands slightly beyond this temperature and the melting-point. Moreover the iodide passes into an amorphous condition between 142° C. and 145°·5 C., and possesses a point of maximum density at 142° C. The following volumes are given for comparison with those of the alloys (the coefficients for both the bromide and chloride are not given because they are practically the same, and one serves for both):—

Bromide of Silver. ° C.	Volume.	*Iodide of Silver.* ° C.	Volume.	
At 750	= 1·167940	At 750 (liquid)	= 1·052946	Contraction on cooling, expansion on heating.
380 (liquid)...	= 1·122840	450 (liquid)	= 1·044990	
383 (solid) ...	= 1·048120	450 (solid)............	= 1·008659	
300	= 1·038760	142 (max. density)	= 1·000000	
200	= 1·027460	145·5 (aft. sudden ex.)	= 1·015750	Expansion on cooling, contraction on heating.
+ 100	= 1·016560	+ 70	= 1·017009	
0	= 1·006060	− 10	= 1·017342	
− 60	= 1·000000	− 60	= 1·017394	

The alloys were examined in the same manner as I have previously described in determining the coefficients of expansion of their constituents*.

They were cast into rods 8 inches long by $\frac{1}{4}$ to $\frac{3}{8}$ inch diameter in warm glass tubes; then by means of a fine steel saw they were cut into lengths of 6 inches, and examined in the expansion-apparatus described and

* "On the Effects of Heat on the Chloride, Bromide, and Iodide of Silver," see *ante*, p. 280.

figured in the above-mentioned paper. The measurements were made by means of a micrometer-screw. The expansions above the point of fusion were determined by the method of the platinum cone described in the previous paper.

The alloys were made by fusing together in a porcelain crucible weighed quantities of the iodide, bromide, and chloride of silver in such proportions as furnished the five compounds described below.

1. CHLORO-BROM-IODIDE OF SILVER HAVING THE COMPOSITION $AgI\,Ag_2Br_2\,Ag_2Cl_2$, OR $Ag_5IBr_2Cl_2$.

The alloy contains:—

Ag I ...	= 26·1692	Ag ..	= 60·1336
Ag Br ..	= 41·8708	I	= 14·1435
Ag Cl ..	= 31·9600	Br ..	= 17·8176
		Cl ..	= 7·9053
	100·0000		100·0000

Specific gravity 6·152, when fused and cast into rods which were allowed to cool in the air; but when the rods were allowed to cool slowly in hot paraffine, the specific gravity was found to be 6·066. The specific gravity, calculated on the assumption that no change of volume takes place, was found to be 5·836, showing a condensation equal to ·0513 on the calculated volume. Fusing-point 330° C. Specific gravity at the fusing-point = 5·5118; at 750° C. = 5·057. The mass fused to a claret-red liquid, which became brick-red, dull orange, and yellow as it cooled, and when cold had a brownish-yellow colour, a good deal resembling bromide of silver. The mass contracted on solidifying, and formed a substance with crystalline fracture, not perfectly homogeneous. A small central core of less dense matter appeared near the upper end of the rod, and was formed during the contraction of the mass. The alloy gave a bright yellow powder, which turned *green* on exposure to light. Loud harsh sounds were sometimes emitted during the cooling of the mass. The substance was somewhat brittle, and broke as easily as a rod of bromide of silver of the same dimensions. Heated in paraffine to 250° C., it was found to be incapable of bending, and was as brittle as when cold. In fracture and general characteristics it closely resembled the bromide of silver.

Placed in the expansion-apparatus the bar expanded regularly up to 125°·5 C., and more rapidly than the chloride or bromide of silver; between 125°·5 C. and 131°·5 C. a slight contraction took place; at 131°·5 the mass began to expand again, and it expanded more rapidly than the chloride or bromide; at the melting-point and at 750° C., however, the volume was nearly the same as that of the bromide. The following results were obtained:—

Coefficient of cubical expansion for 1° C.

	°C.		°C.		
Between	0	and	125·5	=	·00012216
"	125·5	and	131·5 (contraction)	=	·00004902
"	131·5	and	fusing-point (330° C.)	=	·00015882
Expansion in passing from the solid to the liquid state				=	·057390
Between 330° C. and 750° C.				=	·0001760

If we take the volume at 0° C. as unity we have—

	°C.	
Volume at	0	= 1·000000
"	125·5	= 1·015331
"	131·5	= 1·015037
"	330	= 1·046666 (solid)
"	330	= 1·104050 (liquid)
"	750	= 1·177979

The alloy clearly possesses two points of similar density at different temperatures, the one at 131°·5 C., the other at or about 123° C.

2. Chloro-brom-iodide of silver having the composition Ag I Ag Br Ag Cl, or Ag_3 I Br Cl.

The alloy contains :—

Ag I....	41·484	Ag	57·1932
Ag Br ..	33·186	I......	21·4184
Ag Cl ..	25·330	Br	14·1218
		Cl	6·2666
	100·000		100·0000

Specific gravity 6·1197. Calculated specific gravity on the assumption that no condensation takes place = 5·801, showing a condensation equal to ·0519 on the calculated volume. Fusing-point 295° C. Specific gravity of the liquid at the fusing-point = 5·5673; at 750° C. = 5·118. The mass fuses to a dark bromine-red liquid, becoming a solid of the same colour, which changes to a pink, dull opaque brick-red, and finally when the mass is cold to a dull orange. The powder is bright orange, becoming bright green on exposure to light. The fused mass on exposure to light becomes of a dark steel-grey colour. The mass is compact, hard, and homogeneous; it is semitransparent in thin layers. Semicrystalline fracture. Somewhat lustrous at the surface. Gives a clear metallic ring when allowed to fall on an anvil, or when short rods of the alloy are shaken together. It is difficult to break, and has more tenacity than any one of its constituents. It does not bend when cold; and taken from the paraffine-bath at 250° C. it bends slightly, but breaks easily.

In the expansion-apparatus the bar expanded as regularly but not quite so rapidly as the alloy No. 1. Up to 124° C. the coefficient of expansion is nearly the same as that of the bromide of silver. Between 124° C. and 133° C. it contracted more than the preceding; at 133° C. the rod began to expand again, and it expanded now both more than the bromide and more than alloy No. 1 during the same ranges of temperature. At the melting-point the volume is less than that of the bromide, however, and at 750° C. it is nearly the same.

The following results were obtained:—

Coefficient of cubical expansion for 1° *C.*

	°C. °C.	
Between	0 and 100	= ·00009529
„	100 and 124	= ·00010451
„	124 and 133 (contraction)	= ·00060000
„	133 and fusing-point (295° C.)	= ·00020250
Expansion in passing from the solid to the liquid state		= ·05084000
Between 295° C. and 750° C.		= ·00016130

It is curious and anomalous that the coefficient of expansion of the liquid between 295° C. and 750° C. should be less than that of the solid between 133° C. and 295° C.; but the results were concordant, and it must be noted that the expansion between 133° C. and 295° C. is nearly double that of the most expansible of metals. The coefficient between these limits appeared to decrease as the temperature rose; but as the mass, or at least one of its constituents, undergoes at 133° C. a molecular change, passing into an amorphous plastic condition, and as of necessity there is some strain on the rod, it was thought that this decrease of the coefficient might be due to increase of plasticity and consequent slight yielding of the bar; and the first determination (that is to say at the lowest temperature possible above 133° C.) was taken, and the above coefficient, which may consequently be somewhat too high, was deduced from it.

If we take the volume at 0° C. as unity we have—

	°C.	
Volume at	0	= 1·000000
„	100	= 1·009529
„	124	= 0·012037
„	133	= 1·006637
„	295	= 1·039442 (solid)
„	295	= 1·090280 (liquid)
„	750	= 1·163720

The alloy has two points of similar density at different temperatures, owing to the contraction which takes place between 124° and 133° C. The one temperature is 133° C., the other at or about 70° C.

3. Chloro-brom-iodide of silver having the composition $Ag_2 I_2 Ag Br Ag Cl$, or $Ag_4 I_2 Br Cl$.

The alloy contains:—

Ag I	58·6404	Ag	53·8989
Ag Br ..	23·4557	I......	31·6905
Ag Cl	17·9039	Br	9·9813
		Cl	4·4293
	100·0000		100·0000

Specific gravity 6·503; after annealing by slow cooling in paraffine 5·997. Calculated specific gravity on the assumption that no condensation takes place = 5·762, showing a condensation equal to ·0487 on the calculated volume. Fusing-point 320° C. Specific gravity of the liquid at the fusing-point = 5·6971; at 750° C. = 5·3749. Fused to a dark bromine-red liquid, which, after passing through the different changes of colour as No. 2 alloy, finally cooled to a dark orange-coloured opaque solid. Both the exterior of the fused mass and the bright orange-coloured powdered substance turned green on exposure to diffused light. The mass contracted on cooling. Taken from the paraffine-bath at 250° C. it was found to be flexible, and it could be bent through an angle of 40° before breaking; when somewhat cooler it was brittle and easily broken, but when cold it was tenacious and difficult to break. It was compact and homogeneous, and gave a clear metallic ring when allowed to fall on an anvil. In the expansion-apparatus the bar expanded regularly, but much less rapidly than Nos. 1 and 2, up to 124° C. Between 124° C. and 133° C. it contracted considerably more than No. 2 alloy; at 133° C. it began to expand, and between 133° C. and 320° C. it expanded at the same rate as No. 2. At the melting-point and at 750° C. the volume was less than that of either of the preceding.

The following results were obtained:—

Coefficient of cubical expansion for 1° *C.*

	° C. ° C.	
Between	0 and 124........................	= ·00008307
,,	124 and 133 (contracting).............	= ·00189999
,,	133 and fusing-point (320° C.)..........	= ·00020250
Expansion in passing from the solid to the liquid state		= ·02771500
Between 320° C. and 750° C.	....	= ·00012390

If we take the volume at 0° C. as unity we have—

	° C.		
Volume at	0	=	1·000000
,,	124	=	1·010301
,,	133	=	·993201
,,	320	=	1·031068 (solid)
,,	320	=	1·058783 (liquid)
,,	750	=	1·112020

It will be seen that this alloy has two temperatures of maximum density or minimum volume, the one 133° C., the other about −84° C., if we assume that the coefficient of expansion is the same between − 100° C. and 0° C. as it is between 0° C. and 100° C.

4. Chloro-brom-iodide of silver having the composition $Ag_3\ I_3\ Ag\ Br\ Ag\ Cl$, or $Ag_5\ I_3\ Br\ Cl$.

The alloy contains:—

Ag I	68·0171	Ag	52·0984
Ag Br ..	18·1379	I	36·7583
Ag Cl ..	13·8450	Br	7·7183
		Cl	3·4250
	100·0000		100·0000

Specific gravity 5·9717. Calculated specific gravity on the assumption that no condensation takes place = 5·741, showing a condensation equal to ·0385 on the calculated volume. Fusing-point 330° C. Specific gravity of the liquid at the fusing-point = 5·643; at 750° C. = 5·333. Fused to a dark bromine-red liquid, and passed through the same changes of colour as alloy No. 3, finally cooled to a dull orange solid. Lustrous. Turned green on exposure to light. More brittle and less compact than the preceding. *Expanded* in cooling and broke the glass tube in which it was cast during the cooling, but not vigorously. A few longitudinal rifts appeared in the rod. At 250° C. sufficiently flexible to be bent through more than a right angle, but was brittle when cold. In the expansion-apparatus the bar expanded up to 124° C. to a less extent than the preceding; between 124° C. and 133° C. it contracted to a greater extent than the preceding; at 133° C. it commenced to expand again, and between 130° C. and 330° C. it expanded less than the preceding. At the melting-point and at 750° C. the volume was less than that of any of the preceding.

The following results were obtained:—

Coefficient of cubical expansion for 1° *C.*

	° C. ° C.	
Between	0 and 124	= ·00006000
„	124 and 133 (contraction)	= ·00259998
„	133 and 330	= ·00011571
Expansion on passing from the solid to the liquid state		= ·048033
Between 330° C. and 750° C.	....	= ·00012359

If we take the volume at 0° C. as unity we have—

	° C.		
Volume at	0	=	1·000000
„	124	=	1·007440
„	133	=	·984041

	°C		
Volume at	330	=	1·006834 (solid)
„	330	=	1·054867 (liquid)
„	750	=	1·106782

This alloy, like the preceding, has obviously two temperatures of maximum density or minimum volume; the one 133° C., the other at some point far below zero.

5. Chloro-brom-iodide of silver having the composition $Ag_4 I_4$ $Ag Br Ag Cl$, or $Ag_6 I_4 Br Cl$.

The alloy contains :—

Ag I....	73·9285	Ag	50·9634
Ag Br ..	14·7856	I......	39·9528
Ag Cl ..	11·2859	Br	6·2919
		Cl	2·7919
	100·0000		100·0000

Specific gravity = 5·907. Calculated specific gravity on the assumption that no condensation takes place = 5·725, showing a condensation equal 0·291 on the calculated volume. Fusing-point 350° C. Specific gravity of the liquid at the fusing-point = 5·680; at 750° C. = 5·340. Fuses to a bromine-red liquid, which cools to a mass of the same colour. As the mass cools it becomes bright brick-red, dull brick-red, orange-red, and finally, when cold, a rich orange-yellow. It turns green both in mass and in powder on exposure to light. It expands in solidifying, and cracks the tube in which it is cast as vigorously as the iodide of silver itself. It forms a brittle solid when cold, and possesses a number of horizontal rifts produced at the moment of expansion. More brittle than any of the preceding compounds, but less so than Ag I. Crystalline fracture. Lustrous surface. Harsh crystalline noises during cooling. Taken from the paraffine-bath at 250° C., it was so plastic that it could not only be bent upon itself, but twisted like a corkscrew. In the expansion-apparatus the bar expanded up to 124° C. to a less extent than the preceding; between 124° C. and 133° C. it contracted to a greater extent than the preceding; at 133° C. it commenced to expand again, and between 133° and 350° C. it expanded to a less extent than the preceding. At the melting-point and at 750° C. the volume was less than that of any of the preceding.

The following results were obtained :—

Coefficient of cubical expansion for 1° C.

	°C. °C.	
Between	0 and 124	·00005400
„	124 and 133 (contraction)	·00270000
„	133 and 350	·00010800
Expansion on passing from the solid to the liquid state		·03414100
Between 350° and 750° C.		·00014379

If we take the volume at 0° C. as unity we have—

	°C.		
Volume at	0	=	1·000000
”	124	=	1·006696
”	133	=	·979696
”	350	=	1·003132 (solid)
”	350	=	1·037273 (liquid)
”	750	=	1·094790

This alloy, like the preceding, has two temperatures of maximum density or minimum volume; the one at 133° C., the other at some point far below zero.

Table showing approximately the Action of Heat on some Chloro-bromiodides of Silver, between 0° C. *and* 750° C.

The experimental results obtained with the last two alloys were less consonant than those of the other alloys, which might be predicted from the nature of the alloys in question.

General Conclusions.—There are several questions connected with the chloro-bromo-iodides of silver which require to be discussed, and it may be well to take them under separate headings.

Comparison of the alloys with their constituents.—For all purposes of these comparisons we may take the bromide and chloride of silver together, since their coefficients of expansion and certain other relations to heat are very much the same. It will be noticed that the first alloy contains only 26 per cent. iodide of silver, while the four succeeding alloys contain respectively 41, 58, 68, and 74 per cent. If we compare the percentage of silver we find :—No. 1, 60 per cent.; No. 2, 57; No. 3, 54; No. 4, 52; and No. 5, 51: or, again, in No. 1 we have 14 per cent. of iodine to 25 of Br and Cl; in No. 2, 21 of I to 20 of Br and Cl; in No. 3, 31 of I to 14 of Br and Cl; in No. 4, 36 of I to 11 of Br and Cl; and in No. 5, 40 of I to 8 of Br and Cl. The first alloy is scarcely affected at all as regards its coefficients of expansion by the presence of the iodide, and, in fact, resembles bromide of silver in all its properties; on the other hand, the alloys Nos. 4 and 5 are very much affected by the presence of the large amount of iodide of silver they contain, and in many respects resemble the iodide. The greatest divergence from the properties of the constituents is to be found in the alloys Nos. 2 and 3, in which the iodide varies between 40 and 60 per cent. Perhaps this is due to the fact that the iodide only dissolves to a certain extent in the fused bromide and chloride; for we notice that certain properties of the iodide are masked so long as the iodide does not exceed a certain percentage, while they become very apparent as the amount of iodide is increased.

Of the point of maximum density of the alloys.—While the bromide and chloride of silver expand regularly like any ordinary solid, it has been shown that the iodide contracts slightly up to 142° C., considerably between 142° C. and 145°·5 C., and that it possesses its point of maximum density at the latter temperature. Now nothing could possibly be more definite or decided than the behaviour of the alloys at the critical temperature at which contraction commences during the heating of the mass. In the case of the alloys Nos. 2, 3, 4, and 5, this contraction invariably commenced at 124° C., and invariably finished at 133° C. In the case of No. 1 alloy, in which the percentage of iodide of silver was smallest, the contraction began at 125°·5 C. (1°·5 C. higher than the others). The action took place with great precision in every instance. Here, then, we have the curious fact that while the iodide of silver commences its considerable contraction (which occurs simultaneously with its passage from the brittle crystalline state into the plastic amorphous state) at 142° C. and finishes it at 145°·5 C., the chloro-brom-iodide alloys commence their contraction 18° C. lower, and end it 12°·5 C. lower. Thus in the iodide it is effected in the heating through 3°·5 C., while in the alloy it requires 9° C. We must remember that in the alloy the iodide passes into the amorphous condition while it is disseminated through the mass of the bromide and

chloride; and perhaps the same cause as that which lowers the fusing-point lowers also the point of maximum density.

Fusing-point.—While the fusing-point of iodide of silver has been estimated at 450° C., of bromide at 380° C., and of chloride at 350° C., that of the alloys is lower than any of the constituents (except No. 5, which is the same as that of the lowest of its constituents, while it contains 74 per cent. of the constituent with the highest fusing-point, viz. 450° C.). Thus No. 1 melts at 330° C., No. 2 at 295° C., No. 3 at 320° C., No. 4 at 330° C., and No. 5 at 350° C. The most distinctive alloy, No. 2, melts at a temperature which is 155° C. below that of the iodide which constitutes 41·5 per cent. of the weight of the alloy, 85° C. below that of the bromide which constitutes 33 per cent. of the alloy, and 55° C. below that of the chloride which constitutes 25 per cent. of the alloy. Now it is well known that in the case of numerous alloys the fusing-point is lower than that of the mean fusing-points of the components; further, it is known that a mixture of the fused chlorides of sodium and potassium has a lower fusing-point than the mean of the constituent salts. Dr. Matthiessen says, "It is generally admitted that matter in the solid state exhibits excess of attraction over repulsion, whilst in the liquid state these forces are balanced; and in the gaseous state repulsion predominates over attraction." Let us assume that similar particles of matter attract each other more powerfully than dissimilar ones. It will then follow that the attraction subsisting between the particles of a mixture will be sooner overcome by repulsion than in the case of a homogeneous body: hence mixtures should fuse more readily than their constituents. We are at least reminded of the fact that certain perfectly inert bodies, when mixed with substances which decompose at a certain temperature, lower the temperature of decomposition.

Of the contraction of the alloys between 124° *C. and* 133° *C.*—It is a curious fact that until the percentage of iodide of silver in the alloy becomes considerable, the chief influence of the iodide seems to be exerted between that narrow range of temperature; and more than this, that so soon as the contraction is over, the mass undergoes far more rapid expansion than do any of its constituents when heated through the same range of temperature. It is further noticeable that the amount of contraction in some of the alloys exceeds that of the iodide itself, while we know that the other constituents possess high coefficients of expansion. This is all dependent, without doubt, upon the manner in which the iodide changes its condition within the mass of the alloy. Let us take the case of one of the intermediate alloys, say No. 3; in every 100 molecules between the temperatures of 124° C. and 133° C. we have 58 molecules undergoing somewhat rapid contraction, while 42 are undergoing expansion; at the same time other events are taking place within the mass, heat is disappearing as internal work, and is changing the crystalline into the amorphous iodide, converting an opaque, brittle,

highly crystalline body (I speak of the iodide *alone*, not of the alloy) into a transparent, plastic, denser body. What the precise function of the molecular motion which disappears can be it is difficult to assume, since in this case it not only changes the state of the body, but approximates its molecules.

Of the texture, specific gravity, &c. of the alloys.—It is noticeable that when the percentage of iodide of silver is small, the alloy is brittle while hot, and only slightly more tenacious than its constituents when cold (No. 1); as the percentage of iodide increases, the alloy becomes somewhat less brittle while hot, and considerably more tenacious, hard, and compact, than any of its constituents (Nos. 2 & 3); while, when the percentage of iodide becomes considerable (Nos. 4 & 5), the mass becomes extremely plastic while hot, perhaps more so than the iodide itself, and very brittle when cold. The specific gravity is in all cases *above* the mean of that of the constituents; it may be because the intercrystalline spaces of the iodide are now filled with bromide and chloride. Thus, while the sp. gr. of $AgCl$ is 5·505, of $AgBr$ 6·245, and of AgI 5·675, that of the alloys is as follows:—No. 1, 6·152; No. 2, 6·1197; No. 3, 6·503; No. 4, 5·9717; and No. 5, 5·907: while the percentage of the bromide, which alone has a higher specific gravity than that of the alloys, in no case exceeds 42.

In the accompanying curve table (p. 300) the expansion-curves of the iodide and bromide of silver have been added for comparison with those of the alloys; the curve of chloride of silver has been omitted, because it is almost precisely the same as that of the bromide.

I have preferred to call these results "approximate" on account of certain experimental difficulties in the way of very precise determinations, which difficulties I at present see no way of avoiding.

highly crystalline body (I speak of the iodide *alone*, not of the alloy) into a transparent, plastic, denser body. What the precise function of the molecular motion which disappears can be it is difficult to assume, since in this case it not only changes the state of the body, but approximates its molecules.

Of the texture, specific gravity, &c. of the alloys.—It is noticeable that when the percentage of iodide of silver is small, the alloy is brittle while hot, and only slightly more tenacious than its constituents when cold (No. 1); as the percentage of iodide increases, the alloy becomes somewhat less brittle while hot, and considerably more tenacious, hard, and compact, than any of its constituents (Nos. 2 & 3); while when the percentage of iodide becomes considerable (Nos. 4 & 5), the mass becomes extremely plastic while hot, perhaps more so than the iodide itself, and very brittle when cold. The specific gravity is in all cases *above* the mean of that of the constituents; it may be because the intercrystalline spaces of the iodide are now filled with bromide and chloride. Thus, while the sp. gr. of $AgCl$ is 5·505, of $AgBr$ 6·245, and of AgI 5·675, that of the alloys is as follows:—No. 1, 6·152; No. 2, 6·1197; No. 3, 6·503; No. 4, 5·9717; and No. 5, 5·907: while the percentage of the bromide, which alone has a higher specific gravity than that of the alloys, in no case exceeds 42.

In the accompanying curve table (p. 300) the expansion-curves of the iodide and bromide of silver have been added for comparison with those of the alloys; the curve of chloride of silver has been omitted, because it is almost precisely the same as that of the bromide.

I have preferred to call these results "approximate" on account of certain experimental difficulties in the way of very precise determinations, which difficulties I at present see no way of avoiding.

November 16, 1876.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Dr. Henry Edward Armstrong and Capt. George Strong Nares were admitted into the Society.

Prof. W. G. Adams, Mr. Bramwell, Mr. Busk, Dr. Russell, and General

Smythe, having been nominated by the President, were elected by ballot Auditors of the Treasurer's Accounts on the part of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Experimental Contributions to the Theory of the Radiometer."—Preliminary Notice. By WILLIAM CROOKES, F.R.S. &c. Received November 15, 1876.

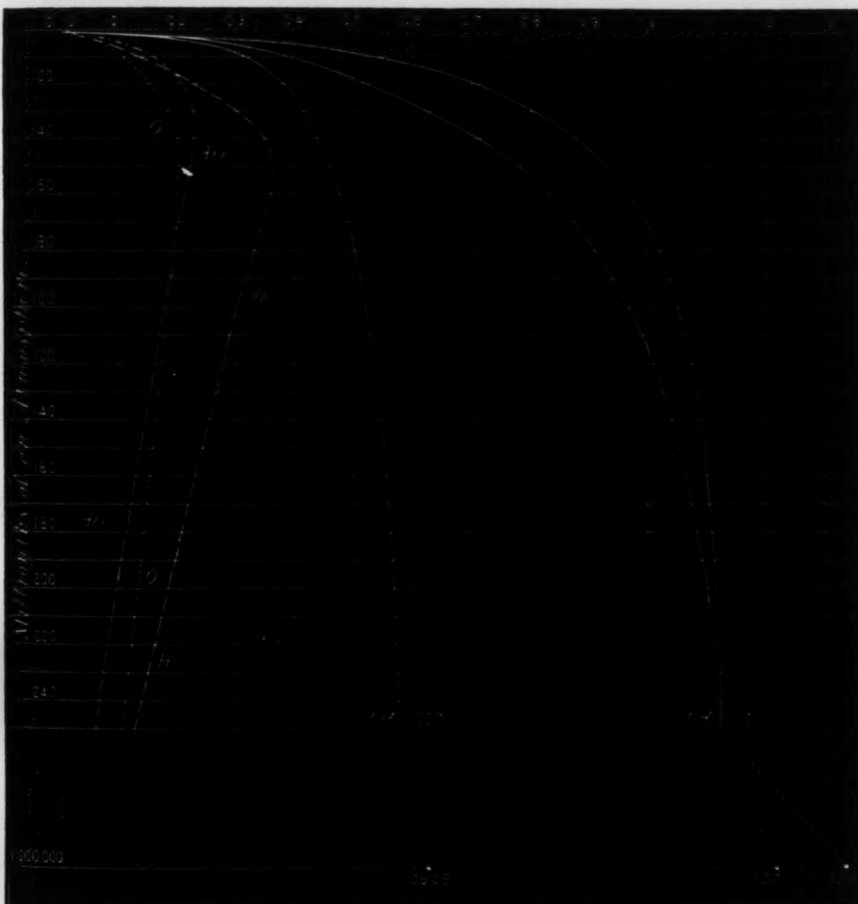
Instead of bringing another preliminary notice before the Society, I should have preferred reserving the announcement of my new results on the Repulsion resulting from Radiation until they were fit to be offered in a more complete form; but the radiometer is now so much occupying the attention of scientific men, and results of experiments with this and allied instruments are appearing so frequently in the scientific journals at home and abroad, that were I not to adopt this method of bringing the results of my more recent experiments before men of science, I might find myself anticipated in some or all of the conclusions at which I have arrived.

On June 15th last I mentioned to the Society that the repulsion resulting from radiation increases up to a certain point as I exhaust the air from the torsion-apparatus. After long-continued exhaustion the force of radiation approaches a maximum, and then begins to fall off. I have since succeeded in experimenting at still higher exhaustions, and with different gases in the apparatus; and by means of a McLeod gauge attached to the mercury pump I have been able to measure the atmospheric pressure at any desired stage of exhaustion. I have not only measured the force of repulsion, but also the viscosity of the residual gas; and from the results I have plotted the observations in curves which accompany this paper, and which show how the viscosity of the residual gas is related to the force of repulsion exerted by radiation. These curves must not, however, be considered as representing more than the broad facts, for I have not included in them my final observations, which in all probability will introduce modifications in them.

In plotting these curves I have supposed my scale to be 1000 metres long, and to represent one atmosphere. Halfway up the scale therefore, or 500 metres, represents half an atmosphere; 999 metres up the scale represents an exhaustion of $\frac{1}{1000}$ of an atmosphere: each millimetre, therefore, stands for the millionth of an atmosphere.

My results have principally been obtained at the top of the scale; and it is the last quarter of a metre which supplies the diagrams accompanying this paper.

When the residual gas is air, the viscosity (measured by the logarithmic decrement of the arc of oscillation) is practically constant up to an exhaustion of 250 millionths of an atmosphere, or 0·19 millim. of mercury, having only diminished from 0·126 at the normal pressure of the atmosphere to 0·112. It now begins to fall off: at 200 millionths it is 0·110, at 100 millionths it is 0·096, at 50 millionths it is 0·078, at 20 millionths it is 0·052, at 10 millionths it is 0·035, and at 0·1 of a millionth of an



atmosphere the log. dec. has fallen to about 0·01. Simultaneously with this decrease in the viscosity, the force of repulsion exerted on a black surface by a standard light varies. It increases very slowly till the exhaustion has risen to about 70 millionths of an atmosphere; at about 40 millionths the force is at its maximum; and it then sinks very rapidly, till at 0·1 millionth of an atmosphere it is less than one tenth of its maximum. On continuing the curves of the log. dec. and the force of radiation, and assuming that the torsion-fibre of glass has no viscosity, it is most probable that

they both would come to zero when the last traces of an atmosphere had been taken out of the apparatus.

The oxygen diagram differs from that of air. The log. dec. is 0·126 at the atmospheric pressure; it falls to 0·111 at a pressure of 250 millionths of an atmosphere; at 100 millionths it is 0·105, at 50 millionths it is 0·093, at 20 millionths it is 0·068, and at 2 millionths it is 0·02. The force of repulsion in oxygen increases very steadily up to an exhaustion of about 40 millionths of an atmosphere; it is at its maximum at about 30 millionths, and thence declines very rapidly.

Hydrogen gives a remarkable diagram. The viscosity at the normal pressure is measured by a log. dec. of 0·063; at 250 millionths of an atmosphere it is 0·057, at 100 millionths it is 0·052, at 50 millionths it is 0·046, whence it rapidly sinks. The force of repulsion increases slowly up to an exhaustion of 250 millionths, then quickly until it attains its maximum at about 50 millionths, and it then rapidly declines. The force of repulsion is very great in a hydrogen vacuum, being in comparison with the maximum in an air vacuum as 70 to 41. Neither is it necessary to get so high an exhaustion with hydrogen as with other gases to obtain considerable repulsion. This shows that in the construction of radiometers it is advantageous to fill them with hydrogen before exhausting.

Carbonic acid has a viscosity of about ·01 at the normal pressure, being between air and hydrogen, but nearer the former. On approaching a vacuum, the force of repulsion does not rise very high, and soon falls off.

Before working with this apparatus I thought that monohydrated sulphuric acid evolved no vapour, and I therefore freely used it for cleaning out the pump and for drying the gases. I can even now detect no vapour-tension; but a comparison of the curves, with and without sulphuric acid, shows that the presence of this body modifies the results. One of my curves represents the action of the residual sulphuric anhydride gas. The experience thus gained has led me to adopt phosphoric anhydride for drying the gases. I can detect no ill effects from the presence of this agent; and I have been able in consequence to push the rarefaction to higher points than before.

The McLeod gauge will not show the presence of mercury vapour. It is therefore possible that I have a greater pressure in the apparatus than is here stated. I have, however, entirely failed to detect the presence of mercury vapour at any great distance from the mercury in the pump; and the tube packed with gold-leaf, which I frequently interpose between the pump and the apparatus, shows no trace of bleaching, and exerts no appreciable effect one way or the other on the results.

With this pump, assisted sometimes by chemical absorption, it is not

difficult to exhaust a radiometer to such a point that it will not move to a candle placed a few inches off; but I have not yet succeeded in stopping the movement of the beam in the torsion-apparatus.

A long series of observations have been taken, at different degrees of exhaustion, on the conductivity of the residual gas to the spark from an induction-coil. Working with air, I find that at a pressure of about 40 millionths of an atmosphere, when the repulsive force is near its maximum, a spark, whose striking distance at the normal pressure is half an inch, will illuminate a tube having aluminium terminals 3 millimetres apart. When I push the exhaustion further, the $\frac{1}{2}$-inch spark ceases to pass; but a 1-inch spark will still illuminate the tube. As I get nearer to a vacuum more power is required to drive the spark through the tube; but at the highest exhaustions I can still get indications of conductivity when an induction-coil actuated with five Grove's cells, and capable of giving a 6-inch spark, is used.

When so powerful a spark is employed there is great danger of perforating the glass, thus causing a very slight leakage of air into the apparatus. The log. dec. now slowly rises, the repulsive force of the candle increases to its maximum, and then slowly diminishes to zero, the log. dec. continuing to rise till it shows that the internal and external pressures are identical. With a fine perforation several days are occupied in going through these phases, and they take place with such slowness and regularity as to afford opportunities for getting valuable observations.

The improvements now added by Mr. Gimingham to the pump render it so easy to obtain high exhaustions that, in preparing experimental radiometers, I prefer to exhaust direct to one or two millionths of an atmosphere. By keeping the apparatus during this exhaustion in a hot-air bath heated to about 300° C. for some hours, the occluded gases are driven off from the interior surface of the glass and the fly of the radiometer. The whole is then allowed to cool, and attenuated air from the air-trap is put in in small quantities at a time, until the McLeod gauge shows that the best exhaustion for sensitiveness is reached; if necessary, this point is also ascertained by testing with a candle. Working in this way, I can now do in a few hours what formerly required as many days. In this manner, employing hydrogen instead of air for the gaseous residue, and using roasted mica vanes set at an angle with the axis, as described further on, I can get very considerably increased sensitiveness in radiometers. I am still unable, however, to get them to move in moonlight. The statements made by an observer nearly a year ago, that he obtained strong rotation by moonlight, must therefore be considered erroneous. My most sensitive torsion-balance will, however, move easily to moonlight.

The above-mentioned facts, in addition to what has already been pub-

lished, leave no reasonable doubt that the presence of residual gas* is the cause of the movement of the radiometer. But few theories are sufficiently strong not to require reinforcement; and in the present case very much remains to be ascertained as regards the mode of action of the residual gas. The explanation, as given by Mr. Johnstone Stoney, appears to me the most probable; and having stood almost every experimental test to which I have submitted it, I may assume for the present that it expresses the truth. According to this the repulsion is due to the internal movements of the molecules of the residual gas. When the mean length of path between successive collisions of the molecules is small compared with the dimensions of the vessel, the molecules rebounding from the heated surface, and therefore moving with an extra velocity, help to keep back the more slowly moving molecules which are advancing towards the heated surface; it thus happens that though the individual kicks against the heated surface are increased in strength in consequence of the heating, yet the number of molecules struck is diminished in the same proportion, so that there is equilibrium on the two sides of the disk, even though the temperatures of the faces are unequal. But when the exhaustion is carried to so high a point that the molecules are sufficiently few and the mean length of path between their successive collisions is comparable with the dimensions of the vessel, the swiftly moving, rebounding molecules spend their force, in part or in whole, on the sides of the vessel, and the onward crowding, more slowly moving molecules are not kept back as before, so that the number which strike the warmer face approaches to, and in the limit equals, the number which strike the back, cooler face; and as the individual impacts are stronger on the warmer than on the cooler face, pressure is produced, causing the warmer face to retreat.

I have tried many experiments with the view of putting this theory to a decisive test. The repulsive force being due to a reaction between the fly and the glass case of a radiometer, it follows that, other things being equal, the fly should revolve faster in a small bulb than in a large one. This cannot well be tested with two different radiometers, as the weight of the fly and the amount of friction would not be the same in each; but I have constructed a double radiometer which shows this fact in a very satisfactory manner. It consists of two bulbs, one large and the other small, blown together so as to have a wide passage between them. In the centre of each bulb is a cup, held in its place by a glass rod, and in

* It is a question whether the residual gas in the apparatus, when so highly attenuated as to have lost the greater part of its viscosity, and to be capable of acquiring molécular movement palpable enough to overcome the inertia of a plate of metal, should not be considered to have got beyond the gaseous state, and to have assumed a fourth state of matter, in which its properties are as far removed from those of a gas as this is from a liquid.

the bulbs is a small four-armed fly with roasted mica disks blacked on one side. The fly can be balanced on either cup. In the smaller bulb there is about a quarter of an inch between the vanes and the glass, whilst in the larger cup there is a space of half an inch. The mean of several experiments shows that in the small bulb the fly rotates about 50 per cent. faster than in the large bulb, when exposed to the same source of light.

One of the arms of another radiometer was furnished with roasted mica disks blacked on alternate sides. The other arm was furnished with clear mica disks. The two arms were pivoted independently of each other, and one of them was furnished with a minute fragment of iron, so that by means of a magnet I could bring the arms in contact, the black surface of the mica then having a clear plate of mica in front of it. On bringing a lighted candle near the instrument, and allowing it to shine through the clear plate on the blackened mica, the clear plate is at once driven away till the arm sets at right angles to the other.

Two currents of force, acting in opposite directions, can exist in the same bulb. I have prepared a double radiometer in which two flies are pivoted one over the other, and having their blackened sides turned in opposite directions. On bringing a lighted candle near, the flies rapidly rotate in opposite directions.

Experiment shows that the force can be reflected from a plane surface in such a manner as to change its direction. If an ordinary radiometer is exposed to light the black surface is repelled, owing to the excess of pressure acting between it and the glass. If, however, a plate of mica were to arrest this force and reflect it back again, the motion should be reversed. Experiment shows that this is the case. A two-disk radiometer was made, having flat opaque mica disks blacked on one side. In front of the black surface of the mica, about a millimetre off, is fixed a large disk of thin clear mica. On bringing a candle near, the molecular pressure streaming from the black surface is caught by the clear plate and thrown back again, causing pressure behind instead of in front; and the result is rapid rotation in the negative direction, the black side now moving towards the light.

To still further test this view of the action, I made another radiometer, similar to the above, but having a clear mica disk on each side of the ordinary mica vane. This prevents the reflection of the pressure backwards, and causes it to expend itself in a vertical plane, the result being an almost total loss of sensitiveness.

The above actions can be explained on the "evaporation and condensation" theory, as well as by that of molecular movement; and I therefore devised the following test to decide between these two theories. A radiometer has its four disks cut out of very clear and thin plates of mica, and these are mounted in a somewhat large bulb. At the

side of the bulb, in a vertical plane, a plate of mica, blacked on one side, is fastened in such a position that each clear vane in rotating shall pass it, leaving a space between of about a millimetre. If a candle is brought near, and by means of a shade the light is allowed to fall only on the clear vanes, no motion is produced; but if the light shines on the black plate, the fly instantly rotates as if a wind were issuing from this surface, and keeps on moving as long as the light is near. This could not happen on the evaporation and condensation theory, as this requires that the light should shine intermittently on the black surface in order to keep up continuous movement.

By cutting a thin plate of aluminium into the form of a spiral, then drawing it out corkscrew fashion, blacking the upper surface and suspending it on a point, a spiral radiometer is made, which rotates like a screw on exposure to light. Here also the black surface need never be in darkness, the pressure acting continuously between the black side of the spiral and the cylindrical tube in which it is mounted.

The experiments with the double radiometer of different sizes showed that the nearer the absorbing surface was to the glass, the greater was the pressure produced. To test this point in a more accurate manner, a torsion-balance was fitted up with a glass suspending fibre and reflecting mirror, as described in my previous papers. At one end of the beam is a disk of roasted mica blacked on one side. In front of this black surface, and parallel to it, is a plate of clear mica, so arranged that its distance from the black surface can be altered as desired, at any degree of exhaustion, without interfering with the vacuum. This apparatus is very sensitive and gives good quantitative results. It has proved that when light falls on the black surface molecular pressure is set up, whatever be the degree of exhaustion. At the atmospheric pressure this disturbance can only be detected when the mica screen is brought close to the black surface, and it is inappreciable when the screen is moved away. As the barometer-gauge rises, the thickness of the layer of disturbance increases. Thus, retaining the standard candle always the same distance off, when the gauge is at 660 millims., the molecular pressure is represented by 1 when the space separating the screen from the black surface is 3 millims., by 3 when the intervening space is reduced to 2 millims., and by 5 when the space is 1 millim. With the gauge 722 millims. high, the values of the molecular pressure for the spaces of 3, 2, and 1 millim. are respectively 3, 7, and 12. When the gauge is at 740 millims., the corresponding values for spaces of 3, 2, and 1 millim. are 11, 16, and 23. With the gauge at 745 millims., the molecular pressures are represented by 30, 34, and 40, for spaces 3, 2, and 1 millim. When the gauge and barometer are level, the action is so strong that the candle has to be moved double the distance off, and the pressures when the intervening spaces are 12, 6, and 3 millims. are respectively 60, 86,

and 107. A large series of observations have been taken with this apparatus, with the result not only of supplying important data for future consideration, but of clearing up many anomalies which were noticed, and of correcting many errors into which I was led at earlier stages of this research. Among the latter may be mentioned the speculations in which I indulged as to the pressure of sunlight on the earth.

Hitherto most of my experiments had been carried on with bad conductors of heat. To get the maximum action of a radiometer it appeared necessary that no heat should pass through to the back surface, but that all should be kept as much as possible on the surface on which the light fell*. At first I used pith, but since learning the advantage of raising the whole apparatus to a high temperature during exhaustion, I have used roasted mica lampblacked on one side for the vanes; for this purpose it is almost perfect, being a good absorber on one face, a good reflector on the other, a bad conductor for heat, extremely light, and able to stand high temperatures. Many experiments have been tried with metal radiometers, some of the results being recorded in previous papers which I have read before the Society; but being less sensitive than pith or mica instruments, I had not hitherto worked much with them. I now tried similar experiments to the above, using the best conductors of heat instead of the worst; and for this purpose thick gold-leaf was selected for the surface on which to try the action of radiation.

An apparatus was constructed resembling a radiometer with an opening at the top, capable of being closed with a plate of glass. Through this I could introduce disks of any substance I liked, mounted in pairs on an aluminium arm rotating on a needle-point. The first disks were of gold-leaf, blacked on alternate sides. After exhaustion, a candle repelled the black surface of one of the disks, but, to my surprise, it strongly attracted the black surface of the other disk. I noticed that the disk which moved the negative way was somewhat crumpled, and had the outer edge curved so as to present a slightly concave black surface to the candle. I soon found that the curvature of the disk was the cause of the anomaly observed, and experiments were then tried with disks of gold and aluminium—the latter being chiefly used as being lighter and stiffer, whilst it acted in other respects as gold.

A radiometer the fly of which is made of perfectly flat aluminium plates, lampblacked on one side, is much less sensitive to light than one of mica or pith, but, as I proved in my earlier papers, it is more sensitive to dark heat. Exposed to light, the black face of a metal radiometer moves away as if it were black pith. When, however, it is exposed to dark

* I have already shown that when a ray of *light* from any part of the spectrum falls on a black surface the ray is absorbed and degraded in refrangibility, warming the black surface and being emitted as radiant heat. In this sense only can the repulsion resulting from radiation be called an effect of heat.

heat, either by grasping the bulb with the warm hand, dipping it into hot water, or covering it with a hot glass shade, it rapidly rotates in a negative direction, the black advancing, and continuing to do so until the temperature has become uniform throughout. On now removing the source of heat, the fly commences to revolve with rapidity the positive way, the black this time retreating as it would if light shone on it. Pith or mica radiometers act differently to this, dark heat causing them to revolve in the same direction as light does.

The outer corners of the aluminium plates, which were mounted diamond-wise, were now turned up at an angle of 45°, the lampblacked surface being concave and the bright convex. On being exposed to a candle, scarcely any movement was produced; when one vane was shaded off the other was repelled slightly, but the turned-up corner seemed to have almost entirely neutralized the action of the black surface. A greater amount of the same corner was now turned up, the fold going through the centres of adjacent sides. Decided rotation was now produced by a candle, but the black surface was *attracted* * instead of repelled. Dark heat still caused the opposite rotation to light, repelling the black surface.

The plates were now folded across the vertical diagonal, the black surface being still inside and the bright metal outside. The actions with a candle and hot glass shade were similar to the last, but more decided.

The plates were now flattened, and put on the arms at an angle, still being in the vertical plane. When the bright surface was outside, scarcely any action was produced by a candle; but when the lampblacked surface was outside, strong repulsion of the black was produced, both with a candle and with a hot shade.

The square aluminium plates were mounted in the experimental apparatus, one being attached to the arm by the centre of one of the sides, and the other by an angle. The opposite corner of the one mounted diamond-wise was turned up at an angle. The outer convex surface of the diamond plate was blacked, and the side of the square plate facing the same way was also blacked, so that either two black or two bright surfaces were always exposed to the light, instead of a black and a white surface, as is usual in radiometers. As might have been expected, both these black surfaces were repelled; but the turned-up corner of the diamond-mounted plate proved so powerful an auxiliary to its black surface, that strong rotation was kept up, the square plate being dragged round against the action of light.

Folding the plates with the angle horizontal has not so decided an action as when the fold is vertical.

Sloping the plates and disks of a lampblacked mica radiometer so as to

* I use the word attraction in these cases for convenience of expression. I have no doubt that what looks like attraction in these and other cases is really due to *vis a tergo*.

have the black outside, and consequently more facing the side of the bulb, greatly increases its sensitiveness.

The above experiments show that shape has even a stronger influence than colour. A convex bright surface is strongly repelled, whilst a concave black surface is not only not repelled by radiation but is actually attracted.

I have also tried carefully shaped cups of gold, aluminium, and other metals, as well as cones of the same materials. I will briefly describe the behaviour of a few typical radiometers made with metal cups, which I have the honour of exhibiting to the Society.

No. 1035. A two-disk, cup-shaped radiometer, facing opposite ways; both sides bright. The disks are 14·5 millims. diameter, and their radius of curvature is 14 millims.

Exposed to a standard candle 3·5 inches off, the fly rotates continuously at the rate of one revolution in 3·37 seconds. A screen placed in front of the concave side so as to let the light shine only on the convex surface, repels the latter, causing continuous rotation at the rate of one revolution in 7·5 seconds. When the convex side is screened off so as to let the light shine only on the concave side, continuous rotation is produced at the rate of one revolution in 6·95 seconds, the concave side being attracted.

These experiments show that the repulsive action of radiation on the convex side is about equal to the attractive action of radiation on the concave side, and that the double speed with which the fly moves when no screen is interposed is the sum of the attractive and repulsive actions.

No. 1037. A two-disk, cup-shaped aluminium radiometer, as above, lampblacked on the concave surfaces.

In this instrument the action of light is reversed, rotation taking place, the bright convex side being repelled, and the black concave attracted.

That this attraction is not apparent only, is proved by shading off the sides one after the other. When the light shines only on the bright convex side no movement is produced, but when it shines on the black concave side, this is attracted, producing rotation.

No. 1038. A cup-shaped radiometer similar to the above, but having the convex surfaces black and the concave bright.

Light shining on this instrument causes it to rotate rapidly, the convex black being repelled. No movement is produced on letting the light shine on the bright concave surface, but good rotation is produced when only the black convex surface is illuminated.

No. 1039. A cup-shaped radiometer like the above, but blacked on both sides.

With this a candle causes rapid rotation, the convex side being repelled. On shading off the light from the concave side the rotation continues, but much more slowly; on shading off the convex side the concave is strongly attracted, causing rotation.

When either of these four radiometers is heated by a hot shade or plunged into hot water, rotation is always produced in the opposite direction to that caused by the light. On removing the source of heat the motion rapidly stops, and then commences in the opposite direction (*i. e.* as it would rotate under the influence of light), the rotation continuing as long as the fly is cooling. Chilling one of these radiometers with ether has the opposite action to exposing it to dark heat.

The vanes of radiometers have also been formed of metal cones, and of cups and cones of plain mica, roasted mica, pith, paper, &c.; and they have been used either plain or blacked on one or both surfaces. These have also been balanced against each other, and against metal plates, cups, and cones. The results are of considerable interest, but too complicated to explain without great expenditure of time and numerous diagrams. The broad facts are contained in the above selections from my experiments.

The action of light on the cup-shaped vanes of a radiometer probably requires more experimental investigation before it can be properly understood. Some of the phenomena may be explained on the assumption that the molecular pressure acts chiefly in a direction normal to the surface of the vanes. A convex surface would therefore cause greater pressure to be exerted between itself and the bounding surface of glass than would a concave surface. In this way the behaviour of the cup-shaped radiometer with both surfaces bright, No. 1035, can be understood, and perhaps also that of Nos. 1038 and 1039. It would not be difficult to test this view experimentally, by placing a small mica screen in the focus of a concave cup where the molecular force should be concentrated. But it is not easy to see how such an hypothesis can explain the behaviour of No. 1037, where the action of the bright convex surface more than overcomes the superior absorptive and radiating power of the concave black surface; and the explanation entirely fails to account for the powerful attraction which a lighted candle is seen to exert on the concave surfaces in Nos. 1035, 1037, and 1039.

II. "Magnetic Observations made at Stonyhurst College Observatory from April 1870 to March 1876." By the Rev. S. J. Perry, S.J., F.R.S. Received July 31, 1876.

A double series of magnetic observations are being carried on continuously at this Observatory. The monthly determinations of the absolute elements date from the year 1863, but the uninterrupted photographic record of the variations of the Declination and of the components of the Intensity was only commenced in 1867. The photographic curves of the Declination and Horizontal Force have all been measured, and are in course of reduction; and the Vertical-Force curves will soon be taken in hand. The results of the first seven years' observations of the Dip and Horizontal Force appeared in the Philosophical Transactions for 1871, and the

present paper contains a precisely similar, and therefore comparable, reduction for the following six years of all the elements of terrestrial magnetism. In the former paper the general result was somewhat at variance with the conclusions arrived at by Sir Edward Sabine from a discussion of the Kew observations; and hence the necessity for this second paper, to discover whether local influences or mere accidental errors would account for the discrepancies. The change of the station of observation during the period from 1863 to 1870 introduced a probable source of error into the first Stonyhurst series; but that uncertainty is now removed, as all the observations since 1868 have been taken in the fixed magnetic hut. It will be unnecessary here to repeat what was said in the former paper on the subject of the instruments used in the observations, as these have remained unchanged since 1863. The only alteration in the observations themselves has been the substitution of weekly in lieu of monthly readings of the Declination from the beginning of the year 1872.

The Horizontal Force.

TABLE I.—Monthly mean values of the Horizontal Force.

Summer Months.	1870.	1871.	1872.	1873.	1874.	1875.	Mean of six years.
April	3·6151	3·6354	3·6275	3·6305	3·6388	3·6459	3·6322
May	3·6164	3·6242	3·6290	3·6353	3·6400	3·6481	3·6322
June	3·6256	3·6250	3·6262	3·6314	3·6390	3·6418	3·6315
July	3·6273	3·6211	3·6309	3·6303	3·6377	3·6399	3·6312
August	3·6229	3·6218	3·6298	3·6333	3·6425	3·6442	3·6324
September	3·6009	3·6370	3·6297	3·6311	[3·6446]	3·6524	3·6326
Means	3·6180	3·6276	3·6289	3·6320	3·6404	3·6454	3·6321

Winter Months.	1870–71.	1871–72.	1872–73.	1873–74.	1874–75.	1875–76.	Mean of six years.
October	3·6207	3·6249	3·6341	3·6366	3·6466	3·6448	3·6346
November	3·6183	3·6303	3·6356	3·6298	3·6468	3·6492	3·6350
December.........	3·6249	3·6340	3·6401	3·6351	[3·6480]	3·6504	3·6388
January	3·6256	3·6300	3·6289	3·6466	3·6492	3·6485	3·6381
February	3·6207	3·6328	3·6330	3·6410	3·6447	3·6482	3·6367
March	3·6229	3·6321	3·6218	3·6412	3·6414	3·6443	3·6340
Means	3·6222	3·6307	3·6323	3·6384	3·6461	3·6476	3·6362
Yearly means ...	3·6201	3·6292	3·6306	3·6352	3·6433	3·6465	3·6342

The figures for September and December 1874 are interpolations.

From the above Table we have for the epoch April 1st, 1873,

The mean Horizontal Force $=3{\cdot}6342$,
With a secular acceleration of $+0{\cdot}0053$.

Comparing this with results of previous years, we find the secular acceleration to be on the increase, having been only 0·0042 for October 1st, 1866, and 0·0047 for January 1st, 1870.

The above value of the secular acceleration is almost identical with that found by Mr. Whipple from a similar discussion of the Kew observations.

With the values given in Table I. we can readily calculate the semiannual inequality.

TABLE II.—Semiannual inequality of the Horizontal Force.

Date.	Correction for secular variation.	Mean value ± secular variation.	Observed values.	Observed – Computed.	
				Summer.	Winter.
July 1, 1870	−0·0145	3·6197	3·6180	−0·0017	
Jan. 1, 1871............	−0·0119	3·6223	3·6222	....	−0·0001
July 1, 1871	−0·0092	3·6250	3·6276	+0·0026	
Jan. 1, 1872............	−0·0066	3·6276	3·6307	....	+0·0031
July 1, 1872	−0·0040	3·6302	3·6289	−0·0013	
Jan. 1, 1873............	−0·0013	3·6329	3·6323	....	−0·0006
July 1, 1873	+0·0013	3·6355	3·6320	−0·0035	
Jan. 1, 1874............	+0·0040	3·6382	3·6384	....	+0·0002
July 1, 1874	+0·0066	3·6408	3·6404	−0·0004	
Jan. 1, 1875............	+0·0092	3·6434	3·6461	....	+0·0027
July 1, 1875	+0·0119	3·6461	3·6454	−0·0007	
Jan. 1, 1876............	+0·0145	3·6487	3·6476	....	−0·0011
Mean differences in the semiannual periods				−0·00083	+0·00070

These numbers give an annual variation of 0·00153, the force being greater when the sun is nearer the earth, which bears out the conclusion arrived at by Sir E. Sabine from similar observations at Toronto, Hobarton, and Kew. The discussion of subsequent Kew observations by Dr. Stewart, and afterwards by Mr. Whipple, shows no semiannual inequality, whereas the first Kew series gave the value 0·0026, which is almost double the above number. The previous series of Stonyhurst observations led to a contrary result; but considerable uncertainty was attached to those observations on account of the correction for change of observing station, which objection cannot be urged against the present series. Unfortunately the significant digits in the number 0·00041 were

accidentally transposed in the former paper; but this affected the result as to amount only, and not as to direction.

TABLE III.—Residual errors in the monthly values of the Horizontal Force.

Summer months.	1870.	1871.	1872.	1873.	1874.	1875.	Mean.	Semiannual mean.
April	− 27	+123	− 9	−32	− 1	+17	+0·00118	−0·00010
May	− 19	+ 7	+ 2	+12	+ 6	+35	+0·00072	
June	+ 69	+ 10	−31	−31	− 8	−33	−0·00040	
July	+ 82	− 33	+12	−47	−26	−56	−0·00113	
August	+ 33	− 31	− 3	−21	+18	−18	−0·00037	
September ...	−191	+117	− 9	−48	+35	+60	−0·00060	
Winter months.	**1870–71.**	**1871–72.**	**1872–73.**	**1873–74.**	**1874–75.**	**1875–76.**	**Mean.**	**Semiannual mean.**
October	−13	−24	+ 16	−12	+35	−36	−0·00057	−0·00009
November ...	−41	+26	+ 26	−85	+33	+ 4	−0·00062	
December ...	+20	+59	+ 67	−36	+40	+11	+0·00268	
January......	+23	+14	− 50	+75	+48	−12	+0·00163	
February ...	−31	+38	− 13	+14	− 2	−20	−0·00023	
March	−13	+26	−130	+12	−39	−63	−0·00345	
Yearly means	− 9	+28	− 10	−17	+12	− 9		

The probable errors deduced from this Table are $\pm 0{\cdot}0033$ for each monthly determination, and $\pm\ 0{\cdot}0004$ for the resulting mean.

The Magnetic Dip.

TABLE I.—Monthly mean values of the Dip.

Summer months.	1870.	1871.	1872.	1873.	1874.	1875.	Mean of six years.
April	69° 33′ 6″	69° 35′ 12″	69° 33′ 15″	69° 29′ 13″	69° 27′ 55″	69° 19′ 58″	69° 29′ 47″
May	40 25	33 17	29 19	32 10	23 4	21 37	29 59
June............	37 47	32 33	31 24	30 11	28 50	21 35	30 23
July	39 30	32 55	29 56	28 34	27 54	20 8	29 50
August	36 36	32 57	29 31	33 45	25 51	22 21	30 10
September ...	40 26	30 57	32 8	30 0	[26 14]	24 8	30 39
Means	69 37 58	69 32 59	69 30 56	69 30 39	69 26 38	69 21 38	69 30 8

TABLE I. (*continued*).

Winter months.	1870–71.	1871–72.	1872–73.	1873–74.	1874–75.	1875–76.	Mean of six years.
	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″
October	69 38 25	69 27 52	69 31 50	69 29 37	69 26 36	69 18 48	69 28 51
November ...	38 22	29 53	31 22	30 15	27 17	19 58	29 31
December......	38 17	30 59	32 27	33 6	[26 55]	21 48	30 35
January	40 8	32 13	30 28	29 11	26 32	25 45	30 43
February	35 26	32 45	31 36	25 36	21 35	24 25	28 34
March	31 53	34 53	30 20	29 19	24 39	24 25	29 15
Means	69 37 5	69 31 26	69 31 21	69 29 31	69 25 36	69 22 32	69 29 35
Yearly means	69 37 32	69 32 13	69 31 9	69 30 5	69 26 7	69 22 5	69 29 52

The numbers for September and December 1874 are interpolated.

We have therefore for April 1st, 1873,

$$\text{The mean dip} \qquad = 69^\circ\ 29'\ 52'',$$
$$\text{With a secular variation} = -\quad 3'\ 5''\cdot 4.$$

The amount of annual diminution from the preceding seven years' observations was only 1′ 49″·2; the dip would therefore appear to be decreasing more rapidly at present; but the value −2′·15, given last year by the Kew results, shows that the Stonyhurst number is probably too large.

TABLE II.—Semiannual inequality of the Dip.

Date.	Correction for secular variation.	Mean Value ± secular variation.	Observed Values.	Observed − Computed.	
				Summer.	Winter.
	′ ″	° ′ ″	° ′ ″	′ ″	′ ″
July 1, 1870...............	+8 30	69 38 22	69 37 58	−0 24	
Jan. 1, 1871............	+6 57	36 49	37 5	....	+0 16
July 1, 1871...............	+5 24	35 16	32 59	−2 17	
Jan. 1, 1872............	+3 52	33 44	31 26	....	−2 8
July 1, 1872...............	+2 19	32 11	30 56	−1 15	
Jan. 1, 1873............	+0 46	30 38	31 21	....	+0 43
July 1, 1873...............	−0 46	29 6	30 39	+1 33	
Jan. 1, 1874............	−2 19	27 33	29 31	....	+1 58
July 1, 1874..............	−3 52	26 0	26 38	+0 38	
Jan, 1, 1875............	−5 24	24 28	25 36	....	+1 8
July 1, 1875...............	−6 57	22 55	21 38	−1 17	
Jan. 1, 1876............	−8 30	21 22	22 32	....	+1 10
Mean differences in the semiannual periods				−0 30	+0 31

This Table gives the winter period an excess of 1′ 1″ over that of summer, which agrees well with the mean of the values 1′·31 and 0′·80 found by Gen. Sabine and by Mr. Whipple from the Kew observations. The value obtained by Dr. Stewart for the period 1863 to 1869 is less than any of the above, and the result from the Stonyhurst observations taken during the same period is considerably below that of Dr. Stewart; but still every series makes the winter number greater than that for the summer.

TABLE III.—Residual errors in the monthly values of the Dip.

Summer months.	1870.	1871.	1872.	1873.	1874.	1875.	Mean.	Semi-annual mean.
April	−5′ 24″	−0′ 13″	+0′ 56″	−0′ 1″	+1′ 47″	−3′ 5″	−1′ 0″	+0′ 1″·8
May	+2 10	−1 52	−2 45	+3 12	−2 49	−1 10	−0 32	
June	−0 12	−2 21	−0 24	+1 28	+3 13	−0 57	+0 8	
July	+1 46	−1 43	−1 37	+0 7	+2 32	−2 8	−0 11	
August	−0 52	−1 26	−1 46	+5 33	+0 45	+0 20	+0 26	
September ...	+3 13	−3 10	+1 6	+2 4	+1 23	+3 23	+1 20	
Winter months.	**1870–71.**	**1871–72.**	**1872–73.**	**1873–74.**	**1874–75.**	**1875–76.**	**Mean.**	**Semi-annual mean.**
October	+0′ 27″	−7′ 1″	+0′ 2″	+0′ 55″	+0′ 59″	−3′ 43″	−1′ 24″	−0′ 1″·3
November ...	+0 39	−4 44	−0 10	+1 49	+1 56	−2 17	−0 28	
December ...	+0 50	−3 23	+1 10	+4 55	+0 49	−0 12	+0 42	
January	+2 56	−1 54	−0 33	+1 15	+1 42	+4 0	+1 14	
February ...	−1 31	−1 6	+0 50	−2 4	−3 0	+2 56	−0 39	
March	−4 48	+1 17	−0 10	+2 54	+0 20	+3 11	+0 27	
Yearly means	−0 4	−2 18	−0 17	+1 51	+0 40	+0 2		

From these figures we conclude that the probable error of each monthly value is ± 1′ 40″, whilst that of the mean is ± 0′ 12″.

The Total Force.

We are now in a position to test the semiannual variation of the Total Force, or the Intensity of the earth's magnetism. Referring back to Table I., we find for the summer periods the mean Horizontal Force = 3·6321, and the mean Dip = 69° 30′ 8″; whilst for the winter periods we get 3·6362 and 69° 29′ 35″. If, then, we apply to the winter epoch the necessary corrections for secular variation, we obtain for the common epoch of January 1st, 1873—

	H. F.	Dip.	T. F.	V. F.
Summer periods	3·6321	69° 30′ 8″	10·3724	9·7152
Winter periods	3·6336	69 31 9	10·3843	9·7280
Excess in winter	0·0015	1 1	0·0119	0·0128

Contrary to the results of the former paper, these figures show a very large excess in the intensity for the winter months, thus strongly confirming the conclusion of the increase of the earth's magnetic force with the nearer approach of the sun. The late Kew reductions lead to the same conclusion, but the difference for the seasons is not so large.

The Magnetic Declination.

To complete the reduction of the absolute magnetic elements, I will now subjoin a discussion of the Declination observations, although the Dip and Intensity were alone included in the former paper. It has been thought advisable not to apply any correction to the observed Declinations, either for disturbances or for diurnal range, especially as the readings were always taken within a short interval from 9 A.M.

TABLE I.—Monthly mean values of the Declination.

Summer months.	1870.	1871.	1872.	1873.	1874.	1875.	Mean of six years.
April	21° 43′ 13″	21° 42′ 9″	21° 21′ 55″	21° 20′ 42″	21° 3′ 12″	20° 59′ 34″	21° 21′ 48″
May	38 23	31 51	23 53	21 29	8 1	54 21	19 40
June	47 20	32 21	21 4	20 42	9 26	59 26	21 43
July	39 24	28 21	22 57	18 29	12 51	57 7	19 52
August	43 16	30 21	27 35	23 17	8 7	54 41	21 13
September	39 21	35 43	25 5	18 19	10 46	21 0 3	21 33
Means	21 41 50	21 33 28	21 23 45	21 20 30	21 8 44	20 57 32	21 20 59

Winter months.	1870–71.	1871–72.	1872–73.	1873–74.	1874–75.	1875–76.	Mean of six years.
October	21° 34′ 19″	21° 36′ 27″	21° 21′ 6″	21° 15′ 40″	21° 13′ 4″	21° 1′ 33″	21° 20′ 22″
November	41 18	31 33	26 1	17 2	12 24	20 53 9	20 15
December	43 56	29 58	30 12	20 29	10 41	48 59	20 43
January	50 35	31 5	28 18	19 53	6 28	53 14	21 36
February	59 6	26 10	29 40	12 31	7 45	54 26	21 36
March	35 48	27 41	23 31	9 2	1 47	52 39	15 5
Means	21 44 10	21 30 29	21 26 28	21 15 46	21 8 42	20 54 0	21 19 56
Yearly means...	21 43 0	21 31 59	21 25 7	21 18 8	21 8 43	20 55 46	21 20 27

The figures in this Table give for the epoch April 1st, 1873—

The mean Declination = 21° 20′ 27″ W.
With a secular variation of – 9′ 27″.

As might be suspected from the geographical positions, this amount of secular diminution is somewhat in excess of that found at Kew for the same epoch, which was –8′ 5″·72.

Table II.—Semiannual inequality of the Declination.

Date.	Correction for secular variation.	Mean value ± secular variation.	Observed values.	Observed – Calculated. Summer.	Observed – Calculated. Winter.
	′ ″	° ′ ″	° ′ ″	′ ″	′ ″
July 1, 1870...............	+25 59	21 46 26	21 41 50	–4 36	
Jan. 1, 1871............	+21 15	41 42	44 10	....	+2 28
July 1, 1871...............	+16 32	36 59	33 28	–3 31	
Jan. 1, 1872............	+11 49	32 16	30 29	....	–1 47
July 1, 1872...............	+ 7 5	27 32	23 45	–3 47	
Jan. 1, 1873............	+ 2 22	22 49	26 28	....	+3 39
July 1, 1873...............	– 2 22	18 5	20 30	+2 25	
Jan. 1, 1874............	– 7 5	13 22	15 46	....	+2 24
July 1, 1874...............	–11 49	8 38	8 44	+0 6	
Jan. 1, 1875............	–16 32	3 55	8 42	....	+4 47
July 1, 1875...............	–21 15	20 59 12	20 57 32	–1 40	
Jan. 1, 1876............	–25 59	54 28	54 0	....	–0 28
Mean differences in the semiannual periods............				–1 50·5	+1 50·5

Here again the winter is in excess of the summer period, the annual variation being 3′ 41″. This agrees also with the Kew results; but the amount found by Mr. Whipple is only 1′ 21″·80.

Table III.—Residual errors in the monthly values of the Declination.

Summer months.	1870.	1871.	1872.	1873.	1874.	1875.	Mean.	Semi-annual mean.
April	–3 20	+5 2	–5 45	+2 29	–5 34	+0 15	–1 6	
May	–7 23	–4 28	–3 0	+4 3	+0 2	–4 11	–2 30	
June	+2 21	–3 11	–5 1	+3 3	+2 14	+1 41	+0 11	+0′ 0″·2
July	–4 48	–6 24	–2 21	+2 38	+6 26	+0 19	–0 42	
August	–0 8	–3 37	+3 4	+8 13	+2 30	–1 30	+1 25	
September ...	–3 16	+2 33	+2 21	+4 2	+5 56	+4 40	+2 43	

TABLE III. (*continued*).

Winter months.	1870–71.	1871–72.	1872–73.	1873–74.	1874–75.	1875–76.	Mean.	Semi-annual mean.
October	−11′ 12″	+0′ 23″	−5′ 31″	−1′ 31″	+5′ 20″	+3′ 16″	−1′ 33″	
November ...	− 3 26	−3 44	+0 11	+0 39	+5 27	−4 21	−0 52	
December ...	0 0	−4 32	+5 9	+4 53	+4 32	−7 44	+0 23	−0′ 0″·2
January	+ 7 26	−2 37	+4 2	+5 4	+1 6	−2 41	+2 3	
February ...	+16 44	−6 45	+6 12	−1 31	+3 10	−0 42	+2 51	
March	− 5 47	−4 27	+0 50	−4 12	−2 1	−1 42	−2 53	
Yearly means	− 1 4	−2 40	+0 1	+2 18	+2 26	−1 3		

This Table shows that we can only rely on the monthly determinations to within 2′ 55″, even excluding the result for February 1871; but the mean value for the whole epoch has only a probable error of 21″. The weekly readings that are now being taken will, it is hoped, sufficiently reduce the probable error of the monthly means.

Summing up the general results of the observations with respect to the main point at issue, viz. the existence of a semiannual inequality in the magnetic elements, it is satisfactory to find a complete confirmation of the conclusion of Sir E. Sabine, that a nearer approach of the sun in the winter months produces a very sensible increase in all the elements of terrestrial magnetism.

III. "On Electrical Conductivity and Electrolysis in Chemical Compounds." By Dr. L. BLEEKRODE. Communicated by WARREN DE LA RUE, D.C.L., F.R.S. Received October 2, 1876.

§ 1. *Introduction.*

In presenting this communication to the Royal Society I wish to state that it is only an abstract of a more extensive paper on the same subject which I hope to publish shortly, and which contains an account of experiments with nearly seventy substances, most of which were never used before for such an investigation. I tried also nearly all the liquefied gases, and a considerable time was spent in preparing them for this kind of research, that was often interrupted by fearful explosions. The invaluable opportunity which Mr. Warren De La Rue, F.R.S., granted me some time ago to try the same compounds with his very powerful battery, led to results which I hope the Society will not consider devoid of interest.

I entered on these experiments with the purpose of establishing, if possible, a relation between electrical conductivity and chemical consti-

tution, and to ascertain if the presence of hydrogen in the compound (and hydrogen can easily be replaced by other substances, especially metals) is connected with the liability to electrolysis.

Though in former years some experiments were made in this way in Germany, I have now had the opportunity of extending them very much, as I used the compounds which modern organic chemistry has taught us since to prepare. It is also important to remark that I only tried substances which are in a fluid condition, or can be reduced to it without requiring a solvent. It is sufficiently known that the solvent generally is electrolyzed at the same time, and the results become very complicated; therefore the gases in their liquefied state were especially fit for examination, the more so because several of them are very often used in chemistry as powerful agents. The gases were liquefied by the method first proposed by Faraday, in strong glass tubes, which had platinum wires fused in at their extremities; these ends were brought close together, at a distance of 2, 3, or 4 millims., in the closed end of the tube; this part was, after the liquefaction took place, filled with the liquid gas. With substances liquid at the ordinary temperature and pressure, the same arrangement was made, but of course there was then no difficulty. I successively caused a current of 10, 20, 40, and 80 galvanic cells (greatest size of Bunsen's) to pass through the liquids, and connected them at the same time with a very delicate galvanometer. I also used the spark of an induction-coil, the length of which exceeded 75 millims. With this apparatus the condensed gases generally exploded, and the other substances were decomposed by the *thermal* effect of the spark: it was a case of dissociation. As to the galvanic current, even the strongest did not pass in a perceptible way through the following compounds (amongst others):—liquid carbonic acid, liquid hydrochloric acid (nor any other hydrogenated acid, as BrH, IH, with exception of CNH), liquid cyanogen, bisulphide of carbon, benzine, tin tetrachloride, zinc-ethyl.

Liquefied ammonia forms a remarkable exception; it conducts the galvanic current even of a moderate battery very well, and it is at the same time electrolyzed; with a battery of 80 cells apparently a new body is separated, because the liquid becomes of an intense blue colour, and much gas is evolved. I shall communicate more particulars on this subject in my aforesaid paper.

Now, though it may be worthy of remark that a compound like hydrochloric acid, when in the liquid condition, opposes a formidable resistance to a galvanic current, which may be called very strong in comparison with those which are generally used for electrolytical purposes, I confess I was not satisfied till I had tried the most powerful current that ever has been produced: it is the current of the chloride-of-silver battery of Mr. Warren De La Rue; and as I proposed during my stay in London to carry on this investigation, he not only most willingly consented to my

proposal, but I had also the favour of his highly esteemed aid in the experiments we thus made together*.

§ 2. *Effects of the Current of a Battery of* 8040 *cells on strongly insulating liquids.* By Dr. L. Bleekrode, *aided by* Warren De La Rue, F.R.S.

At the time of these experiments, the battery, which has been already the subject of some communications to the Royal Society, had attained a number of 8040 cells, which could be separately used in different series, so as to obtain currents of various intensity; the longest spark produced between a point positive and a disk negative in free air had a length of 8·5 millims. (0·348 inch). We tried only the liquids named above; though few in number, yet they are interesting from their constitution and their importance as chemical compounds.

The following notes were made as we experimented on the substances ready for examination.

I. *Ammonia* (H_3N).

The current of 3240 cells confirmed the results obtained already on a former occasion, when the ordinary galvanic battery of 80 cells was used, only the effect was stronger now. Streams of a deep blue colour arose in the liquid gas, and the positive electrode assumed a black colour, much gas being evolved at the same time. When the current ceases, the blue colour rapidly disappears, and the liquid becomes bright again.

II. *Bisulphide of Carbon* (CS_2).

We first tried the current from 3240 cells; the negative electrode, some inches in length, was seen to be repelled from the surface of the liquid till it came in contact with the glass of the tube; and it appeared from some floating particles that internal motions took place, probably caused by heat, though the hand did not detect any elevation of temperature. The current from 5640 cells was too strong; then the spark jumped between the electrodes, and these were covered with a brown tint (from a deposit of carbon). We tried afterwards if a polarization current could be detected on a Thomson galvanometer; but this failed, yet the instrument was very sensible. If two fingers were placed on the ends of the connecting wires, the current which was excited by this contact was strong enough to cause the luminous index to fly away off the whole scale.

III. *Benzine* (C_6H_6).

With 3240 cells a strong vibratory motion was observed in the liquid.

With 5640 cells this motion increased and became more apparent, also a ringing sound (very similar to that emitted by the contact-breaker of a

* I owe also many thanks to Prof. Frankland, who granted me all the facilities of his chemical laboratory in the Science Schools, South Kensington, and to his assistant, Mr. Cameron, for his valuable aid in preparing these substances.

moderate induction-coil when active) was heard. This was caused by one of the electrodes, the longest, that passed through the whole tube, and therefore was somewhat free in its movement; the passage of the current into the liquid set it in a vibratory condition, and the benzine presented an undulating surface. The effect was the strongest when the wire was negative, and the surface of the liquid was then depressed; when the long wire was positive the contrary was observed, the vibrations were less and the fluid ran up the wire. We did not find any sign of a polarization current.

The peculiar phenomenon of vibrations seems to be connected with a beautiful experiment which Mr. Warren De La Rue discovered before. When one of the terminal wires of the battery of 8040 cells ended in a flat horizontal copper disk, and the other in a very fine platinum wire (·002 inch) placed above it, a little further than the striking distance, the platinum electrode being negative, the electricity streamed out of the wire with a luminous appearance; but the wire was at the same time in a vibratory condition, as it described a luminous circle (sometimes changing into an ellipse). When the platinum wire was positive the vibrations were not so strong, and therefore the radius of the circle described was less.

IV. *Tin tetrachloride* ($SnCl_4$).

The current from 8040 cells passing through the liquid caused strong vibrations when the long wire (electrode) was negative, otherwise the vibrations were less when it was positive. We could not detect any current caused by polarization, nor any sign of decomposition.

V. *Carbonic acid* (CO_2).

We tried liquid carbonic acid gas at once with the current from 5640 cells. This was too much for the small space between theelectrodes; the spark jumped between them, and the heat developed caused the tube to explode with great violence. Notwithstanding we may conclude that this liquid gas also must be a very bad conductor, as sparks never appear in conducting substances.

VI. *Hydrochloric acid* (HCl).

The liquid hydrochloric acid gas was prepared from chloride of ammonium and concentrated sulphuric acid, which had been previously put into the tube and brought gradually into contact; the gas evolved had to pass through the acid, and was in this way deprived of moisture. At first a series of 2160 cells was used, but without result, no action being visible; afterwards we tried the current from 3240 cells, and the vibrations in the liquid were then very apparent by the undulating surface; at last 5640 cells were applied, when the ringing sound became audible from the vibrating electrode. We did not use the whole series for fear of an explosion possibly occurring by the spark jumping; yet the

experiment was sufficient to conclude that the liquid gas (which, dissolved in water, belongs to the best conductors) opposes a formidable resistance even to an extraordinarily strong galvanic current, and is not decomposed in a perceptible way.

VII. *Cyanogen* (C_2N_2).

The current from 3240 cells did not produce any effect on the liquid gas. With 5640 cells the vibrations were very apparent; and again we observed the same difference between the negative and the positive electrode as with the benzine, the liquid running up the wire when it was positive. No polarization was detected.

VIII. *Zinc-ethyl* ($Zn(C_2H_5)_2$).

With 3240 cells vibratory motions were observed in this liquid. They were very beautiful when the battery was increased to 5640 cells; the temperature of the liquid augmented now sensibly. Afterwards we detected a current caused by polarization of the electrodes, which produced a deflection of five divisions on the scale. We have therefore reason to conclude that electrolytic action took place, which may be accompanied therefore by vibratory motions in the liquid, though a very strong galvanic current is required.

IX. *Benzine* (C_6H_6).

The experiment III. with benzine was repeated in order to ascertain, if possible, whether a current could pass through a compound without electrolysis. The whole series of 8040 cells was now applied, and the spark jumped between the electrodes. A large deposit of carbonaceous matter was observed throughout the liquid, and was undoubtedly separated by the heat of the sparks. We had another tube with electrodes at a somewhat greater distance, and the current was kept passing during two minutes. No sparks jumped, and the liquid was in a strong vibratory condition, showing that the electricity was transmitted, yet we could not afterwards detect the slightest deflection caused by polarization. We observed also that the electrodes after the experiment were still very clean. Now it must be remarked that with liquids which proved themselves extremely bad conductors, the absence of a polarization current, when tested with the galvanometer, cannot lead to the conclusion that no electrolysis at all took place; for the great resistance which the current of the whole battery of 8040 cells had to overcome in the case of benzine was so much weakened, that it caused only a deflection of about 55 divisions on the scale of the sensitive Thomson galvanometer placed in the circuit; the same resistance in the liquid was obviously opposed to the feeble current that might result from polarization, which must therefore be unable to act on the galvanometer. The polarization current can then be no longer applied as a test for electrolysis in all cases.

If the same current was conducted through pure water, immediately a copious quantity of gas was evolved, but nothing of a vibratory motion in the liquid was visible. This seems only to occur in the case of highly insulating liquids, and illustrates the conduction of electricity in the way which was called by Faraday "the carrying discharge;" this acts mechanically; and it appears that the greater part of electricity is then transported by the molecules without their splitting up, as in the case of electrolysis.

The results of the experiments carried on in this investigation, with regard to electrolysis, may thus be stated:—

1. A general connexion between definite chemical characters and liability to electrolysis seems not to exist. The character that a compound contains hydrogen, which is easily replaced by a metal or radicals, or that it contains a metal for which another may be easily substituted, is not always accompanied with the conductive power that is required for electrolysis. With liquid ammonia this is the case, but with zinc-ethyle (which in contact with air bursts into flames, and is destroyed from its Zn changing into ZnO), benzine (which is easily transformed in different compounds by substitution), and tetrachloride of tin an enormous resistance is offered to the most powerful galvanic current ever used.

2. Sometimes a very bad conductive power is accompanied by a great difficulty in getting the hydrogen replaced in the compound. A remarkable example of this fact is offered by liquid hydrochloric acid; this compound was kept for months, even years, in contact with strips of zinc, and up to the present time very little action is perceptible. Gore* has communicated several experiments of this kind with similar results.

3. It appears that it is not the nature of the actual constituents in the compound which renders it proper to conduct electricity; but that this is more dependent on the inner arrangement of the molecules.

4. Although in the case of very bad conductors, as liquid carbonic acid and liquid hydrochloric acid, no test for electrolysis can be applied, yet it must not be concluded that they may not be decomposed by electrical agency. By using spirals of zinc and platinum† twisted together for a considerable space of time, they were actually split up; this fact shall be further elucidated in the paper mentioned in § 1.

The Hague, September 1876.

* Phil. Mag. [IV.] vol. xxix. p. 543 (1865).

† Gladstone and Tribe used this combination to analyze water and some organic compounds (Journal of the Chem. Society, 1872, p. 461).

November 23, 1876.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council proposed for election was read as follows :—

President.—Joseph Dalton Hooker, C.B., M.D., D.C.L., LL.D.

Treasurer.—William Spottiswoode, M.A., LL.D.

Secretaries.— { Professor George Gabriel Stokes, M.A., D.C.L., LL.D.
Professor Thomas Henry Huxley, LL.D., Ph.D.

Foreign Secretary.—Professor Alexander William Williamson, Ph.D.

Other Members of the Council.—Major-General John T. Boileau; Warren De La Rue, D.C.L.; Professor P. Martin Duncan, M.B., P.G.S.; Professor William H. Flower, F.R.C.S.; Professor Michael Foster, M.D.; Edward Frankland, D.C.L.; Francis Galton, M.A.; William Augustus Guy, M.B.; John Russell Hind, F.R.A.S.; The Rev. Robert Main, M.A.; William Pole, C.E., Mus. Doc.; The Rev. Bartholomew Price, M.A.; Rear-Admiral G. H. Richards, C.B.; Henry Clifton Sorby, Pres. Mic. Soc.; Professor Henry J. Stephen Smith, M.A.; Professor Balfour Stewart, M.A.

Mr. J. Croll and Prof. T. A. Thorpe were admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Influence of Geological Changes on the Earth's Axis of Rotation"*. By George H. Darwin, M.A., Fellow of Trinity College, Cambridge. Communicated by Professor J. C. Adams. Received October 13, 1876.

(Abstract.)

The subject of the fixity or mobility of the earth's axis of rotation in that body, and the possibility of variations in the obliquity of the ecliptic, has of late been attracting much attention; but the author be-

* Since this paper was in manuscript Sir William Thomson has delivered his address to the Mathematical Section of the British Association at Glasgow. He therein touches on this subject, and gives some of the results attained here; but as he has not stated how he has attacked the problem, and as the subject has been recently attracting much attention, the author still ventures to offer his paper to the Royal Society.

lieves that it has not hitherto been treated at much length. The paper, of which the following is an abstract, is an attempt to investigate the results of the supposition that the earth is slowly changing its shape, with especial reference to the effects on the obliquity of the ecliptic and on the geographical position of the earth's axis of figure.

1. This part of the paper is devoted to the consideration of the precession and nutations of an ellipsoid of revolution which is slowly and uniformly changing its shape. The change is supposed to proceed from causes internal to the earth, and only to continue so long as the total changes in the principal moments of inertia C and A remain small compared to their difference, C−A.

The problem is treated by means of M. Liouville's extension of Euler's equations of motion of a rigid body about a point *. By an approximate method these equations may be treated as linear, and the solution divided into two parts.

Let θ be the obliquity of the ecliptic; Π cosec θ the precession of the equinoxes; $-n$ the angular velocity of rotation of the ellipsoid; $A+at$, $A+bt$, $C+ct$ the principal moments of inertia at the time t. Then it is shown that the secular effect on the obliquity of the ecliptic, as resulting from the motion of the principal axes in the body (which constitutes the first part of the solution), is given by the equation

$$\frac{d\theta}{dt}=-\frac{\Pi}{2n}\,\frac{a+b-2c}{A};$$

and as resulting from the change in the impressed forces, due to the change of shape of the body (which constitutes the second part), is given by

$$\frac{d\theta}{dt}=\frac{\Pi}{2n}\,\frac{a+b-2c}{C-A}.$$

The former part may be neglected compared with the latter. But from such geological changes as we are entitled to assume in the case of the earth, the total change in the obliquity of the ecliptic must be exceedingly small. Even gigantic polar ice-caps during the Glacial period could not have altered the position of the arctic circle by so much as 3 inches; and this is the most favourable redistribution of matter on the earth's surface for producing that effect. Thus the obliquity of the ecliptic has remained sensibly constant throughout geological history.

It is also shown that, during any gradual deformation of the ellipsoid, the instantaneous axis of rotation will always remain sensibly coincident with the principal axis of figure.

In the course of the work by which the previous results are attained there is shown to be a small inequality in the motion of the instantaneous axis, in consequence of which that axis describes a circle with uniform

* Liouv. Journ. 2e série, t. iii. 1858, p. 1; Routh's Rigid Dynam. p. 150.

velocity, and is coincident with the axis of figure every 306th day (in the earth). This circle touches the meridian along which the axis of figure is travelling in consequence of the deformation of the earth's shape. The diameter of the circle is shown in a particular case (not unfavourable to produce a large effect) to be less than $\frac{1}{376}''$. But although this inequality appears to be so small, it is of interest and is discussed at some length. It is shown that, if the earth be not quite rigid, this inequality might have the effect of modifying the path of the axis of figure in the body, in consequence of readjustments to a figure of equilibrium.

Various hypotheses as to the power of adjustment are considered, and the paths of the instantaneous and principal axes in the precession of a viscous spheroid undergoing deformation are found.

It is maintained that although the earth may be sensibly rigid to the tidally deforming forces exercised by the sun and moon, it would not be so to considerable departures from the figure of equilibrium, such as would arise from a wandering of the pole of figure from its initial position; and that readjustments to an approximate form of equilibrium probably take place, at considerable intervals of time, impulsively by means of earthquakes. Such periodical adjustments would not sensibly modify the geographical path of the principal axis as due to terrestrial deformation. But it is held that during the consolidation of the earth there must have been great instability in the geographical position of the poles. Throughout the rest of the inquiry, however, the hypothesis of the earth's sensible rigidity, together with the possibility of more or less rare impulsive readjustments to the figure of equilibrium, is adhered to. In consequence of these results dynamical considerations may be dismissed, and it only remains to consider the kinematical question of the change in the earth's principal axes due to any deformation of its shape.

2. Formulæ for this end are here found, and are adapted for numerical calculation. It is assumed, in the first place, that the deformation is such that there is no change in the strata of equal density; and accordingly all suppositions as to the nature of the internal changes accompanying geological upheaval and subsidence are set aside.

3. The forms of continent and depression are next investigated, which, for the transport of a given quantity of matter from one part of the earth's surface to another, would cause the maximum deflection of the principal axis of greatest moment—subject, however, to the condition that the layer excavated or piled up shall nowhere exceed a given small fraction of the earth's radius.

It is shown that the continents and depressions must be of uniform height and depth; there must be two of each, all similar to one another; that each has one of its own kind diametrically opposite to it; that they are in shape sphero-conics, formed by the intersection of a certain elliptic cone with the sphere; that the centres of the four sphero-conics

are all on the same *complete* meridian and all in latitude 45°. A Table of numerical results depending on the values of certain elliptic functions is given.

4. In this part an endeavour is made to collect evidence as to the extent to which the earth may have undergone deformation from geological changes. The object is to discover what are the largest areas over which there has been a consentaneous rise or fall, and what is the greatest vertical amount of that rise or fall; also to determine how the erosion of the land and the sea affect the local excesses or deficiencies of matter on the earth's surface. The areas and amounts of elevation and subsidence which on a sealess and rainless globe are equivalent, as far as producing excesses or deficiencies of surface matter, to those which obtain on the earth are referred to as "effective;" and it is only the effective elevation or subsidence which we require to know in order to determine the shift of the earth's axis.

The evidence as to area is very meagre, because precise boundaries to regions of elevation and subsidence cannot be assigned; but, *faute de mieux*, the author's father, Mr. Charles Darwin, marked out for him on a map an area in the Pacific Ocean which (on account of the structure of the coral islands) he believes to have undergone subsidence within a recent geological period. From a consideration of this and of other points the author believes that from $\frac{1}{10}$ to $\frac{1}{20}$ of the whole earth's surface may, from time to time, have undergone elevation and subsidence. The greatest vertical effective amount of rise or fall cannot be determined from geological evidence, because of the effects of erosion and of the influx of the sea into parts below the mean level of the earth.

The only way of determining the point seems to be to find what is the difference of mass, standing on unit area of the earth's surface, in an ocean of, say, 15,000 feet deep, and in land of, say, 1100 feet high. From this difference of mass the effective elevation of an ocean-bed in its conversion into land can be at once determined. Taking the above numbers, it is found to be 10,436 feet; and in the examples given in the following part, the deflection of the polar axis, for an assumed effective elevation of 10,000 feet, is given in each case.

It is then pointed out that if the deformation of the earth were of very wide extent, the level surface of the sea would approximately follow the rocky surface, and that thus there might be sufficient change in the earth's shape to sensibly affect the position of the principal axis, without there being any geological signs of elevation or subsidence.

5. Numerical application is now made of the preceding work to the case of the earth, and, as before stated, all the results are given for 10,000 feet of effective elevation.

The first application is to continents and seas of maximum effect, and a Table of results is given. It may be here stated that if $\frac{1}{200}$ of the earth's surface is elevated, the deflection of the pole is $11\frac{1}{3}'$; if $\frac{1}{20}$,

$1° 46\frac{1}{2}'$; if $\frac{1}{10}$, $3° 17'$; and if $\frac{1}{2}$, $8° 4\frac{1}{2}'$ *. In each case an equal area is supposed to fall simultaneously.

Other examples are then given for continents and seas which do not satisfy the maximum condition; in some the boundaries are abrupt cliffs, in others shelving.

The conclusion is arrived at, that a single large geological change, such as those which obtain on the earth, is competent to produce an alteration in the position of the pole of from one to three degrees of latitude, on the hypothesis that there is no change in the law of internal density.

6. Various hypotheses as to the nature of the internal changes accompanying the deformation of the earth are discussed.

First, it is shown that if upheaval and subsidence are due to a shrinking of the earth as a whole, but to the shrinking being quicker than the mean in some regions and slower in others, the results are the same as those previously attained.

Second, the increase of surface matter due to the deposit of marine strata also gives the same results.

Third, the hypothesis that upheaval and subsidence are due to the intumescence or contraction immediately under the regions in question is considered. Under certain special assumptions, too long to recapitulate, it is shown that the previous results must be largely reduced. A Table of the values of the reducing factor for various thicknesses of the intumescent strata is given; from which it appears that if the stratum is tolerably thin and at all near the surface, the deflection of the pole is reduced to quite an insignificant amount. Even if the intumescence extends right down to the centre of the earth in a cone bounded by the elevated region, the results would be only about $\frac{2}{3}$ of the former ones. Hence it appears that the earlier results can only be stated as a superior limit to what is possible.

7. In conclusion it is pointed out that if the earth be quite rigid, no redistribution of matter in new continents could ever cause the deviation of the pole from its primitive position to exceed the limit of about 3°. But if the previously maintained view is correct, that the earth readjusts itself periodically to a new form of equilibrium, then there is a possibility of a cumulative effect; and the pole may have wandered some 10° or 15° from its primitive position, or have made a smaller excursion and returned to near its old place. No such cumulation is possible, however, with respect to the obliquity of the ecliptic.

It is suggested that possibly the glacial period may not have been really one of great cold, but that Europe and North America may have been then in a much higher latitude, and that on the pole retreating they were brought back again to the warmth. There seems to be, however, certain geological objections to this view.

* The area of Africa is about ·059, and of South America about ·033 of the earth's surface.

II. "On the Structure and Development of the Skull in the Urodelous Amphibia."—Part I. By W. K. PARKER, F.R.S. Received November 9, 1876.

(Abstract.)

Through the kindness of several friends* I have been enabled to work out the development of the skull in a Salamandrian type, which can now be compared with that of a Batrachian.

I was the more anxious to do this work perfectly (it had been done in part) because Professor Huxley showed me, some two years ago, certain errors in my first paper on the Batrachian skull; and I wished not only to go over that ground again, but also to have the morphology of the Salamandrian type of skull quite mastered, so that the two might be compared together.

Moreover one important error in my first paper (on the Frog's skull) arose from my taking it for granted that a certain element, the "stapes," arose similarly in the two groups. Also an impetus was given to me by the publication of Professor Huxley's article on the "Amphibia" in the ninth volume of the 'Encyclopædia Britannica,' and his invaluable paper on the skull of *Menobranchus* (a low Perennibranchiate Urodele) in the 'Proceedings of the Zoological Society' (January 1874).

In the present paper I have shown the condition of the skull in *nine* stages of the Axolotl (*Siredon*); and then, as a *tenth* stage, the skull of *Amblystoma* is given, the Salamandrian into which certain individual Axolotls pass when they take on a higher metamorphic condition.

But the earlier stages of the *cranium* of *Siredon* are well illustrated by what is seen in the lower Perennibranchs. Prof. Huxley's interesting *Menobranchus* is not nearly so low and simple a type as that here given by me, namely *Proteus*.

Moreover the truly Salamandrian *Amblystoma* is not an average kind of "Caducibranch," but differs from the majority of the species in several particulars. *Serionata perspicillata*, one of the smallest of the order, is a good average type; and happily a little larva of this species gives me an intercalary stage between my *third* and *fourth* of *Siredon*.

The materials here offered to the Royal Society are but a portion of what I could have offered; but as the bulk of such a communication would have been far greater than I can ask space for, I have here and there made reference to unpublished matter on the skull of a number of these tailed Amphibians.

After describing these skulls in their stages, and through their changes, I have made a somewhat detailed comparison of the Salamandrian with the Batrachian type of skull.

* Messrs. Günther, Flower, Murie, Mivart, A. Agassiz, Rupert Jones, and Tegetmeier, especially the last named.

Then, at the end, as I am spending my life not to illustrate the cranial morphology of this type or of that, but as digging down to find *one common root*, I have made an incipient attempt at showing what is common to the whole series of the Vertebrates—of the *brain-bearing* Vertebrates, at any rate.

It is evident that beneath the neural axis, which arises in "epiblast," there is a foundation, laid in "mesoblast," of the whole animal, from its snout to the end of its tail.

This foundation, or rather *root-stock*, is double, and each moiety lies right and left of a truly azygous structure, the *notochord*—a structure which, according to some, arises in the mesoblast also, but which, according to the latest and best observations (namely, those of Mr. Balfour), arises, in the Selachians at least, in the lowest layer, the "hypoblast."

Whether the notochord is mesoblastic or hypoblastic, at present is not of vital moment to the morphology of a vertebrated animal: the important points are that the notochord is *universal*, and that it always passes some distance into the skull.

There are several important modifications in the region of the *head*, as compared with the body generally, that make the problem of cranial morphology an extremely difficult one.

To mention some, there are:—(1) the swelling of the neural axis into three vesicles; (2) the flexure of the head upon itself; (3) the development of three pairs of sense-capsules, that press upon its sides and mingle with its structures; (4) the union of a palatal diverticulum with the brain to form the pituitary body, thus arresting the median notochord; and (5) the dying out of the pleuro-peritoneal space in the region of the throat.

Thus the modifying causes are manifold in the head of a vertebrated animal,—some of them showing their effects very early in the life of the embryo; whilst others, that relate to the specializations of the parts of the cranium and of the parts of the face, the parts that encircle the mouth and sense-capsules and that form the basket-work of the branchial apparatus—these appear later.

For details of what can be seen in the growing skull of a Salamandrian, I must refer to the main paper. Here I may remark that, while the development of the Batrachian skull seems strongly to favour the doctrine of the *facial* nature of the "*pro*-notochordal" bands ("trabeculæ"), the study of the Urodelous type suggests that they are truly *basal* (*basicranial*).

In front they are evidently facial, or belong to the visceral-arch series, and are *axial* to the premaxillary arch; but in the internasal and interorbital regions they are very probably mere continuations of the parachordal tract of mesoblast which in the trunk gives rise to the bodies of the vertebræ.

The difficulty with regard to the existence of another preoral arch is now settled; the so-called "antorbital" cartilage of Urodeles is a distinct ethmo-palatine rudiment of a visceral arch, and crops up in several groups; the pterygoid foregrowth of the suspensorium is another thing, a *symplectic process*, and not an independent arch.

The Urodeles differ from the Batrachia in having an "ascending process" to their suspensorial "pedicle;" only in *Proteus* is this absent, because only in this type does the trabecula fail to send upwards an alisphenoidal crest.

Late or *early*, the Batrachia never fail to develop an epihyal (hyomandibular) segment, and this is always specialized to form the "columella auris."

Proteus develops a large, and *Siren* and *Menopoma* a small, cartilage of this nature: it is never specialized into a columella, however, in them. In *twelve* other kinds ("Caducibranchs") I have failed to find a rudiment of this segment.

In the Batrachia, as I have shown (the fact was first pointed out to me by Professor Huxley), the stapes develops in the fenestral cleft as an independent cartilage.

In the Salamandrians, as I long ago asserted, the stapes is segmented off from the pre-existing cartilage of the floor of the periotic capsule. I erred in supposing the frog's stapes to be formed in the same way.

In the Batrachia, as I correctly showed in my first paper, a *ray* from the elbow of the suspensorium becomes detached, to form part of the external auditory apparatus; not, however, the "columella," as I supposed, but the cartilaginous *annulus tympanicus*.

That ring of cartilage I correctly referred to the category of "branchial rays," such as are seen in the Selachians: it is, in truth, their spiracular cartilage (that of the sharks, not that of the skates, which is a *free* metapterygoid segment).

As a rule, in the Urodeles, there is a suspensorio-stapedial ligment running forwards *beneath* the "portio dura" nerve; in my chief instance, the Axolotl, there is, instead of this, a fascia passing *over* that nerve to the place of the spiracular ray of the tadpole, namely, the back of the suspensorium above.

In the huge Menopome, which partly loses its gills, and in certain small true Caducibranchs (namely, *Spelerpes salmonea*, *S. rubra*, *Desmognathus fuscus*) there is a recurrence of the spiracular cartilage *over* the portio dura.

This generally attaches its small posterior end to the face of the stapes, and then is encupped by the stapedial ossification: on one side of *Desmognathus* it does this, and on the other it is distinctly ossified, and not fixed to the stapes.

This *succedaneum* for the true columella is very large in the Menopome: it is a thick but somewhat flattened cartilage, in outline like a

bell-flower, and having its broad-lipped end attached, like a snail's foot, to the top of the back of the suspensorium, and its small roundish end, or apex, set in a neat bony cup that grows from the face of the stapes.

Nevertheless the "spiracle" or "tympano-Eustachian cleft," is scarcely at all apparent in the Urodeles; in the Batrachia it is large within, but never fairly open externally.

In the Urodeles I miss entirely the copious *labial* growth of cartilage *outside* the true visceral arches; and although *Siren lacertina* has horn on its jaws, I feel certain that its mouth is never suctorial even in a very early stage.

It is not suctorial in the "Aglossal Toads," as I have recently shown, but they have a rich growth of labial cartilages.

There are some more very important facts in the morphology of the skull in the Urodeles that I am anxious to lay before the Society.

The nasal roofs are free, independent "paraneurals," and not mere outgrowths of the trabeculæ; thus they correspond with the eyeballs and ear-capsules.

The trabeculæ cranii appear much *later* than in the Batrachia, and are at first (*a*) relatively much smaller, and (*b*) have three or four times as much of their substance situated *parachordally*.

The hinder half of the basilar plate, or "investing mass," is formed as a pair of distinct cartilages. The trabeculæ chondrify four or five days later than the visceral arches, and the "parachordals" ten or twelve days later than the trabeculæ.

My earliest observations of the notochord, in unhatched Axolotl "fry," show only a moderate downbend of the apex of the notochord.

But Mr. Balfour's observations show that in Selachians it is like a sheep-hook; mine, on older embryos, show a *moniliform* condition of the notochord in front, seemingly due to pressure in growth against the pituitary gland.

I have not seen in Amphibia what Götte shows in the Bombinator Toad, namely, a cartilaginous notochordal sheath; this is well seen, however, in embryo sharks and rays.

But nothing seen in sharks and rays is more suggestive than what I am in this paper describing in the Urodeles, for they form *two rudimentary vertebral centra* behind the pituitary body.

Besides this, in several kinds, that part of the notochord which, in the "Sauropsida," is jammed in between the investing-mass cartilage, where it forms the median occipital condyle, is in certain Urodeles formed into a small, imperfect, intercalary vertebra.

This vertebra is formed of a pair of "parachordal" patches of cartilage and a tract of notochord which gets its own bony sheath; the side pieces ossify independently (in *Spelerpes rubra*), and then coalesce with the anterior part of the notochordal segment.

We have thus, in front of the vertebra which serves as the "atlas"

to carry the head, an imperfect vertebra with a broad fore end: in the adult (*Spelerpes*, *Triton*) this joint has coalesced with the head-carrying vertebra, and forms its *odontoid* process; it then resembles the *true* odontoid process of an ox or sheep.

I cannot refrain from coupling the facts above given with another, namely, that the "hypoglossal" is a spinal nerve in these and other *Ichthyopsida*. That the fore end of the notochord should vary in the cranium of different types of Vertebrata is not to be wondered at; the two evanescent vertebral rudiments in the hind cranium of the Urodeles and the intercalary joint may be set over against what I have lately made out in the Selachians.

In the Sharks the cartilage on each side of the notochord in the neck (itself ensheathed in cartilage) is some time before it breaks up into the moieties of centra, and these do not correspond in number with the neural arches.

In the Rays we have a similar state of things; but I cannot discover any segmentation of the cervical parachordal tracts, even in young unhatched skates that have acquired their final form.

In this very imperfect *abstract* I have made mention of those things that have produced the greatest effect upon my own mind, not caring so much for *order* as for *suggestiveness*. A true theory of the skull does not yet exist; it draws near, however, to the time of its delivery.

November 30, 1876.

ANNIVERSARY MEETING.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

General Smythe, for the Auditors of the Treasurer's Accounts on the part of the Society, reported that the total receipts during the past year, including a balance of £245 carried from the preceding year, amount to £17,895 13*s*. 11*d*.; and that the total expenditure in the same period amounts to £17744 15*s*., leaving a balance at the Bankers' of £126 15*s*., and £24 3*s*. 11*d*. in the hands of the Treasurer.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following Lists:—

Fellows deceased since the last Anniversary.

On the Home List.

John Joseph Bennett, F.L.S.
Rev. Joseph Bosworth, D.D.
William Sands Cox.
Campbell De Morgan, F.R.C.S.
Charles Enderby, F.L.S.
Sir Henry Percy Gordon, Bart.
John Higginbottom, F.R.C.S.
Charles Holland, M.D.
Sir John William Kaye, K.C.S.I.
Colonel Sir John Le Couteur.
George William, Lord Lyttelton, D.C.L.
Edmund Alexander Parkes, M.D.
Henry Wyldbore Rumsey, M.D.
George Poulett Scrope, F.G.S.
George Augustus Frederick Charles Holroyd, Earl of Sheffield.
Sir Francis Shuckburgh, Bart.
Francis Sibson, M.D.
Philip Henry, Earl Stanhope, D.C.L.
Lieut.-Col. Alexander Strange.
Rev. Thomas Smith Turnbull, M.A.
John Webster, M.D.
Rev. John Wilson, D.D.

On the Foreign List.

Adolphe Théodore Brongniart.
Christian Gottfried Ehrenberg.

Change of Name and Title.

Right Hon. Benjamin Disraeli *to* Earl of Beaconsfield.
Viscount Walden *to* Marquis of Tweeddale.

Fellows elected since the last Anniversary.

Henry Austin Bruce, Lord Aberdare.
Capt. William de Wiveleslie Abney, R.E.
Prof. Henry Edward Armstrong, Ph.D.
Rev. William B. Clarke, M.A., F.G.S.
James Croll, F.R.S.E.
The Right Hon. Benjamin Disraeli, D.C.L.
Edwin Dunkin, Sec. R.A.S.
Prof. John Eric Erichsen, F.R.C.S.
David Ferrier, M.A., M.D.
Col. Augustus H. Lane Fox.
Prof. Alfred Henry Garrod, M.A.
Robert Baldwin Hayward, M.A.
Charles Meldrum, M.A., F.R.A.S.
Edward James Reed, C.B.
Prof. William Rutherford, M.D.
The Right Hon. George Sclater-Booth.
Robert Swinhoe, F.R.G.S.
Prof. Thomas Edward Thorpe, Ph.D.

The President then addressed the Society as follows:—

GENTLEMEN,

THE annals of the Royal Society show that the year ending with this Anniversary presents no falling off in the value and interest of the communications brought before our meetings, as compared with previous years, and indeed surpasses them in number and extent of publications, and in demands on the time of your Council. We have been called upon more frequently than ever to aid in giving effect to those efforts for the advancement of natural knowledge which, whether originating in private enterprise or in the Councils of the State, have marked the year as a memorable one in the history of science.

Before, however, proceeding to the historical summary which this statement involves, I have to discharge the always painful task of recalling to memory the names of the most distinguished of our Fellows who have died since the last Anniversary. In Science we have lost Mr. Bennett, Mr. Campbell De Morgan, Dr. Parkes, Mr. Poulet Scrope, Dr. Sibson, and Lieut.-Col. Strange; in letters and public services, the Rev. Dr. Bosworth, Lord Lyttelton, Earl Stanhope, and the Rev. Dr. Wilson; and the names of the botanist Brongniart and the veteran microscopist Ehrenberg disappear from the list of Foreign Members.

As regards the part taken by your Council in the labours of the year now expired, I feel it to be my duty, as it is, indeed, my pleasure, to inform you, so far as the limits of an Anniversary Address will admit, of the importance of those labours—and the more so, as without this opportunity it would not be easy to make you acquainted in a way commensurate with their value with the scientific services of your Council as contradistinguished from their current duties.

As anticipated in my Address of last year, application has been made to the Treasury for a grant to cover the cost of printing the decade 1864–73 of our Catalogue of Scientific Papers, comprising now more than 100,000 titles; and I am happy in having to announce that the application was acceded to in the same handsome spirit as that in which the Lords of the Treasury, during Mr. Gladstone's administration, placed a sum upon the Parliamentary votes to defray the expense of printing the first six volumes. The value of this work becomes more and more appreciated with lapse of time; and you will be glad to learn that the continuation of this Catalogue from year to year has been ordered by your Council as a permanent part of the Society's official work. As you are aware, the expenditure for this work appears regularly in our annual balance-sheet.

Acting under a recommendation by the Library Committee, your Council offered the custody of our collection of Oriental MSS. to the India Office under certain conditions, viz. that the manuscripts which require binding should be bound, and a Catalogue made of the whole collection. The Secretary of State for India in Council has accepted the offer with its

conditions; and at a fitting opportunity the collection will be transferred to a locality where it will be in competent hands and be readily accessible to students and scholars.

Arrangements have been made for the publication of the Reports of the naturalists sent to Rodriguez and Kerguelen Islands in a separate quarto form, with illustrations; and a grant of £100 from the Donation Fund has been made in aid of the work. The botanical specimens have been named, and are being distributed to the Herbaria of Kew, of the British Museum, of the Edinburgh Botanic Gardens, and others. A complete set of the zoological collections will be deposited in the British Museum, and the remainder distributed among the Museum of the Royal College of Surgeons of London and the Museums of Edinburgh, Dublin, Oxford, and Cambridge.

The Report on the results of the "Eclipse" Expedition has been drawn up by Mr. Lockyer, and is far more satisfactory than could have been anticipated, considering the unfavourable conditions which prevailed during the whole of the time the observations were being made. It now appears that the light which photographs the prominences does not come from hydrogen, but most probably from calcium, while the photograph of the corona with the prismatic camera shows that its chief light is derived from the hydrogen. The complete account of the eclipse will appear in our 'Proceedings' very shortly.

For the financial state of the Society I must refer you to the balance-sheet prepared by our Treasurer now in your hands. It shows that our resources have been increased by receipt of the Dircks bequest, £878 12*s*. 10*d*. A further increase will occur towards the end of the year by the incoming of the £2000 Consols to which we are entitled under the will of our late Fellow, R. C. Carrington. Besides these, an addition has been made to our Trust Funds by the settlement of the long-pending question of the Handley bequest. The amount ultimately awarded to us was £6378 19*s*., the balance of which, after payment of legacy duty and certain legal charges, has been invested, as may be seen in our balance-sheet, in Reduced 3-per-cent. Stock.

The Donation Fund has been increased by the receipt of the £500 bequeathed by our late Fellow, Sir Charles Wheatstone, raising the total to £6333 10*s*. 4*d*. Additions to this fund are greatly to be desired: it is applied, as you are aware, in aid of research; and a very strict account is kept of its expenditure. Were such a fund at all what it ought to be, considering the amount of capital accumulating in this country, in great part the direct outcome of scientific inquiry, we should have fewer complaints of the insufficiency of means of encouragement for research.

To Sir Charles Wheatstone we are further indebted for a valuable collection of portraits of scientific men, including one of the devisor, and one of Boyle (by Kneller), both in oil. Mrs. Selwyn has presented the negatives of the eleven years' series of photographs of the sun-spots

(more than 2000) taken at Ely, from 1863–1874, under the late Canon Selwyn's instructions.

You will share my feeling of pleasure when I inform you of the deposit in the hands of our Treasurer of a munificent contribution, £6000, to be devoted to the aid of scientific research, by Mr. Thomas Phillips Jodrell, the founder of the Chair of Animal Physiology in University College, London, and donor of the Laboratory of Physiological Research to the Establishment at Kew. Early in last year, Mr. Jodrell informed me by letter that it was his wish to place at the disposal of this Society, as the one body in which all branches of British science are represented, this generous sum, to be applied (principal as well as interest) in any manner that the Society may consider most conducive, for the time being, to the encouragement among our countrymen of original research in the physical sciences—his object being not, on the one hand, to found a permanent endowment for the benefit of a future generation, nor, on the other, to relieve the Government of any part of its obligations to the present, but to ascertain, as far as may be, by practical experiment on a limited scale, to what extent the progress of original research in the physical sciences is retarded in this country by the want of public support to those engaged in it, and in what form an increased measure of such support would be most likely to promote its development.

I need hardly add that your Council, before whom I laid Mr. Jodrell's letter at once, thankfully accepted his offer, and appointed a Committee to consider and to report upon the best means of giving effect to his liberal views. Before, however, the Committee had presented their report, we were informed of the intention of Her Majesty's Government to increase largely the funds placed at the Society's disposal in aid of scientific investigations, and to allow part of the increment to be devoted to the sustentation or remuneration of investigators—thus fulfilling the main desire which Mr. Jodrell had in view in making his donation.

When I communicated the intention of the Government to Mr. Jodrell, he signified his desire to reopen the question of the application of the £6000, which he still wished to leave in our Treasurer's hands; for his object had been to induce the Government to do what, to the surprise of every one, it had done, and not to supplement a permanent government endowment by a temporary one of his own. Whatever might be the ultimate decision, he did not doubt that this Society would be the most competent agency for carrying it into effect; and he suggested that the fund should be invested temporarily, and the question of its appropriation reserved until we should meet this session. Finally, Mr. Jodrell has proposed that the gross sum should be retained in its present investment in the prospect of some want of it arising in the course of the next few years, and that the interest accruing in the mean time should be applied by the Society as part of our revenue. This proposal was willingly

accepted, and the best thanks of your Council have been presented to Mr. Jodrell.

In April last I was informed by the Lord President of the Council that Her Majesty's Government had under consideration the question of giving further aid to scientific research, by increasing the Parliamentary grant of £1000 per annum which is administered by the Council under the recommendation of the Government Grant Committee in aiding investigators with apparatus and assistance. They proposed in future to augment the Grant annually for five years by £4000, to vest the administration of the whole in the Science and Art Department, and to invite the Society's Council to aid Her Majesty's Government, as hitherto, with advice and assistance as to its appropriation and expenditure, and further to give us the power of recommending, in certain cases, the payment of personal allowances to investigators. The communication also advised that the Presidents of fifteen learned bodies in the United Kingdom should be *ex officio* members of the Government Grant Committee,—a change in its constitution more apparent than real, as the majority of the Presidents specified were already Fellows of the Society. After several conferences with the Minister, the original proposal was, with his concurrence, modified, and made to apply to the additional £4000 only, the administration of the original £1000 remaining as heretofore, to be accounted for to the Treasury, and the recommendations of the Council with respect to the appropriation of the additional sum to be liable to revision by the Lord President, in whose department the vote is taken, and who must be responsible to Parliament for its expenditure. With this proposal your Council concurred, on the understanding that should it happen that the Lord President found it inadvisable to act upon all your Council's recommendations (which, in his Lordship's opinion, is never likely to happen), the Council should have the opportunity of revising them, so that, if thought desirable, the items of the grant to which exception had been taken might be allocated in some other way.

There are therefore now two Government grants in aid of scientific research, one of £1000 per annum, for the administration of which your Council is directly accountable to the Treasury, and which, as heretofore, will be appropriated to the providing of instruments and assistance for scientific inquirers: the other, of £4000 annually for five years, to be applied to the aid of investigators, not only by providing instruments and assistance, but occasionally by personal allowances or grants of money, in accordance with recommendations to be made to the Lord President.

The constitution of this new Committee is not yet settled; but it will probably consist of the existing one, together with all the *ex officio* members as proposed.

Before dismissing this subject I feel it to be incumbent on me to express our obligations to His Grace the Duke of Richmond and to Lord Sandon, for the active interest they took in providing the grant, and for the liberal manner in which they entered into the views of the Council in respect of its appropriation.

Two of the provisions of the Vivisection Bill called forth an earnest remonstrance from your Council, which was communicated by the President to the Prime Minister in June last. These provisions were the limitation of experiments, even under anæsthetics, to such only as can be shown to contribute directly to the prolongation of human life and the alleviation of human suffering; and the prohibition of experiments upon dogs. It was pointed out, in the communication to the Minister, that, as regards both these limitations, the Bill went beyond the recommendations of the Royal Commissioners upon Vivisection for scientific purposes; and, in respect of the first of them, it was represented that the history of physical science shows that all the great discoveries which have contributed to the welfare of mankind have resulted from investigations pursued in the interests of pure science, without reference to their practical application, and that to this rule physiology forms no exception, since all the physiological truths which constitute the foundation of the rational practice of medicine have been ascertained by experiments upon living animals, conducted by persons actuated by that desire for the advancement of natural knowledge which the Royal Society was instituted to foster; and it went on to say:—"Profoundly convinced of the mutual dependence of all branches of physical science, the President and Council feel that any legislation which arrests the development of one is an injury to all, and they would lament the admission into the Statute-book of a principle which is essentially antagonistic to the progress of all Natural Knowledge."

With respect to the second provision it was urged that, while the Bill professed to regulate experiments only, it prohibited them in the case of dogs, although the constitution of the dog is such as to render it indispensable for some of the most important physiological problems.

The receipt of this letter was followed by Lord Beaconsfield's communicating with me on the subject, when I had the opportunity of representing the views of the Council as being unalterable as to the necessity of modifying, if not of rescinding, these two provisions. The Prime Minister promised and gave full and, as it has proved, favourable consideration to the Council's representations; for, before the third reading of the Bill, its provisions were so modified as to place no obstacle in the way of experiments on all animals for purely scientific purposes by properly qualified persons.

On a subsequent occasion, when time did not allow of my communicating previously with the Council, I ventured, in the name of the

Society, to request an audience with the Home Secretary, principally on the subject of the clauses that limited the making of experiments to registered localities, thus preventing physiologists from pursuing their researches during their vacation travels, or at their temporary residences at watering-places and other localities in which no registered institution existed. On this occasion also I found a willing ear lent to the Society's voice, followed by a favourable consideration of our representations, special certificates being now procurable which enable the experimenter to pursue his researches wherever he may be. On the same occasion I urged the confining the operation of the Bill to warm-blooded animals, but with only partial success; the provision which extended to all animals was finally curtailed, so as to apply to the vertebrate class only. Lastly, a protest against the clause compelling all experimenters to transmit to the Secretary of State a detailed report of all the experiments they might undertake, and their results, was more successful; for the Bill now requires reports to be made only when called for by the Secretary of State.

The Loan Collection of Scientific Instruments.—In my address of last year the proposed action of Government in reference to this important object was stated, together with the opinion of the Lord President and Vice-President of the Council, that it might prove the means of carrying out that recommendation of the Science Commissioners which dealt with the want of a Museum illustrating methods of experimenting and means of observing (see 4th Report, § 93). This was followed by a letter from the Duke of Richmond addressed to myself, suggesting that the Scientific Societies should organize in connexion with the Exhibition a series of Conferences, similar to the sectional meetings of the British Association. This led to that brilliant gathering in May last of scientific men from the metropolis and all parts of Europe (not fewer than thirty-five from Germany alone), and from America, many of them charged by their Governments to report on the collections, and to those public lectures on the instruments and apparatus displayed by many of the most eminent of these scientific men, which imparted such value and interest to the Exhibition.

Among the objects so exhibited, amounting to 20,000 in all, were to be seen specimens of the work or evidences of the genius of a considerable proportion of the eminent scientific men and manufacturers of scientific instruments from the days of Tycho Brahe and Galileo down to the present day, together with a collection of the appliances for scientific teaching adopted in many countries.

It cannot fail to be a matter of congratulation that the objects lent by the Royal Society were not surpassed in scientific value or in historic interest by those of any other institution or country, though among these are the Conservatoire des Arts et Métiers of Paris, and

the Museums of Berlin, Florence, and Haarlem. We contributed twenty-seven articles, all of the best construction of their day, and which may be regarded as monuments of the skill of famous makers. They include:—Boyle's air-pump with double barrel, presented by himself in 1662; Newton's original reflecting telescope, constructed by himself in 1671; Huyghens's aerial telescope, with three object-glasses, of 122, 170, and 240 feet focal length, presented respectively by himself in 1691, by Newton, and by the Rev. G. Burnet; a large levelling-instrument used in the Ordnance Survey; two chronometers by Arnold, which were taken round the world by Capt. Cook; Capt. Kater's hygrometer; Priestley's electrical machine; and Sir H. Davy's experimental Safety Lamp.

The interest excited by the Exhibition is best shown on the one hand by the number of visitors, which at the end of September amounted to a quarter of a million, and on the other by the efforts made by a large body of scientific men, who desire to see effect given to the views of the Lord President in founding a permanent Museum of this nature. A memorial to this effect, signed by more than one hundred scientific men, has been addressed to the Duke of Richmond, representing the advantages of a Museum of Scientific Apparatus, Appliances, and Objects, and of Chemical Products—illustrating both the history and the development of Science—with which the objects now contained in the "Patent Museum" should be incorporated. Among the advantages enumerated which would accrue from such an institution, are the saving of time and labour to investigators, assisting teachers, informing constructors of philosophical instruments as to the directions in which reproductions are wanted, or in which improvements may be effected, and possibly the lending instruments to investigators under suitable restrictions.

With regard to the advantage of combining the objects of the Patent Museum with the general collection, it is pointed out that the value of these objects as mere subjects of a patent is very imperfectly represented by their separate exhibition, whereas it would be greatly enhanced were they placed in juxtaposition with instruments of the same nature, which, though unpatented, may be both better adapted to their purpose and of greater instructive value.

The Meteorological Office.—In my last year's Address I stated that the Lords Commissioners of the Treasury had appointed a Committee to inquire into the working of the Meteorological Office, and the value of the results hitherto obtained by it, and that the result of this might afford to Her Majesty's Ministers the opportunity of adopting measures that would greatly increase the scientific efficiency and public interest of that Office.

The labours of the Treasury Committee are now concluded. It sat frequently during the whole of last session of Parliament, examined many witnesses, scientific and practical, including the most eminent

meteorologists of this country whose attendance could be obtained; and a report has been drawn up which will shortly be laid before Parliament. It will include the answer of the Committee of this Society to a letter addressed to the President and Council by a Committee of the Treasury requesting information on the following points:—1. As to the extent to which the objects indicated in the Reports presented by your President and Council in 1855 and 1865 for the guidance of the Office, had been attained by means of the labour and publications of the Office. 2. How far they had led to a better knowledge of the laws governing the weather, and to the discovery of new laws. 3. How far they had led to the collection of data, not otherwise procurable, that form a necessary basis for the establishment of new laws. 4. Should the same objects be further pursued? and if so, in accordance with the programme of operations now in force? 5. Should a change in the programme of the operations appear desirable, what should their nature be?

The Committee of the Royal Society, which consisted of Sir G. Airy, the Rev. R. Main, Professors Adams, Stokes, and B. Stewart, Dr. Guy, Messrs. De La Rue, Warington Smyth, Broun, and Spottiswoode, sent in a series of recommendations to the Council that were embodied in a letter addressed to the Treasury Committee.

Your Council reported that oceanic meteorology had been greatly enriched by the investigations made with regard to winds, currents, and temperatures of the ocean, and by the deductions obtained therefrom, and that these contained results on which sailing-directions of the most trustworthy character, for the use of navigators, can be constructed.

As regards terrestrial meteorology, that the number of stations for which observations are signalled for the purpose of forecasts should be increased rather than diminished; and that these forecasts are justified by the results of three-fourths of the cases recorded.

That daily weather-charts are considered to have contributed materially to a diffusion of the knowledge of meterological phenomena among all classes, and are on that account of great utility; and, further, their preparation and issue are regarded as beyond the means of private establishments, and eminently worthy the support of the Government. That the publications of the Office generally bear the impress of a scrupulous regard to accuracy, and embrace a collection of data not otherwise attainable, and supply a large mass of material of a nature indispensable to the establishment of new laws; and that all the work appears to have been done in accordance with the recommendations of the Royal Society.

As to the future of the Office, your Council were of opinion that the programme now in force should be generally followed; that the hitherto unpublished results of oceanic observations should be brought out as soon as possible, so that the meteorology of all navigable parts of the ocean

should be known; that the rules followed in forecasting storms should be published for the information of future meteorologists; that it would be advantageous to publish weekly averages of the climate of the British Isles for the use of agriculturists and collectors of statistics of health, mortality, and the distribution of disease, these averages to be printed in a tabular form, giving the results not only of the week but of the previous week, of the corresponding week of the foregoing year, and of the average of the corresponding weeks in the foregoing 10 years. That the operations of the self-recording observatories should be continued as at present, until the expiration of the 12-year sun-period, after which the subject of their number and position might be advantageously reconsidered; and that special observations should at once be taken for the purpose of comparing their records, ascertaining their local peculiarities, and determining such constants as would ultimately permit of a large reduction of their number; and with regard to the eye-observing stations, it was recommended that their position and number should be reconsidered, with the view of obtaining a closer approximation to the meteorological conditions of the British Isles.

More important, however, by far, than these recommendations relating to the collection and reduction of observations, is the expressed opinion of your Council that the most practical method of advancing meteorology is by endeavouring to place the science on a firm basis, not by the accumulation and digestion of observations, but by research and experiment—and that this can only be done by the Government securing the services of scientific men who can devote their time to this object. To this end your Council recommended that the Office should be presided over by a man of the highest scientific attainments, preferably as sole head of the office, and if not, as chairman of a committee composed, like the present, of men eminent in science, but fewer in number—and that an adequate salary should be given to the presiding head, if an individual, or to the members of the committee, if it be retained.

Such is the substance of the recommendations of your Council; and I have every reason to hope that they will be carried out by the Government in as liberal a spirit as were our previous recommendations for the guidance of the Office in 1855 and 1865. Should this be the case, we may expect to be applied to for suggestions as to the general or precise nature of the researches and experiments which your Council have indicated as being essential for placing the science on a firm basis. We have excellent examples of what may be expected from such researches in the essays of our late Fellow, Professor Daniell, and in the more recent contributions to meteorological science of Sir J. Herschel, Balfour Stewart, Tyndall, Strachey, and others; and we look to further improvements from the application of the study of hydrodynamics and the phenomena of light, electricity, and acoustics, and other branches of physical science,

to the elucidation of the many unsolved problems which have so long fettered the investigation of the laws of climate.

It will not be thought out of place here if I add a few remarks on the present state of Meteorology as one of the physical sciences, the progress it has really made, and the direction in which further progress is attainable. In this I have been aided by General Strachey, a late member of the Treasury Committee, who, having studied meteorology in India as well as in Europe, has kindly drawn up a statement of our views, and placed it at my service for this Address.

Without question, the chief point in which meteorology now differs from what it was, is the recognition of the necessity for taking into consideration the facts observed at many places simultaneously over a large area, instead of facts observed in succession at a single locality. This great step has been no doubt mainly due to the extension of the electric telegraph, which renders possible the rapid juxtaposition of observations made over a very large area, and the equally rapid dispatch to great distances of the results derived from the consideration of such observations, thus furnishing the means both of acquiring the knowledge and of making it practically useful. A comparison of the first feeble efforts to appreciate the nature of the fluctuations of barometric pressure recorded in the Reports of the British Association for the years following 1843, and chiefly due to Mr. Birt, and the beautiful synoptical charts now published in many countries, of which those prepared by Captain Hoffmeyer may be taken as an example, will indicate the great progress made in this direction. Charts such as these convey very complete information as to how the chief variations of weather occur over the greater part of Europe and the United States—though why they occur is yet too little understood. It is, unfortunately, still true that very little has been done towards tracing out the physical causes of the changes of pressure of the occurrence of which we are thus made aware; but it is not to be doubted that, the facts being now presented to students in a readily accessible and intelligible shape, no great interval is likely to elapse before the causes that produce them are ascertained, at all events approximately.

It is practically certain that the changes of atmospheric pressure are immediately dependent on changes of temperature; but no intelligible relation has yet been established between the two, except in the very vaguest manner. And this indicates the first great want of scientific meteorology—namely, an improved theoretical knowledge of the movements of elastic fluids subject to changes of temperature. The difficulties to be surmounted in this branch of mechanics are great; but probably the means may be attained of subjecting the hypotheses that will eventually form the basis of scientific meteorology to the rigorous test of mathematical calculation, though hardly the first step has yet been taken in that direction.

For the purpose of bringing one class of the observations on which a scientific meteorology must be based into a form suited for the application of theoretical tests such as these, the harmonic analysis seems to supply the necessary means. This method may be familiarly explained as having for its object to break up any observed series of quantities representing a recurring phenomenon, such as the diurnal or annual variations of temperature or atmospheric pressure, into other series so arranged that each observed quantity shall be conceived to be an aggregate consequence of a number of different series of variations from the mean value—the first of such series being completed but once in the whole epoch under consideration, the second recurring twice, or being completed in half the epoch, the third recurring thrice, or in one third of the epoch, and so forth.

The arithmetical computations requisite for thus transforming periodical observations being very laborious, Sir W. Thomson (adopting an idea of his brother's, Prof. J. Thomson) has proposed to construct a machine that shall perform the calculations with the aid only of the graphical projection of the curve resulting from the recorded observations—an illustrative model of which he exhibited to the Society in the course of last Session, and an account of which has been published in our 'Proceedings.'

It is well to remark that this treatment of meteorological or other observations gives no direct aid in referring the phenomena to physical causes, and is only to be regarded as a means of bringing them into a shape in which they can be compared with theoretical formulæ or dynamical or other hypotheses. It has too long been thought that the arithmetical manipulation of the results of meteorological observation was a sufficient end to be attained; and too often the necessity for seeking for the efficient causes of the phenomena has been lost sight of. An altogether useless refinement has also frequently been insisted upon in recording observations of what, in a scientific sense, may be termed insignificant details; and a wholly illusory appearance of accuracy has been aimed at, far beyond what can in fact be attained. The true conception of averages is, in meteorological calculations as in many others, very often missed, and mean results are exhibited which have no real signification.

The relation of meteorology to physics may be compared to that of the natural-history sciences to physiology. Physics include the study of the forces the operation of which on the atmosphere gives rise to meteorological phenomena. The intelligent application of physical research is unquestionably one of the most necessary elements in the satisfactory progress of this science; and Professor Tyndall's study of the action of the air and other gases in relation to radiant heat affords an excellent illustration of the manner in which experimental investigation contributes to the knowledge required to explain atmospheric phenomena.

The prodigality of nature in supplying the germs from which life on the earth is sustained, and the comparatively extremely small proportion

of those germs that ever come to maturity, has often been a subject of comment. I venture to remark that a prodigal waste no less conspicuous is to be seen in the long rows of volumes on our shelves containing meteorological observations which doubtless contain vast numbers of scientific germs, but germs not destined to fructify. It is without doubt one of the most serious difficulties that attend our efforts at progress in this science to determine what records to make, what to keep, what to publish. So long as our knowledge is so rudimentary, we cannot properly judge what is essential and what unimportant. Like many other difficulties, this, I presume, must be left to time for its solution; at all events, I shall not attempt it.

In concluding my observations on this subject, I would further impress upon all interested in it that it is to well-directed thought on the physical connexion between the many closely related atmospheric phenomena which are now so clearly presented to the student in the publications of the present day, that we must look for real progress in the science—and that it will almost certainly be found that it is rather through an examination of the better-known recurrent phenomena, viewed broadly, that success will be secured, than by a laborious search after deviations from what is of common occurrence.

The most important scientific incident of the year is unquestionably the return of the '*Challenger*' from her voyage round the world and three years and a half of persevering exploration. It is, moreover, one in which the Royal Society has taken the deepest interest, having (as the Lords of the Admiralty officially state) originated it, and having been called upon by the Government to take a very active share in advising as to its organization and equipment.

The 'Challenger' left Sheerness on the 17th of December, 1872, and, after a voyage of 69,000 miles, arrived at Portsmouth on the 24th of May last with all her officers (with the exception of Captain Nares, who was called home to command a more perilous enterprise, and the late Willemöes-Suhm, who died at sea) in excellent health—their labours crowned with complete success, their collections in perfect order and preservation, and the scientific staff eager to work out the results of their long and arduous investigations. The success of this expedition is the more gratifying, as it is the first and only enterprise of its kind which has been undertaken by any nation for carrying out a thorough and purely scientific investigation of the great oceanic areas, under adequate superintendence and with full appliances.

It is impossible for any one who has not taken an active part both in the organization and conduct of such an expedition as this of the 'Challenger,' to estimate the number and value of the factors that have mainly contributed to its success. Foremost among these were the wise liberality of the Ministry, which gave orders for its outfit being

complete on all points, and the care and efficiency of the Naval Department, shown in the attention paid to every detail, in the choice of the ship, its stores, in the selection of the Commander, its executive officers and crew, and in the forethought bestowed on its sailing-directions. The selection of the Scientific Staff (consisting of Sir C. Wyville Thomson, Mr. Wild (Secretary), and Messrs. Murray, Moseley, and Dr. Willemöes-Suhm, naturalists, and Mr. Buchanan), and the apportioning of their duties, was intrusted to your Council; and the manner in which that Staff has carried out its instructions, merits your highest approbation—as is, indeed, testified by the award of a Royal Medal to its Chief, Sir C. Wyville Thomson. Essential to complete success as all these requirements were, they would have been wholly unavailing but for another, which no foresight could provide for and no forethought guarantee; and that is, concord! The trials of social life on shipboard are proverbial; and, according to the early traditions of the naval service, a philosopher afloat used to be considered as unlucky a shipmate as a cat or a corpse. In this case, thanks to the admirable spirit in which the Commander and his executive worked with the head of the Scientific Staff and his subordinates, I am informed that harmony reigned on board throughout the voyage. And *à propos* of this, I may be allowed here to allude to another prejudice which was once (if it be not still) current in the service, and which I hope the experience of both the 'Challenger' and Polar ships will finally dispel—namely, that to have one mess-cabin only for the Commander and his officers would be incompatible with naval discipline. The contrary practice in both these Expeditions has, as I am assured, been attended with the happiest results—and this notwithstanding the addition to the mess of that dreaded element, the philosophers.

Before proceeding to glance cursorily at some of the unpublished results obtained by the 'Challenger,' I must direct your attention to the number and value of the scientific documents which have been from time to time sent home for immediate publication; for in respect of work published during the progress of the voyage this expedition stands quite alone. I refer especially to the seven folio Reports of proceedings by Capt. Nares and his successor Capt. Thomson, including twenty-nine charts of sections of the great oceans, with soundings and isotherms for all depths from the surface to the bottom. These publications, the issue of which we owe to the zeal and assiduity of the late and present Hydrographers of the Admiralty, are of the greatest interest—the seventh especially, which gives a complete *résumé* of the observations obtained over the whole Atlantic, and deals very ably with their results. It shows that this ocean presents three deep basins, separated by suboceanic ridges. Of these basins, one (the Eastern) extends along the coasts of the Old World, following its main sinuosities from the latitude of Great Britain to that of South Africa. The other two together occupy a somewhat similar

position along the coasts of the New World: namely, a northern one extends from lat. 60° N. to 10° N., expanding greatly in the tropics; and a southern one, commencing a few degrees to the south-eastward of the other, extends far into the Antarctic basin, of which it may be regarded as a northern prolongation. Between the eastern and two western basins a comparatively narrow belt of suboceanic highlands extends from the Arctic to the Antarctic circle along a sinuous line which, roughly speaking, occupies a mid-channel course.

Of the oceanic islands, the Azores, St. Paul's rocks, Ascension, and Tristan d'Acunha are emerged peaks of these highlands. The Bermuda Islands rise out of immense depths in the N.W. basin; Fernando de Noronha and Trinidad Island (in 20° S. lat.) rise out of the S.W. basin; while not more than one island (St. Helena) is to be found throughout the whole length of the Eastern basin.

Not only is this discovery of great importance in relation to the suboceanic distribution of life, but also in reference to theories of the distribution of land-animals and plants. In the present state of our knowledge, it disposes of all speculations as to the former existence of tracts of now submerged land, which, extending from the great continents to the islands in question, might have aided the migration thither of animals and plants; and it obliges us to conclude that they were peopled with living things by the direct or indirect agency of the elements.

Did time allow, I would have directed your attention to the discussion on Oceanic Circulation contained in these Reports—a subject that has produced, within a very few years, a library of scientific literature, in which the names of Carpenter, Croll, and Wyville Thomson will ever hold a high place,—as also to the memoirs contributed to our 'Transactions' and 'Proceedings' by Thomson, Willemöes-Suhm, Moseley, Buchanan, and Murray.

The as yet uninvestigated materials collected by the Expedition include soundings, dredgings, and trawlings at the surface, bottom, and intermediate depths, from 354 stations in the Atlantic, Pacific, Southern, Antarctic, and Pacific Oceans, and in the China Sea and Malay archipelago, all which have to be studied in connexion with simultaneous observations for the temperature, specific gravity, chemical composition, and movements of the sea-water at these stations, and with others relating to the mineral matter covering the floor of the ocean.

Sir C. Wyville Thomson informs me that, as yet, no close estimate can be formed of the number of specimens fit for mounting for museum-purposes which were collected in the deep sea; but he thinks that 100,000 would be well within the mark; and this is of course exclusive of microscopic organisms. Being collected over a vast area presenting comparatively very slight variations in physical conditions, the general character of the fauna which they represent is, as might be expected, on the whole uniform. At the first glance it seems to consist of a multitude

of closely allied forms, requiring in many cases great care and skill to determine what among them should be regarded as types of species, and what as local or accidental forms of one species.

In the collection of the abyssal fauna, Sponges and Echinodermata predominate; and Sir C. Wyville Thomson expects that at least one half of these consists of undescribed and newly discovered species—an opinion in which he is fortified by competent judges who have inspected the collection. Crustacea, Annelida, and Polyzoa are also well represented, and are to a great extent of new and remarkable forms. Fishes are numerous, but are for the most part referable to families already made known by the memoirs of the late Rev. R. Lowe on the Fishes of Madeira.

All the specimens have been preserved in such a way that they can be investigated anatomically in every detail; and those who visited the biological laboratories on board the ship at Sheerness, and saw the extent and nature of the appliances for the preservation of soft animals of all sorts, may readily understand how rich a harvest awaits the reapers who have sown so diligently.

Considering how liberal has been the action of the Government hitherto, there cannot be a doubt of Sir Wyville Thomson's being placed in a position that will enable him to superintend the publication of the results of this Expedition on a scale and with a completeness commensurate with their value and worthy of the nation. An unequalled opportunity is now afforded him of investigating the phenomena of migration, variation, of the first appearance, succession, multiplication, and extinction of forms belonging to many orders of the animal kingdom—and this over areas so extensive that they may be regarded as, in a certain sense, the equivalents of geological periods. For this purpose it appears essential that the collections should be kept together under the eyes of the naturalists who formed them, until every species and variety has had attached to it all the details respecting its habitat and environing conditions that were obtained when it was collected; otherwise the primary object of the Expedition will be frustrated.

It may appear superfluous to suppose that any other course would be possible under the circumstances; but that it is not so is proved by the fact that, many and important as have been the collections made during voyages of discovery and survey which have been dispatched from our shores and brought to England, there is absolutely not one of them, from the days of Cook to the present time, of which, so to speak, any thing like the whole material has been published. True enough, this has in some cases been attributable to a want of energy on the naturalists' parts; but it has far more frequently been due to the parsimony or indifference of the Government, which has refused the opportunity of study, or the means of publication, or both.

Before leaving this subject, I must mention the endeavour of our Fellow, Mr. Sorby, to determine the nature of the Red Clays of the ocean-bottom,

of which we have heard so much. He informs me that, though any conclusions now to be drawn from his observations must be provisional, it is safe to consider that many specimens of the Red Clay are so entirely analogous to what the Gault must originally have been, that those specimens might almost be looked upon as being as truly modern Gault as the *Globigerina*-ooze is modern Chalk. In the Gault the grains of fine sand are chiefly quartz derived from the decomposition of schistose rocks. But the Pacific and Atlantic muds from great depths contain, besides quartz-fragments, others of glassy felspar, pumice, and other volcanic products; and Mr. Sorby has not been able to detect any difference between the main mass of the Gault and other rocks which are composed of very minute granules like those derived from felspar or other minerals which, in a similar manner, easily undergo complete chemical decomposition. Independent, therefore, of the presence of different organic remains, and of the modern volcanic products, there is little or no difference between the Red-Clay deposits and some of the earlier stratified rocks.

The return of the *Polar Expedition* is too recent to allow of any accurate estimate being formed of the value of the scientific facts which it has accumulated. Captain Nares, in his official Report to the Admiralty, bears warm testimony to the services (both as a collector and an observer) of Captain Feilden, who was selected by your Council as Naturalist to the Expedition; and we have very good reason to believe that his and Mr. Hart's contributions to Arctic Geology and Natural History generally will prove to be the most important and extensive ever obtained from the highest latitudes of the globe.

From a communication with which Captain Feilden has favoured me, it appears that there are no signs of a cessation of animal or vegetable life up to the furthest point reached by the Expedition: birds and mammals occur on the shores of the Polar basin in lat. 82° 45′; and the sea itself abounds in Crustacea and Mollusca, which latter were collected in a fresh state chiefly on the recently raised beaches. Of land mammals, the Lemming and its enemy, the Ermine, were found on the North Greenland coast, between the parallels of 82° and 83°, along with twenty or thirty species of flowering plants, including the beautiful *Hesperis Pallasii*, *Saxifraga flagellaris*, and *Vesicaria arctica*. The absence of whales from Smith's Sound was a noteworthy fact: we may assume that the great *Mysticetus*, which is almost extirpated in the Spitsbergen seas, and which was traced up Baffin's Bay and to Prince Rupert's Inlet, is now hemmed in by the polar ice of Bank's Straits and McClintock's Channel, and did not attempt to face the pack of Smith's Sound. Birds, which abound in Baffin's Bay, were scarce in the Sound, owing to the cold tides and want of open water in the Polar basin; nevertheless the Knot, the Sanderling, and the long-tailed Skua Gull were all observed to breed on the shores of that basin.

Insects were found at the extreme point reached by Captain Feilden; and, of the lower orders, Echinodermata were very common. Among these is a beautiful *Comatula*, identical with one dredged up in 82° 6′ by Captain Buchan, in the 'Dorothea,' in 1818, and afterwards by Franklin in the 'Trent,' in lat. 82° 26′. As the latter localities are on the east coast of Greenland, and this species had not been found previously in any part of the American Polar sea, another reason is suggested for concluding that Greenland is an island, and that the coast traced to the eastward by the sledge expedition of the 'Alert' is truly its northern one.

The geology of Smith's Sound is very instructive, Captain Feilden having succeeded in laying down its outline, at any rate, and connecting its rocks with some of those of the Polar regions to the south. Gneiss, syenite, and hornblende rocks extended from Cape Isabella, in lat. 78°, to Hayes Sound, in lat. 79°, where they were overlain by marine beds of Silurian conglomerates, full of fossils, dipping E. and W., and reaching northward to Cape Collinson. On the Greenland coast, in Bessel's Bay and Petermann Ford, the same rocks are found. On the opposite coast, in Discovery Bay, these fossiliferous rocks, if they ever existed, must have been denuded, and are replaced by azoic slates and limestones presumably answering to the Silurians of American geologists. This formation was traced to lat. 82°, where an anticlinal ridge occurs, the northern strata of which dip to the N.N.E., and are, in lat. 82° 44′, overlain by Carboniferous limestones.

Miocene strata were discovered near Discovery Bay, in lat. 81° 44′, including a 20-foot seam of coal rich in fossil plants. Postpliocene beds full of shells, and sometimes 400 feet thick, filled up the valleys, and overlay hills 100 feet high; these contained bones of the musk-ox and seal, together with drift wood, all deposited as they might have been under existing conditions.

Drift pine wood abounds on the shores of the Polar Sea, no doubt drifted from the Siberian rivers; and birch wood occurred in the Sound.

Evidence of a recent change of climate was met with in the number of deserted Eskimo settlements, which were traced nearly as far north as the parallel of 83°. One of the houses was roofed with large whales' ribs.

I have now, Gentlemen, concluded my endeavour to bring under your notice some of the principal labours of your Council during the past year, together with their immediate and prospective results. I should have liked, had time permitted, to direct your attention to a few of the more interesting papers and experiments that have been brought before us at our evening meetings, and to point out to you that in the consideration and preparation of papers for publication a heavy burden is laid on your Secretaries and that longsuffering body the Committee of Papers. It would perhaps surprise you could you be made aware of the amount and importance of the work connected with papers which is performed by

your officers and the Committee. It is work which only in its results comes before the eye of the Society; but I think you will agree with me that those results show how well and faithfully the work has been done.

On the motion of Sir James Alderson, seconded by Mr. Francis Galton, it was resolved—"That the thanks of the Society be returned to the President for his Address, and that he be requested to allow it to be printed."

The President then proceeded to the presentation of the Medals:—

The Copley Medal has been awarded to Professor Claude Bernard, For. Mem. R.S., for his numerous contributions to the science of Physiology.

It fell to the lot of Claude Bernard to make, at about the same time, two discoveries of which it may be said that they have proved more pregnant of physiological interest than any two discoveries which have been made by the same man during the last five-and-twenty years. Not only were the discovery of the glycogenic function of the liver, and of vaso-motor nerves, of prime importance at the time at which they were made, but their subsequent influence on the progress of physiology has been such that it would be difficult to overrate it.

When, in 1853, Bernard published his work 'Sur une Nouvelle Fonction du Foie,' physiologists, notwithstanding the proof afforded by Liebig and others that animals are able to form fat out of the starch and sugar of their food, still clung with remarkable tenacity to the view that the great distinction between animal and vegetable life lay in the fact that the chemical actions of the former were exclusively destructive, and of the latter constructive. When, however, Bernard showed that the hepatic cells were able, like the vegetable cells, to manufacture and deposit in themselves a veritable *starch*, the older view received its death-blow; the constructive powers of the animal economy could no longer be denied, and the minds of physiologists became open to the fact that in studying animal nutrition they must be prepared for the existence of other processes than those of simple destructive oxidation. The subsequent discovery of glycogen in other (and especially in fœtal) tissues made this still more clear. How beneficial this clearing-away of erroneous theoretical conceptions has been is shown by the rapid progress which the physiology of nutrition has made during the last quarter of a century.

The discovery of glycogen has also another influence of a general character. Governed too much by the leading idea of the animal body being composed of organs with special functions, physiologists were content with the view that the liver was an organ whose function is to secrete bile, and that when it had secreted a proper quantity of bile its work was done. The fact that in the liver, at the same time that bile was being secreted, chemical labours of an apparently wholly different

kind were being carried on, put an end to these narrow conceptions. It was felt at once that a new path of inquiry had been opened up for the study, not only of hepatic, but of all other tissues—a path of which even yet we see only the beginning.

Though such theoretical considerations as the foregoing stamp the discovery of glycogen as emphatically epoch-making in the history of physiology, its immediate and practical fruits were not inconsiderable. It and the subsequent discovery by Bernard that puncture of the fourth ventricle produces a temporary artificial diabetes, at once threw a vivid light over the dark subject of diabetic disease; and if neither the labours of Bernard himself nor those of Pavy and others, who have extended and, in a measure, corrected Bernard's conclusions, have cleared up the whole mystery of this fatal malady, its rational pathology began with the discovery of glycogen; and the complete interpretation of it, when it comes, must be based on Bernard's results.

No less epoch-making than the discovery of glycogen was the observation made by Bernard in the early months of 1852, that division of the cervical sympathetic caused a dilatation of the blood-vessels of the face and neck. That simple experiment was the beginning of the long series of researches on vaso-motor nerves, on nerves of secretion, we may perhaps add nerves of nutrition, and on inflammation, which so eminently characterize the physiology of the present generation. The progress of physiology during the last twenty years has been far more rapid with respect to our knowledge of the laws regulating vascular supply and secretion than in any other direction. Nor is the value of Bernard's initial experiment lessened by the fact that in a later month (August) of the same year, Brown-Séquard had independently obtained similar results to those of Bernard, and had pushed them further than he had, nor by the fact that Waller in the same year had seen the importance of the new truth more clearly than Bernard himself seems at first to have done. The air of physiology was at that time heavy with some such discovery; and since Bernard not only was the first to call attention to the facts, but also subsequently expounded fully their importance, his merit in the discovery cannot be diminished by others having independently arrived at the same results.

But Bernard's merits as a physiologist do not end here. Second only in importance to the discovery of glycogen and vaso-motor nerves was the observation made by him in 1856, and at about the same time independently by Kölliker, that the South-American arrow-poison, urari, destroys the conductivity and irritability of motor nerve-endings, but leaves muscular contractility intact. This was of great theoretical importance, inasmuch as it afforded striking evidence in support of Haller's views on muscular contractility, views which had been somewhat thrown into the background; and though the opinions expressed by Bernard in publishing this important discovery have not been fully con-

firmed by subsequent inquiries, the fact which he and Kölliker enunciated, that profound differences exist between the action of the poison on the contractile tissue itself and its action on the endings of the motor nerves, remains as a fundamental doctrine of physiology. The discovery of the properties of urari had, moreover, all the contingent advantages of the invention of a method. Urari has proved of indispensable advantage as a means of physiological analysis; its use in this respect is second only to that of chloroform and other anæsthetics. Many of the most important results in physiology gained during the last quarter of a century would probably have never been reached without the assistance of urari. Indirectly, therefore, we owe these to Bernard and Kölliker.

We are also indebted to Bernard for what was his earliest work, an important research on the functions of the pancreas, more especially on the use of the pancreatic juice in the digestion of fat. This alone was an important addition to physiological science; but it retires into the background before the more important labours on which I have dwelt.

Lastly, in addition to these special researches, physiology has been enriched by a series of general lectures on the nervous system, on digestion, on poisons, on the properties of blood and other animal fluids, in which Bernard not only brought forward many other observations of interest and importance, as for instance those on carbonic-oxide poisoning, but also directed his readers in a lucid and striking manner to general considerations of great value.

Some of the views which he has thus put forward have not stood the test of subsequent investigation; but many of them, for instance the conception of the blood as an internal medium on which the several tissues live, have become part and parcel of the higher physiological teaching of the day; and by the exposition of his general views, Bernard has done service to physiology quite commensurate with the fruit of his more special inquiries.

[The Medal was received for M. Bernard by His Excellency the French Ambassador.]

The Rumford Medal has been awarded to M. Pierre Jules César Janssen, For. Mem. R.S., for his numerous and important researches on the radiation and absorption of light, carried on chiefly by means of the spectroscope.

For the last 16 years Janssen's labours have been unceasing; and he is continuing them with unabated vigour at the present moment.

His first communication dates from 1860, in which year he recorded some observations on the absorption of radiant heat by the interior of the eye. This thesis gained for him the Doctorat ès sciences physiques. In 1862 he published the first section of his celebrated researches on the origin of the telluric lines of the solar spectrum: he gave us the new form of spectroscope of which we are only now beginning to take full

advantage (I allude to the "Direct-vision Spectroscope," long associated with the name of Hofmann, because that optician was employed by Janssen), and pointed out how spectrum-analysis might enable us to settle the vexed question of the existence of a lunar atmosphere.

In 1866, with reference to the telluric line, he experimented at La Villette on a tube, some 37 metres long, containing steam at the pressure of 7 atmospheres, with the result that by comparing the spectra he was enabled to demonstrate that the telluric lines were really due to the absorption of aqueous vapour. The experiment was repeated in another form by observations of the spectrum of a flame several miles away through the vapour overlying the Lake of Geneva.

After these researches he sought and obtained a mission from the Paris Academy of Sciences to South-eastern Europe to make observations on the spectra of stars; and he was enabled to establish the fact that aqueous vapour exists in the atmosphere of some of them.

These researches on aqueous vapour led him to observe many spectra, among which were iodine, bromine, and others; and in the 'Comptes Rendus' and Proceedings of the Société Philomathique, observations are recorded which show that at that time he and Ångström were in the van of such researches.

Janssen's observations regarding aqueous vapour naturally led him to take every occasion of studying the solar atmosphere; and since the annular eclipse of 1867 (which he observed at Trani) there has been one total solar eclipse only which he has not studied.

After a scientific mission to the Azores in 1867, he went to India in 1868 to observe the great eclipse of that year. Not only were his observations of the eclipse itself of the highest value, but during the eclipse, with a flash of genius, the thought occurred to him that an eclipse was not necessary to the observation of the social phenomena into which everybody was inquiring; and he was the first to apply the method, now well known, which is being utilized in all civilized countries for the advancement of knowledge.

For some months after the eclipse Janssen remained in India, and brought home a rich series of observations, opening up many branches of inquiry which have since proved most fruitful in result.

In the eclipses of 1870 in Africa, 1871 in India, and 1875 in Siam, Janssen was present, and advanced further the question which he had set himself, and with the solution of which his name will always be associated.

Janssen's skill as an observer and his sound knowledge of optical and mechanical questions, have not been shown merely in connexion with the spectroscope; he was anxious to observe not only the recent transit of Venus, but to obtain records of several physical phenomena which can be observed only at such times. For this purpose he gave attention to astronomical photography; and the result was the introduction of his

revolving apparatus, which was instantly adopted by our own eclipse parties, and will probably be the only photographic instrument used in future transits.

Janssen is at the present time engaged in organizing a physical observatory, and is taking daily photographs of the sun, preliminary to obtaining daily spectrum-photographs to elucidate all those inquiries which have been raised by his former work.

I have limited this statement to those researches of M. Janssen which have reference to the Rumford Medal. In the sixty notices of his papers printed in the 'Catalogue of Scientific Papers,' some will be found on other branches of knowledge, the results of his many scientific missions, of which a list is appended:—

1857–58. Determination of the Magnetic Equator on the Coast of Peru.
1861–62. Study of the Telluric Lines in Italy.
1864. Continuation of this inquiry from high points in the Alps.
1867. Observations of the Annular Eclipse at Trani (Italy). Observations of the Eruption of Santorin. Magnetic Observations in the Azores.
1868. Observations of total Eclipse in India. Discovery of the new method. Optical and Magnetical observations at Simla.
1870. Observations of total Eclipse in Africa. Janssen escaped from Paris in a balloon to make these.
1871. Observations of the total Eclipse in Asia.
1874. Observations of the Transit of Venus in Japan.
1875. Observations of a total Eclipse of the Sun in Siam.

[The Medal was received by M. Janssen.]

A Royal Medal has been awarded to Mr. William Froude, F.R.S., for his researches, both theoretical and experimental, on the Behaviour of Ships, their oscillations, their resistance, and their propulsion.

It is generally admitted that Mr. Froude has done more than anybody else towards the establishment of a reasonable theory of the oscillation of ships in wave-water, as well as for its experimental verification. The very accurate instruments which he has contrived for the measurement of a ship's oscillation at sea have even permitted him to measure (as a differential phenomenon) the mean wave acting upon the ship with a degree of exactness exceeding that with which it has hitherto been possible to ascertain the profile of the surface-wave of the sea.

He was also the first to establish on thoroughly sound principles the mechanical possibility of that form of motion known as the trochoidal sea-wave, which more nearly than any other appears to represent the shape of smooth ocean-wave, and which now forms the groundwork of all useful theories of the oscillation of ships.

He has also conducted a series of experiments, extending now over

many years, on the Resistance, Propulsion, and Form of Ships, and on the very important and little-understood question of the law connecting the behaviour of ships, in all these respects, with that of models of ships on a much smaller scale. These experiments have been conducted partly for the government, and with public money; but they have also very largely taxed Mr. Froude's own private resources, the sums repaid to him by no means representing his whole expenditure on these matters, and including no compensation whatever for his own time or labour.

The amount of mechanical skill, as well as of theoretical acuteness, which has been exhibited in all this work has placed Mr. Froude in the foremost rank of all investigators on this subject. No one, indeed, has ever done more, either theoretically or practically, for the accurate determination of a ship's motion, whether in propulsion or in waves, than Mr. Froude. Without undervaluing other modern writers, it is not too much to say that his investigations at present take completely the lead in this very important question—most important to a maritime nation.

Mr. Froude's papers are mainly to be found in the 'Transactions' of the Institution of Naval Architects and of the British Association, as also in separate official reports published as "Blue Books."

[The Medal was received by Mr. Froude.]

A Royal Medal has been awarded to Sir Charles Wyville Thomson, for his successful direction of the scientific investigations carried on by H.M.S. 'Challenger.'

In consequence of representations made to Her Majesty's Government by the President and Council of the Royal Society, the Lords of the Admiralty, in 1872, fitted out and commissioned the ship 'Challenger' for the purpose of undertaking a survey of the ocean of a more systematic and complete character than any which had hitherto been attempted.

After crossing the Atlantic in various directions, the distinguished officer, Captain Nares, who was intrusted with the command of the 'Challenger,' was instructed to proceed southward to the Antarctic regions, and thence to take his way along the western side of the Pacific to Japan; from Japan he was to cross the Pacific, and, running southward through its eastern region, to return to England by way of Cape Horn.

The track taken by Captain Nares, and his successor in command, Captain Thomson, covered 69,000 thousand miles; and the chief objects of the expedition were to obtain at stations of accurately ascertained position, observations by which the temperature of the sea, and its physical and chemical condition, from the surface to the bottom and at all intermediate depths, could be determined, to drag up the sea-bottom itself in quantities sufficient for its satisfactory examination, to ascertain the nature of the fauna at the surface and at the bottom, and to collect

and preserve the animals thus obtained, in such a manner as to enable their nature and affinities to be determined, under more favourable conditions than those afforded by life on shipboard, on the return of the vessel.

In this way it was hoped by those who proposed the Expedition to the Government, that a firmer foundation by far than any which formerly existed would be laid for the physical geography of the ocean.

The Fellows of the Royal Society hardly need to be reminded of the manner in which those duties have been performed. From time to time, in the space of the three years and a half during which the 'Challenger' has cruised in every variety of climate, and circumnavigated the globe, many long and interesting Reports, sent home by the Director and the other officers of the Staff, have been laid before the Society by order of the Lords of the Admiralty, have been printed in the 'Proceedings,' and afford solid evidence of the nature and value of the work that has been done.

We have records of serial temperatures and determinations of the sea-bottom obtained at 354 stations, of the extraordinary fact of the occurrence of peroxide of manganese in masses over thousands of square miles, of the final answer to the vexed question as to the habitation of the *Globigerinæ* (which contribute so largely to the existing processes of rock-formation), of the general uniformity of the deep-sea fauna all over the world, together with many other new and interesting discoveries which need not be enumerated. The collections which have been formed are of unexampled value for their extent and the excellency of their preservation.

It may be truly said that no Expedition for scientific purposes ever left the shores of any country better organized or more abundantly provided with all that would be required for its efficiency; and it is no less true that none has ever more completely fulfilled the purpose for which it was organized.

Under these circumstances the President and Council of the Royal Society have judged that the award of a Royal Medal to Sir Wyville Thomson is a well-earned recognition of the great success which he and the Scientific Staff of the 'Challenger,' under his direction, have rendered to Science, and, at the same time, a fitting acknowledgment, on their part, of the successful manner in which he has discharged the duty with which, on their recommendation, the Government intrusted him.

[The Medal was received by Sir Wyville Thomson.]

The Statutes relating to the election of Council and Officers were then read, and Mr. Wollaston Blake and Mr. C. V. Walker having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were collected, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—Joseph Dalton Hooker, C.B., M.D., D.C.L., LL.D.

Treasurer.—William Spottiswoode, M.A., LL.D.

Secretaries.— Prof. George Gabriel Stokes, M.A., D.C.L., LL.D.
Prof. Thomas Henry Huxley, LL.D., Ph.D.

Foreign Secretary.—Prof. Alexander William Williamson, Ph.D.

Other Members of the Council.—Major-General John T. Boileau; Warren De La Rue, D.C.L.; Professor P. Martin Duncan, M.B., P.G.S.; Professor William H. Flower, F.R.C.S.; Professor Michael Foster, M.D.; Edward Frankland, D.C.L.; Francis Galton, M.A.; William Augustus Guy, M.B.; John Russell Hind, F.R.A.S.; The Rev. Robert Main, M.A.; William Pole, C.E., Mus. Doc.; The Rev. Bartholomew Price, M.A.; Rear-Admiral G. H. Richards, C.B.; Henry Clifton Sorby, Pres. Mic. Soc.; Professor Henry J. Stephen Smith, M.A.; Professor Balfour Stewart, M.A.

The thanks of the Society were given to the Scrutators.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Compounders.	£4 yearly.	Total.
November 30, 1875.	4	48	257	258	567
Elected			+ 3	+ 15	+ 18
Deceased		− 2	− 9	− 13	− 24
Since compounded ..			+ 2	− 2	
November 30, 1876.	4	46	253	258	561

Statement of Receipts and Expenditure during the Year ending November 30, 1876.

Receipts	£	s.	d.
Annual Contributions	1056	0	0
Admission Fees	180	0	0
Compositions	212	0	0
Rents	322	19	3
Dividends (exclusive of Trust Funds)	1523	16	7
Proceeds of Jodrell Fund	131	9	1
Sale of Transactions, Proceedings, &c.	635	14	9
Petty Receipts and Repayments	32	4	3
	£4094	3	11
Sale of Land, Acton Estate	350	0	0
	£4444	3	11
Balances from 1875	245	0	0

Expenditure	£	s.	d.	£	s.	d.
Salaries and Wages				1205	16	0
Prof. Rupert Jones £75, Prof. W. G. Adams £75 } for Arctic Manual				150	0	0
Dr. I. B. Balfour £100, Mr. G. Gulliver £100 } Rodriguez Expedition				200	0	0
R. Meldola—Eclipse Expedition				50	0	0
The Scientific Catalogue				235	5	0
Books for the Library				75	12	2
Binding ditto				117	5	4
Printing Transactions, Part I. 1875. Separate Copies to Authors and Publisher	17	2	0			
Ditto Proceedings, Nos. 163–172	520	0	9			
Ditto Miscellaneous	84	9	4	1891	2	11
Paper for Transactions and Proceedings	371	0	3			
Binding and Stitching ditto	135	5	10			
Engraving and Lithography	763	4	9			
Soirée and Reception Expenses				104	5	5
Miscellaneous Expenses	46	7	4			
Coal, Lighting, &c.	101	11	10			
House Expenses	144	0	2			
Shand, Mason, and Co., Fire-hose and Fittings	85	4	0			
Tea Expenses	20	18	2			
Fire Insurance	41	15	0	549	2	6
Taxes	17	4	4			
Advertising	15	11	6			
Few and Co., Law Charges	44	0	6			
Postage, Parcels, and Petty Charges	30	7	8			
Mablethorpe Schools, Donation	2	2	0			
				£4578	9	4

Trust Funds.

Trust Funds.

	£	s.	d.	£	s.	d.
Donation Fund Dividends	180	6	7			
" Wheatstone Bequest	500	0	0			
Rumford Fund	68	19	3			
Wintringham Fund	35	12	6	13206	10	0
Copley Medal Fund	9	19	7			
Davy Medal Fund	32	13	1			
Handley Bequest	6378	19	0			
Jodrell Fund	6000	0	0			
				£17895	13	11

	£	s.	d.	£	s.	d.
Donation Fund	460	0	0			
" Bought £517 9s. 3d. Consols	500	0	0			
Wintringham Fund	35	14	0			
Copley Medal Fund	4	14	7			
Dr. Andrews, Bakerian Lecture	4	0	0			
Mr. Romanes, Croonian Lecture	2	19	3	13166	5	8
Davy Medal Fund	21	0	0			
Handley Bequest—Legacy Duty	637	17	10			
" Bought £5898 2s. 5d. Reduced	5500	0	0			
Jodrell Fund—Bought 6221 14s. 1d. New Threes	6000	0	0			
Balance at Bank				126	15	0
Balances on hand, Catalogue and Petty Cash				24	3	11
				£17895	13	11

W. SPOTTISWOODE,
Treasurer.

Dircks Bequest.

	£	s.	d.
Consols	169	0	10
New Threes	534	12	0
India Fours	175	0	0
	£878	12	10

	£	s.	d.
Consols transferred to Society's account	169	0	10
New Threes: sold for £505 4s.; bought £492 5s. 6d. India Fours	534	12	0
India Fours transferred to Society's account	175	0	0
	£878	12	10

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £136 per annum.
Estate at Acton, Middlesex (34 A. 2 R. $4\frac{3}{4}$ P.), £167 17s. 10d. per annum.
Fee Farm near Lewes, Sussex, rent £19 4s. per annum.
One fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
Stevenson Bequest. Chancery Dividend. One fourth annual interest on £85,336, Government Annuities and Bank Stock (produced £474 7s. 5d. in 1875–76).
£19,898 2s. 5d. Reduced 3 per Cent. Annuities.
£30,847 10s. 11d. Consolidated Bank Annuities.
£403 9s. 8d. New $2\frac{1}{2}$ per Cent. Stock—Bakerian and Copley Medal Fund.
£6221 14s. 1d. New Threes.—Jodrell Fund.
£667 5s. 6d. India Fours.
£660 Madras Guaranteed 5 per Cent. Railway Stock—Davy Medal Fund.
£10,000 Italian Irrigation Bonds—The Gassiot Trust.

Trust Funds. 1876.

Scientific Relief Fund.

	£	s.	d.
Investments up to July 1872, New 3 per Cent. Annuities	6328	11	2
" " Metropolitan 3½ Consols	100	0	0
	£6428	11	2

Dr.	£	s.	d.	*Cr.*	£	s.	d.
Balance	246	4	6	By Grants	170	0	0
Dividends	191	7	7	Balance	267	12	1
	£437	12	1		£437	12	1

Donation Fund.

£6333 10*s.* 4*d.* Consols.

	£	s.	d.		£	s.	d.
To Balance	598	7	0	By Grants	460	0	0
Sir C. Wheatstone Bequest	500	0	0	Bought £517 9*s.* 3*d.* Consols	500	0	0
Dividends	180	6	7	Balance	318	13	7
	£1278	13	7		£1278	13	7

Rumford Fund.

£2322 10*s.* Consols.

	£	s.	d.		£	s.	d.
Two years' Dividends, 1876	138	1	5				

Bakerian and Copley Medal Fund.

£403 9*s.* 8*d.* New 2½ per Cent.

	£	s.	d.		£	s.	d.
To Balance	70	11	7	By Gold Medal	4	14	7
Dividends	9	19	7	Bakerian Lecture	4	0	0
				Balance	71	16	7
	£80	11	2		£80	11	2

Wintringham Fund.

£1200 Consols.

	£	s.	d.		£	s.	d.
To Balance, 1875	35	14	0	By Payment to Foundling Hospital, 1876	35	14	0
Dividends, 1876	35	12	6				
	£						

Croonian Lecture Fund.

	£	s.	d.		£	s.	d.
To one fifth of Rent of Estate at Lambeth Hill, payable by the College of Physicians	2	10	3	By Croonian Lecture	2	10	3

Davy Medal Fund.

£660 Madras Guaranteed 5 per Cent. Railway Stock.

	£	s.	d.		£	s.	a
To Balance	201	18	8	By Payment for Medallion	21	0	0
Dividends	32	13	1	Balance	213	11	9
	£234	11	9		£234	11	9

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.

	£	s.	d.		£	s.	d.
To Balance	178	10	0	By Payments to Kew Committee	498	18	4
Dividends and drawn Bonds	733	8	4	Bonds bought	177	0	0
				Balance	236	0	0
	£911	18	4		£911	18	4

The Jodrell Fund.

£6221 14*s.* 1*d.* New 3 per Cent. Stock.

	£	s.	d.		£	s.	d.
To Interest and Dividend	131	9	1	By Payment to Royal Society	131	9	1

The Handley Fund.

£5898 2*s.* 5*d.* Reduced 3 per Cent. Stock.

Interest and Dividend .... £90 3*s.* 1*d.*

Account of the appropriation of the sum of £1000 annually voted by Parliament to the Royal Society (the Government Grant), to be employed in aiding the advancement of Science (continued from Vol. XXIV. p. 101).

1876.

1. Mr. J. A. Broun, for investigating the effects due to the Sun's Rotation on the Earth's Magnetism, and the Atmospheric Variations	£100
2. Dr. Stenhouse, for continuing his Researches on Products obtained from the Lichens, and for a Research on Pyrogallol and its Derivatives	50
3. Mr. J. A. Fleming, for a Research on the production of Induced Currents in Liquid and Gaseous Conductors when under the influence of a Magnetic Field	25
4. Mr. G. J. Romanes, for Apparatus to enable him to continue and extend his Researches into the Muscular and Nervous Systems of the Medusæ	50
5. Mr. E. Neison, for an Investigation of the structural constitution of the Octylalcohol Methylhexylcarbinol, and a detailed examination of its derivatives, and of the derivatives of Sebacic Acid	25
6. Mr. J. N. Lockyer, for continuation of Spectroscopic Researches	100
7. Mr. W. Crookes, for continuation of Investigation on Repulsion accompanying Radiation, and for Apparatus	50
8. Prof. W. G. Adams, for aid in further Investigation of the Action of Light on Selenium	25
9. Mr. C. R. A. Wright, for a Research on the Determination of the Absolute Value of Chemical Affinity in terms of Electrical Magnitudes	50
10. W. Murrell, for a Research on the Physiological Action of certain Alkaloids, particularly Jaborandine and Gelsemanine	50
11. Dr. Carpenter, for purchase of Dr. Semper's collection of tropical Antedons from the Philippine Seas.—£200; or, as an alternative, for finishing his own series of drawings of *Antedon*	100
12. Sir W. Thomson, for continuation of Harmonic Reduction of Tidal Observations	50
13. Sir W. Thomson and Prof. J. Thomson, for the construction of an Analyzing Machine suitable for performing the Harmonic Analysis of Meteorological Observations and of Tidal Observations	100
	£775

Dr.	£	s.	d.		*Cr.* £	s.	d.
To balance on hand, Nov. 30, 1875....	797	7	4	By Appropriations as above	775	0	0
To Grant from Treasury (1876)......	1000	0	0	Printing and Postal charges	6	6	6
				Balance on hand, Nov. 30, 1876	1016	0	10
	£1797	7	4		£1797	7	4

Account of Grants from the Donation Fund in 1875–76.

	£	s.	d.
For account of the Eclipse Expedition	£300	0	0
Rev. Dr. Robinson, for a series of Experiments to complete the Theory of the Cup Anemometer	160	0	0
	£460	0	0

Report of the Kew Committee for the Year ending October 31, 1876.

Magnetic Work.—The Magnetographs have been in constant operation throughout the year. The horizontal-force instrument has undergone readjustment of its suspension on two occasions, in order to increase its sensibility, which appears to have somewhat diminished of late.

In accordance with the usual practice, determinations of the scale-values of all the instruments were made in the first week of the new year, and of the Bifilar when required.

The monthly observations with the absolute instruments have been continued, as usual, by Mr. Whipple and Mr. Figg, and the results are given in the Tables appended to this Report.

The paper referred to in the last Report, containing the results of the Absolute Observations for the six years ending March 1875, having been read before the Royal Society, has been printed in vol. xxiv. of the 'Proceedings.'

The two Sergeants of the Royal Artillery, formerly in Sir E. Sabine's office at Woolwich, have been in regular attendance at Kew throughout the year, principally engaged in the preparation of his paper "Contributions to Terrestrial Magnetism, No. XV.," which will be printed in the Philosophical Transactions.

The tabulation of the magnetic curves, alluded to in the last Report as being in arrear, for the years 1871–73 was almost completed when it was stopped in May by the resignation of the extra assistant appointed for that work. The vacancy has not yet been filled, and the work, together with the measurement of the recent curves, is at present suspended.

Magnetic data have been supplied to Prof. Barrett, Prof. Core, Messrs. Elliott Brothers, Mr. Gordon, Dr. Guthrie, the Hydrographic Office, Prof. Balfour Stewart, Dr. Marshall Watts, and Mr. H. Watts.

The Unifilar, Declinometer, and Azimuth Compass used by Captains Anderson and Featherstonhaugh, of the Royal Engineers, during their operations in the North-American Boundary Commission, have been returned to Store in the Observatory, and the observations made with them having been examined, will be embodied in a paper shortly to be presented to the Royal Society.

Captain Maclear, R.N., has visited the Observatory, and, assisted by the Staff, redetermined the constants of the magnetic instruments used by him during the 'Challenger' Expedition, thus completing the observations by making Kew the base station of the voyage.

A communication having been made to the Committee by Mr. R. J. Ellery, Director of the Melbourne Observatory, requesting an expression of opinion from them as to the desirability of continuing the photographic registration of the magnetometers at his Observatory, and asking also for suggestions for dealing with the results obtained, a circular was drawn up, and replies to it were received from the following gentlemen :—

Prof. J. C. Adams.
Prof. W. G. Adams.
Prof. Buys Ballot.
J. A. Broun, Esq.
Captain F. J. O. Evans.
Prof. C. Hornstein.
Dr. C. Jelinek.
Prof. J. v. Lamont.
Rev. H. Lloyd, D.D.
Rev. R. Main.
C. Meldrum, Esq.
Major-General W. J. Smythe.
Prof. Balfour Stewart.
Prof. G. G. Stokes.
Prof. H. Wild.

Most of whom strongly advocate the continuance of self-recording magnetometers in Victoria.

Meteorological Work.—The several self-recording instruments for the continuous registration respectively of pressure, temperature, humidity, wind (direction and velocity), and rain have been maintained in regular

operation under the care of Mr. T. W. Baker, assisted by T. Gunter. The daily standard eye-observations for the control of the automatic records have been made regularly, as well as daily observations in connexion with the Washington synchronous system.

In addition to the regular work of Kew as one of the self-recording Observatories in connexion with the Meteorological Office, the duty of examining and checking the work of the six other self-recording Observatories of the same character has been carried on, in accordance with the method described in the Report of the British Association for 1869. This portion of the work has been performed of late by Messrs. Hawkesworth, Aldridge, and Harrison.

The arrears of work, caused by the removal of Mr. Cullum to the charge of the Valencia Observatory, have been cleared off, and the work is now up to date.

The Observatories at Aberdeen, Armagh, Falmouth, Glasgow, Stonyhurst, and Valencia have been visited by Mr. Whipple, and their instruments inspected.

Electrometer.—This instrument having experienced an accidental derangement in June has since failed in its action, and all attempts at setting it to work satisfactorily have been hitherto unsuccessful. The maker, Mr. White, of Glasgow, has promised to visit the Observatory at an early date and examine it to find the cause of failure.

A determination of the scale-value of the Electrometer by means of a 100-cell Bunsen battery was made in January.

Photoheliograph.—The Photoheliograph having been replaced in the Royal Observatory, Greenwich, by one of the instruments constructed for use in the Transit-of-Venus expeditions, was returned to the Observatory January 5th, and re-erected in the Dome, but was again dismounted in March, and sent, together with a number of solar negatives, to the Loan Exhibition, South Kensington, where it now remains.

The re-examination of the measurements of the Kew sun-pictures, as noticed in former Reports, has been steadily carried on throughout the year by Mr. Whipple, assisted by Mr. M'Laughlin, who has been temporarily engaged for this purpose; and the Ephemerides for the whole period of the Kew Sun-Spot Observations have been recalculated by Mr. A. Marth, and are now in the Observatory. All of these operations have been conducted at the expense of Mr. De La Rue.

A new Micrometer for use in India, with a Photoheliograph, has been made under the supervision of Mr. De La Rue, in which various modifications, suggested by experience obtained in the use of the instrument at Kew, have been introduced.

At the request of Mr. Hind, F.R.S., a careful inspection has been made of the Kew sun-pictures from 1858 to 1875, with a view to obtain evidence as to the existence of the intra-Mercurial planet. The observations bearing on the question have been communicated to that gentleman.

The eye-observations of the sun, after the method of Hofrath Schwabe, have been made daily by Mr. Foster, when possible, as described in the Report for 1872, in order, for the present, to maintain the continuity of the Kew record of sun-spots.

A catalogue of the Schwabe MSS., deposited in the Observatory, has been made for the Royal Astronomical Society.

Extra Observations.—The observations with Prof. H. E. Roscoe's Photometer were discontinued in November last, the year for which the experiment was undertaken having expired. The instrument has since been returned to the Owens College.

The Solar-radiation Thermometers are still observed daily.

The Campbell Sundial, described in the last Report, continues in action, and the improved form of the instrument, giving a separate record for every day of the duration of sunshine, has been regularly worked since March.

At the request of the Editor of the 'Times,' a copy of the traces of the self-recording instruments on a reduced scale, together with an epitome of the general features of the weather, is now prepared. This is published every week in that journal, the cost to the Observatory being defrayed by the proprietors.

Verifications.—A fair increase has occurred in this branch of the work of the Observatory. The following magnetic instruments were verified :—

A Unifilar for Lieut. Wille, Norwegian Navy.
,, ,, The Royal Naval College, Greenwich.
,, ,, Elliott Brothers, London.
A Dip-circle ,, Lieut. Wille, Norwegian Navy.
,, ,, Captain Jelagin, St. Petersburg.
A Fox-circle ,, Lieut. Wille, Norwegian Navy.
A pair of Dipping-needles for Dr. Rijckevorsel, Batavia.
,, ,, ,, Mr. Meldrum, Mauritius.
,, ,, ,, Senhor Capello, Lisbon.
A Dipping-needle ,, Mr. Chambers, Colaba.
A set of three Magnets for Zi-ka-Wei Observatory.

A Dip-circle of a high degree of accuracy has been obtained, and after verification forwarded to Dr. Da Souza, Coimbra; and a similar instrument, having an accessory telescope fitted to enable it to be used as an altazimuth, has been purchased and verified for the Zi-ka-Wei Observatory.

The Magnetographs ordered by Dr. C. H. Vogel for the Potsdam Astrophysical Observatory have been constructed, but before verification were lent by Dr. Vogel to the South-Kensington Loan Exhibition, where they are now being exhibited, in a building erected specially by the Commissioners for the purpose.

A set of Magnetographs, constructed in 1860 for the Batavia Ob-

servatory, have been returned to England for repair and alterations, and are now undergoing verification.

The following meteorological instruments have been verified, this portion of the work being entrusted to Mr. T. W. Baker, assisted by Messrs. Foster, Constable, and Welch :—

Barometers,	Standards	96
,,	Marine and Station	106
		202
Aneroids		28
Thermometers,	ordinary Meteorological	1410
,,	Boiling-point Standards	36
,,	Mountain	34
,,	Clinical	1560
,,	Solar radiation	90
		3130

In addition, 221 Thermometers have been tested at the melting-point of mercury.

10 Standard Thermometers have been calibrated and divided at Kew.

The following is the list of miscellaneous instruments which have been verified :—

Hydrometers	129
Rain-gauges	29
Dial Anemometers (Robinson's)	20

In addition to the Admiralty, Meteorological Committee, and opticians, a number of instruments of various kinds have been verified for the Standards Department and the Inland Revenue Office.

The total increase in the number of instruments verified over last year has been 385, and in fees paid £36 13*s*. 1*d*.

There are now at the Observatory undergoing verification 290 Thermometers, 110 Hydrometers, and 20 Barometers.

London Office for receipt of instruments for verification.—Arrangements have been made with Mr. Strachan, of the Meteorological Office, who now receives instruments for verification at Kew, at 116 Victoria Street, Westminster, and takes charge of them on their return.

A Thermograph and Barograph, purchased by Dr. van der Stok for the Batavia Observatory, are now undergoing verification.

A Tabulating instrument of the most efficient pattern has been purchased and verified for the Zi-ka-Wei Observatory.

Mr. Galton's apparatus for testing Thermometers has received several additions, serving to improve its utility ; and a series of experiments have been made with it, the results of which will be laid before the Royal Society.

A new Cathetometer of great accuracy has been constructed and erected against the Mural Quadrant wall.

Two portable Barometers have been cleaned and repaired, in order that they may be used in making a comparison between the Kew and Greenwich Standard Barometers at an early date.

One Sextant has been verified.

Meteorological data have been supplied to Prof. Balfour Stewart, Mr. J. G. Symons, Mr. Lloyd, the Editors of the 'Illustrated London News,' and the 'Times.'

Chronometer Testing.—One Chronometer has been rated for an optician, but no further steps have been taken towards making this a regular branch of the Observatory work.

Pendulum Experiments.—Mr. C. S. Peirce, of the United States Coast Survey, who has recently been making pendulum observations at Berlin, Geneva, and Paris, arrived at the Observatory in June; after having had the necessary fittings put up in the pendulum-room, he erected his apparatus, and made a complete series of vibrations. He has since returned to America.

Instruction given.—Dr. E. van der Stok, Vice-Director of the Batavia Observatory, has received instruction in the use of the self-registering and absolute instruments, both magnetical and meteorological.

Dr. Hamberg, of the Upsala Observatory, received some instruction in the use of Meteorological instruments. Two assistants in the Standards Department received instruction in the manipulation of Thermometers.

Waxed Paper supplied.—Waxed paper has been supplied to the following Observatories:—

Coimbra,	Radcliffe,
Colaba,	Stonyhurst,
Lisbon,	and to
Mauritius,	The Meteorological Office.

Loan Exhibition.—The Committee having been requested by the Science and Art Department to exhibit objects of interest in their possession at the Loan Collection of Scientific Apparatus, all the instruments either of superseded patterns or duplicates which could be spared without suspending the work of the Observatory were put in order at the expense of the Department, and placed in the galleries at South Kensington. Thirty-one articles (enumerated in the following list) are exhibited.

The Kew Photoheliograph.
Stand with 5 Photographs of the Sun, taken with the Kew Heliograph, and 1 Photograph of a Scale.
Photographic self-registering Declination Instrument.
Photographic self-registering Horizontal-Force Instrument.
Ronalds's Photo-Barograph.
Balance Anemometer.
Ronalds's Electrical Apparatus and Collector.
Kreil's Barograph.

Electrical Machine used by Ronalds.
Ronalds's Rain-and-Vapour Gauge.
Eight-haired Saussure's Hydrometer.
Thomson's divided-ring Electrometer and Gauge.
St.-Helena Magnetometers, comprising the instruments for
Declination,
Bifilar,
Vertical Force.
Declination-Compass used by Sir J. Richardson.
Vibration-Apparatus used by Captain Barnett.
Dip-Circle used by Sir J. C. Ross.
Apparatus for swinging Pendulums.
Invariable Pendulum in Vacuum Chamber.
Air-Pump, Stand for Vacuum Chamber, and Telescope with stand.
Gassiot's Rigid Spectroscope and Lamp.
Quadrant by Butterfield, of Paris.
Kew Pattern Dip-Circle.
Portable Unifilar Magnetometer.
Hodgkinson's Actinometer.
Model of Mr. Galton's Sextant-testing Apparatus.
Model of Mr. Cooke's " " "
Two engravings of Kew pattern Magnetographs, in frame.

The Société Française de Photographie having made application to the Committee for assistance in their exhibition of objects illustrating the adaptation of photography to scientific purposes, a set of curves, magnetical and meteorological, together with a few prints from the solar negatives, were forwarded to Paris, where they were exhibited in the Palais de l'Industrie.

Workshop.—The several pieces of Mechanical Apparatus, such as the Whitworth Lathe and the Planing Machine, procured by Grants from either the Government-Grant Fund or the Donation-Fund, for the use of the Kew Observatory, have been kept in thorough order; and many of them are in constant, and others in occasional use at the Observatory.

Library.—During the year the Library has received as presents the publications of

11 English Scientific Societies and Institutions,
27 Foreign and Colonial Scientific Societies and Institutions,

and numerous pamphlets from various individuals. A few standard works of reference have been purchased, and a number of periodicals bound.

Observatory and Grounds.—H.M. Commissioners of Woods and Forests have painted the exterior of the building and put the roof into thorough repair.

The gravelled footway, mentioned in the last Report, has also been made across the Old Deer Park to the Observatory.

During the high tides of last winter the Thames overflowed its banks in the neighbourhood of the Observatory, and the basement was flooded, but no damage was done to any of the instruments.

A new well has been sunk to the north of the building, the old one being contaminated by drainage during the floods.

Staff.—The Staff employed at Kew are as follows:—Mr. G. M. Whipple, B.Sc., Superintendent; T. W. Baker, First Assistant; J. W. Hawkesworth, J. Foster, H. M'Laughlin, F. G. Figg, E. G. Aldridge, R. W. F. Harrison, E. Constable, T. Gunter, and P. Welch. Mr. Samuel Jeffery resigned the appointment of Superintendent at the end of February, and the Committee, at their last meeting, have appointed Mr. G. M. Whipple, formerly First Assistant, to fill his post.

Mr. J. E. Cullum, having been made Superintendent of the Valencia Observatory, resigned his appointment in December. Messrs. A. B. Deane, J. Lawrence, E. Hux, and G. A. Henniker have also resigned during the year.

Committee.—The Committee is constituted as follows:—

Gen. Sir E. Sabine, K.C.B., *Chairman.*

Mr. De La Rue.	The Earl of Rosse.
Capt. Evans.	Mr. R. H. Scott (*Hon. Sec.*).
Mr. F. Galton.	Major-Gen. W. J. Smythe.
Mr. Gassiot.	Lieut.-Gen. Strachey.
Rear-Adm. Richards.	Mr. E. Walker.

Visitors.—The Observatory has been honoured by the presence, among others, of:—

The Members of the Permanent Committee of the Vienna Congress, viz.:—Profs. Buys Ballot, Bruhns, Cantoni, Mohn, and Wild.
British Horological Institute.
Senhor Capello.
Mr. R. J. Ellery.
Mons. J. C. Houzeau.
Rev. H. Howlett.
Dr. Kundt.
Dr. Lemström.
Mr. D. Milne-Home.
Sir Rawson Rawson.
Dr. Recknagel.
Mr. A. Cowper Ranyard.
Dr. Sohncke.
M. Albert Tissandier.
M. Gaston Tissandier.
Prof. von Oettingen.
Baron von Wrangell.

The following is the Balance-sheet of the Observatory for the year; and it will be seen that the finances are in a fairly satisfactory condition:—

Abstract. *Kew Observatory Receipts and Payments Account from November 1, 1875, to October 31, 1876.*

Dr. RECEIPTS.

	£	s.	d.	£	s.	d.
To Balance from 1874–75				585	10	3
Royal Society (Gassiot Trust)	£249	19	8			
" "	248	18	8			
				498	18	4
Meteorological Committee	162	10	0			
" "	162	10	0			
" "	162	10	0			
" "	162	10	0			
				650	0	0
Meteorological Committee, for Postages &c.	20	14	1			
" " Pagoda Observations	5	13	0			
				26	7	1
Payment for Instruments by Commission				435	15	0
Sale of Waxed Paper				66	15	0
Verification Fees, Magnetic Instruments	33	2	0			
" " Met. Com., B. T. Inst.	41	2	6			
" " " Admiralty	32	7	6			
" " " Commissions	68	15	6			
" " Opticians &c.	205	1	1			
Sale of Standard Thermometers	9	0	0			
Payments for Copying Registers	29	17	6			
				419	6	1
Mr. De La Rue for Sun-work				109	13	0
Mr. Peirce for Pendulum Expenses				3	1	9
Sale of Photographic Residues				6	2	3
				£2801	8	9

PAYMENTS. *Cr.*

	£	s.	d.	£	s.	d.
By Salaries and extra work				1101	1	4
Rent of Land	£11	10	0			
Fuel and Gas	59	7	1			
Furniture and Fittings	58	1	7			
Chandlery &c.	19	4	1			
Painting and Repairs	24	16	3			
				172	19	0
Printing and Stationery	49	9	2			
Postages	9	7	7			
Library	11	5	5			
Messenger and Housekeeper	52	0	0			
Porterage and Contingencies	36	5	2			
				158	7	4
Instruments purchased on Commission				511	3	2
Postages and Payments on behalf of Meteorological Committee				13	7	8
Purchase of Waxed Paper, Packing ditto, &c.				86	7	10
Chemicals	33	13	5			
Ice and Carbonic-Acid Gas	15	9	1			
Anemograph Sheets	2	8	9			
Repair of Instruments and Purchase of New ditto	38	18	7			
Carpenters' Work and Sundries	22	0	2			
				112	10	0
Sun-work Expenses				106	4	6
Pendulum Expenses				19	4	7
Expenses on behalf of Loan Scientific Exhibition	18	16	1			
" " Mr. Galton's Experiment	0	5	2			
				19	1	3
London and Westminster Bank	386	0	7			
London and County Bank	93	0	0			
Cash in hand	22	1	6			
Balance				501	2	1
				£2801	8	9

November 15, 1876. Examined, compared with the vouchers, and found correct. (Signed) W. J. SMYTHE, *Auditor.*

ASSETS.

	£	s.	d.
By Balance as per Statement	501	2	1
Verification Fees due	31	17	5
Standard Thermometers sold	4	7	6
" " in stock (valued at)	107	16	0
Waxed Paper sold	4	15	0
" " in stock	41	5	0
Meteorological Committee Sundries	0	16	4
Mr. De La Rue for Sun-work	6	4	6
Mr. Galton	3	8	2
Loan Scientific Exhibition	18	16	1
	£720	8	1

LIABILITIES.

	£	s.	d.
To Gas and Fuel	11	4	5
Chemicals	1	8	0
Instruments and Apparatus	10	9	0
Stationery and Printing	1	2	6
Library	1	2	0
Balance	695	2	2
	£720	8	1

APPENDIX.

Magnetic Observations made at the Kew Observatory, Lat. 51° 28′ 6″ *N., Long.* 0^h 1^m 15^s·1 *W., for the year October* 1875 *to September* 1876.

The observations of Deflection and Vibration given in the annexed Tables were all made with the Collimator Magnet marked K C 1, and the Kew 9-inch Unifilar Magnetometer by Jones, the property of the Magnetic Office, directed by General Sir E. Sabine.

The Declination observations have also been made with the same Magnetometer, Collimator Magnet N E being employed for the purpose.

The Dip observations were made with Dip-circle No. 33, the needles 1 and 2 only being used; these are 3½ inches in length.

The results of the observations of Deflection and Vibration give the values of the Horizontal Force, which, being combined with the Dip observations, furnish the Vertical and Total Forces.

These are expressed in both English and metrical scales—the units in the first being one foot, one second of mean solar time, and one grain; and in the other one millimetre, one second of time, and one milligramme, the factor for reducing the English values to metric values being 0·46108.

By request, the corresponding values in C.G.S. measure are also given.

The value of log π^2K employed in the reduction is 1·64365 at temperature 60°.

The induction-coefficient μ is 0·000194.

The correction of the magnetic power for temperature t_0 to an adopted standard temperature of 35° Fahr. is

$$0{\cdot}0001194\,(t_0-35)+0{\cdot}000{,}000{,}213\,(t_0-35)^2.$$

The true distances between the centres of the deflecting and deflected magnets, when the former is placed at the divisions of the deflection-bar marked 1·0 ft. and 1·3 ft., are 1·000075 ft. and 1·300097 ft. respectively.

The times of vibration given in the Table are each derived from the mean of 12 or 14 observations of the time occupied by the magnet in making 100 vibrations, corrections being applied for the torsion-force of the suspension-thread subsequently.

No corrections have been made for rate of chronometer or arc of vibration, these being always very small.

The value of the constant P, employed in the formula of reduction $\frac{m}{X}=\frac{m'}{X'}\left(1-\frac{P}{r_0^2}\right)$, is $-0{\cdot}00179$.

In each observation of absolute Declination the instrumental readings have been referred to marks made upon the stone obelisk erected about

a quarter of a mile north of the Observatory as a meridian mark, the orientation of which, with respect to the Magnetometer, was determined by the late Mr. Welsh, and has since been carefully verified.

The observers' initials refer—W to Mr. G. M. Whipple, and F to Mr. F. G. Figg.

Observations of Deflection for Absolute Measure of Horizontal Force.

Month.	G. M. T.	Distances of Centres of Magnets.	Temperature.	Observed Deflection.	Log $\frac{m}{X}$. Mean.	Observer.
1875.	d h m	foot.	°	° ′ ″		
October	26 12 31 P.M.	1·0	53·6	15 45 39	9·13461	W.
		1·3	......	7 6 25		"
	2 17 "	1·0	54·0	15 44 38		"
		1·3	......	7 6 2		"
November	23 12 32 P.M.	1·0	46·2	15 44 58	9·13395	W.
		1·3	......	7 6 6		"
	2 27 "	1·0	46·1	15 44 47		"
		1·3	......	7 6 1		"
December	20 12 44 P.M.	1·0	49·4	15 45 13	9·13414	W.
		1·3	......	7 6 18		"
	2 16 "	1·0	49·0	15 44 17		"
1876.		1·3	......	7 5 51		"
January	25 12 10 P.M.	1·0	40·3	15 45 58	9·13380	W.
		1·3	......	7 6 25		"
	2 9 "	1·0	42·4	15 44 38		"
		1·3	......	7 5 59		"
February	23 12 34 P.M.	1·0	52·5	15 44 28	9·13430	W.
		1·3	......	7 5 54		"
	2 5 "	1·0	52·2	15 44 42		"
		1·3	......	7 6 12		"
March	27 1 10 P.M.	1·0	43·6	15 48 20	9·13520	F.
		1·3	......	7 7 35		"
	2 38 "	1·0	45·6	15 47 17		"
		1·3	......	7 7 12		"
April	25 12 57 P.M.	1·0	64·2	15 43 7	9·13431	F.
		1·3	......	7 5 18		"
	2 43 "	1·0	65·8	15 42 21		"
		1·3	......	7 5 4		"
May	26 12 53 P.M.	1·0	56·5	15 42 52	9·13368	F.
		1·3	......	7 5 20		"
	2 29 "	1·0	56·6	15 42 17		"
		1·3	......	7 4 57		"
June	27 12 33 P.M.	1·0	74·6	15 40 12	9·13383	F.
		1·3	......	7 4 0		"
	2 46 "	1·0	79·5	15 39 29		"
		1·3	......	7 3 38		"
July	26 12 42 P.M.	1·0	79·8	15 38 24	9·13335	F.
		1·3	......	7 3 20		"
	2 33 "	1·0	83·1	15 37 32		"
		1·3	......	7 2 49		"
August	28 12 27 P.M.	1·0	67·9	15 40 42	9·13368	F.
		1·3	......	7 4 21		"
	2 27 "	1·0	69·9	15 40 53		"
		1·3	......	7 4 7		"
September	26 12 22 P.M.	1·0	67·9	15 41 0	9·13371	F.
		1·3	......	7 4 33		"
	2 33 "	1·0	69·1	15 40 45		"
		1·3	......	7 4 5		"

Vibration Observations for Absolute Measure of Horizontal Force.

Month.	G. M. T.	Temperature.	Time of one Vibration.	Log mX. Mean.	Value of m.	Observer.
1875.	d h m	°	secs.			
October	26 11 55 A.M.	51·6	4·6179	0·31403	0·53005	W.
	26 2 45 P.M.	53·0	4·6235			,,
November	23 12 2 P.M.	44·6	4·6255	0·31278	0·52888	W.
	23 3 1 P.M.	47·0	4·6252			,,
December	20 12 12 P.M.	47·9	4·6261	0·31302	0·52915	W.
	20 2 57 P.M.	49·2	4·6253			,,
1876.						
January	25 11 33 A.M.	37·0	4·6233	0·31272	0·52876	W.
	25 2 45 P.M.	43·1	4·6254			,,
February	23 11 57 A.M.	50·6	4·6260	0·31290	0·52918	W.
	23 2 34 P.M.	51·3	4·6274			,,
March	27 12 2 P.M.	41·4	4·6206	0·31330	0·52997	F.
	27 3 27 P.M.	46·6	4·6232			,,
April	25 12 6 P.M.	61·8	4·6293	0·31320	0·52936	F.
	25 3 28 P.M.	65·8	4·6285			,,
May	26 12 9 P.M.	56·0	4·6286	0·31212	0·52832	F.
	26 3 11 P.M.	56·7	4·6265			,,
June	27 11 44 A.M.	72·8	4·6330	0·31319	0·52907	F.
	27 3 23 P.M.	82·0	4·6344			,,
July	26 11 52 A.M.	78·3	4·6357	0·31297	0·52864	F.
	26 3 20 P.M.	84·6	4·6356			,,
August	28 11 33 A.M.	66·9	4·6338	0·31238	0·52848	F.
	28 3 11 P.M.	69·2	4·6338			,,
September	26 11 25 A.M.	65·3	4·6323	0·31266	0·52867	F.
	26 3 16 P.M.	67·5	4·6305			,,

Declination Observations.

Month.	G. M. T.	Uncorrected.		Corrected for Torsion.		Observer.
		Observation.	Monthly Mean.	Observation.	Monthly Mean.	
1875.	d h m		West.		West.	
October	27 12 34 P.M.	19° 45′ 16″		19° 45′ 16″		W.
	28 12 31 „	19 38 1		19 34 0		„
	29 12 20 „	19 35 24	19° 39′ 34″	19 34 42	19° 37′ 59″	„
November	24 12 13 „	19 34 41		19 34 41		W.
	25 12 41 „	19 34 18	19 34 30	19 33 53	19 34 17	„
December	21 12 48 „	19 34 57		19 33 33		W.
	23 12 38 „	19 32 44	19 33 51	19 33 51	19 33 42	„
1876. January	26 12 37 „	19 32 48		19 35 29		W.
	27 12 6 „	19 33 18	19 33 3	19 29 43	19 32 36	„
February	24 12 13 „	19 35 22		19 36 24		W.
	26 12 45 „	19 37 46	19 36 34	19 37 5	19 36 45	F.
March	28 12 44 „	19 32 46		19 34 5		F.
	29 12 27 „	19 37 28		19 39 27		„
	31 12 35 „	19 39 40	19 36 38	19 37 21	19 36 58	„
April	26 12 37 „	19 33 43		19 34 6		F.
	27 12 39 „	19 29 10		19 29 33		„
	28 12 40 „	19 32 3	19 31 39	19 32 3	19 31 54	„
May	27 12 37 „	19 31 0		19 29 24		F.
	29 12 32 „	19 31 16	19 31 8	19 33 39	19 31 32	„
June	28 12 22 „	19 36 39		19 34 48		F.
	29 12 33 „	19 32 26	19 34 33	19 32 8	19 33 28	„
July	27 12 37 „	19 33 32		19 32 49		F.
	28 12 25 „	19 29 30	19 31 31	19 29 52	19 31 21	„
August	29 12 21 „	19 27 28		19 29 56		F.
	30 1 11 „	19 35 10	19 31 19	19 33 18	19 31 37	„
September......	27 12 28 „	19 29 29		19 32 1		F.
	28 12 31 „	19 31 30	19 30 30	19 32 55	19 32 28	„

Dip Observations.

Month.	G. M. T.	Needle.	Dip.	Observer.	Month.	G. M. T.	Needle.	Dip.	Observer.
1875.	d h m	No.			1876.	d h m	No.		
Oct.	25 3 13 P.M.	1	67° 48′·40	F.	Apr.	26 3 2 P.M.	1	67° 47′·37	F.
	3 13 ,,	2	47·94	,,		3 0 ,,	2	46·69	,,
	26 3 22 ,,	1	50·87	W.		28 3 37 ,,	1	47·62	,,
	3 24 ,,	2	48·78	,,		3 55 ,,	2	47·00	,,
	Mean......		67 49·00			Mean......		67 47·17	
Nov.	24 3 0 P.M.	1	67 47·84	W.	May	29 2 52 P.M.	1	67 47·06	F.
	2 58 ,,	2	46·60	,,		2 52 ,,	2	46·00	,,
	25 3 8 ,,	1	48·44	F.		30 3 0 ,,	1	46·81	,,
	3 5 ,,	2	47·53	,,		2 59 ,,	2	45·75	,,
	Mean......		67 47·60			Mean......		67 46·41	
Dec.	21 2 41 P.M.	1	67 47·81	W.	June	28 3 1 P.M.	1	67 46·75	F.
	2 42 ,,	2	46·12	,,		3 1 ,,	2	46·06	,,
	22 3 3 ,,	1	48·38	F.		29 2 56 ,,	1	47·06	,,
	3 5 ,,	2	46·56	,,		2 57 ,,	2	46·43	,,
	Mean......		67 47·22			Mean......		67 46·57	
1876.									
Jan.	26 2 57 P.M.	1	67 47·75	W.	July	27 3 29 P.M.	1	67 46·00	F.
	2 58 ,,	2	46·93	,,		3 32 ,,	2	45·62	,,
	27 3 4 ,,	1	47·50	F.		28 3 6 ,,	1	46·68	,,
	3 2 ,,	2	46·88	,,		3 10 ,,	2	45·69	,,
	Mean......		67 47·26			Mean......		67 46·00	
Feb.	24 3 17 P.M.	1	67 47·56	F.	Aug.	29 2 52 P.M.	1	67 45·38	F.
	3 19 ,,	2	47·50	,,		2 53 ,,	2	45·31	,,
	28 3 10 ,,	1	47·81	,,		30 2 53 ,,	1	46·06	,,
	3 13 ,,	2	46·93	,,		2 52 ,,	2	46·06	,,
	Mean......		67 47·45			Mean......		67 45·70	
Mar.	28 3 17 P.M.	1	67 48·12	F.	Sept.	27 3 6 P.M.	1	67 48·25	F.
	3 20 ,,	2	47·19	,,		3 6 ,,	2	47·38	,,
	29 3 1 ,,	1	48·44	,,		28 3 6 ,,	1	46·43	,,
	3 3 ,,	2	47·25	,,		3 7 ,,	2	46·31	,,
						29 3 3 ,,	1	46·56	,,
	Mean......		67 47·75			3 1 ,,	2	45·68	,,
						Mean......		67 46·77	

Magnetic Intensity.

Month.	English Units.			Metric Units.			C.G.S. Measure.		
	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.
1875. October ...	3·8879	9·5348	10·2970	1·7926	4·3963	4·7478	0·1793	0·4396	0·4748
November	3·8852	9·5172	10·2797	1·7914	4·3882	4·7398	0·1791	0·4388	0·4740
December..	3·8854	9·5148	10·2776	1·7915	4·3871	4·7388	0·1791	0·4387	0·4739
1876. January...	3·8856	9·5157	10·2783	1·7916	4·3875	4·7391	0·1792	0·4387	0·4739
February .	3·8842	9·5135	10·2759	1·7909	4·3865	4·7381	0·1791	0·4386	0·4738
March ...	3·8820	9·5104	10·2721	1·7899	4·3851	4·7363	0·1790	0·4385	0·4736
April	3·8854	9·5144	10·2771	1·7915	4·3869	4·7386	0·1791	0·4387	0·4739
May	3·8835	9·5036	10·2664	1·7906	4·3820	4·7337	0·1791	0·4382	0·4734
June	3·8876	9·5148	10·2785	1·7925	4·3871	4·7393	0·1792	0·4387	0·4739
July	3·8887	9·5133	10·2773	1·7930	4·3864	4·7387	0·1793	0·4386	0·4739
August ...	3·8846	9·5008	10·2643	1·7911	4·3807	4·7327	0·1791	0·4381	0·4733
September	3·8857	9·5120	10·2750	1·7916	4·3858	4·7376	0·1792	0·4386	0·4738

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"On the Behaviour of the Fixed Elements of the Connective Tissue of the Tongue in Inflammation." By George F. Dowdeswell, B.A. Cantab. Communicated by J. Burdon Sanderson, F.R.S., Professor of Physiology in University College. Received June 14, 1876*.

[Plate 8.]

The connective tissue of the tongue of Batrachians was first studied, with reference to the changes which it undergoes in inflammation, by Prof. Cohnheim in 1869†. The animal employed by him was the ordinary edible frog (*Rana esculenta*). His mode of preparation was as follows:—A plate of glass about 3″ by 5″ was first prepared, having a smaller oblong plate, measuring 1″ by 0″·7, cemented to it with Canada balsam at one end. This was surrounded by a cork ring border of the same thickness. On this plate the body of the frog (previously curarized) was placed, resting on its back, in such a position that the tongue could be readily extended over the oblong plate with the aid of

* Read June 15, 1876. See *ante*, p. 272.

† Cohnheim, "Ueber das Verhalten der fixen Bindegewebskörperchen bei der Entzündung," Virchow's 'Archiv,' vol. xlv. p. 333.

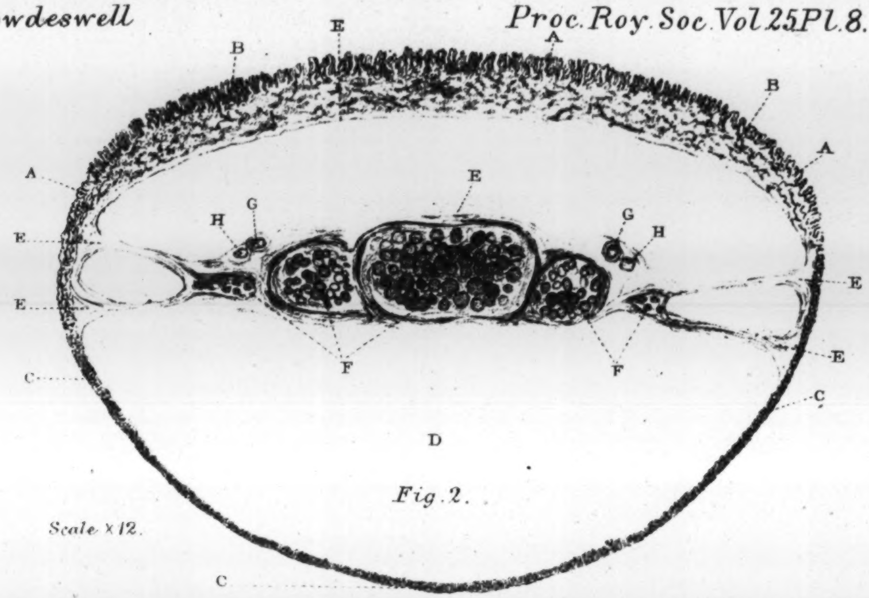
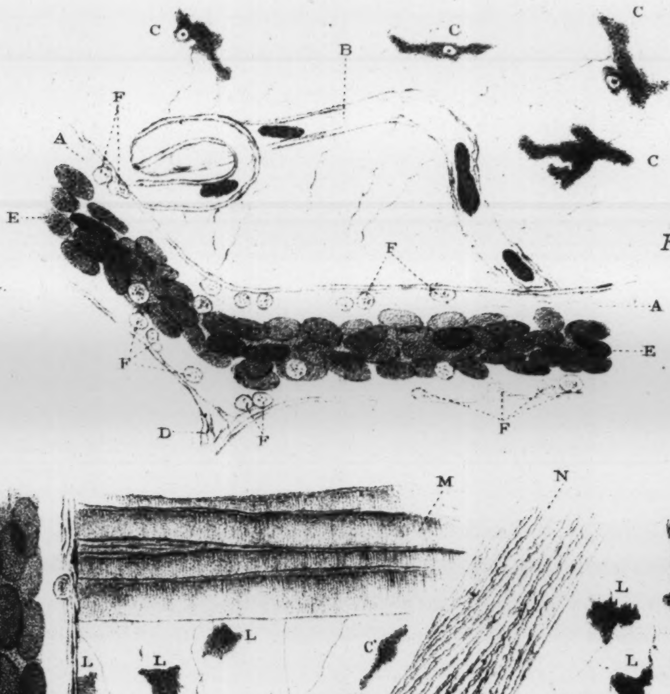


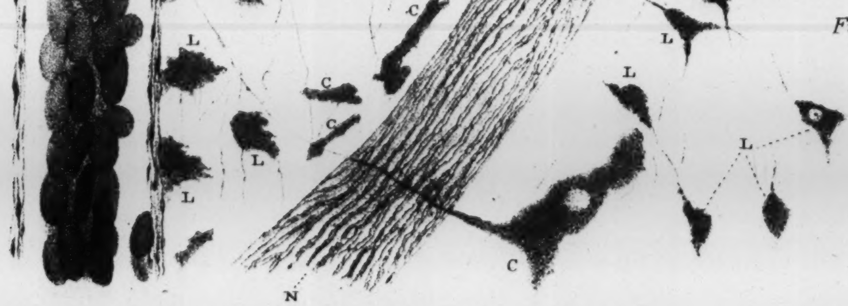
Fig. 2.

Scale ×12.



Scale, 1/1000 inch.

Fig. 3.



Scale, 1/1000 inch.

Fig. 4.

W.H.Wesley lith. W.West & Co imp.

pins stuck into the cork ring. As thus displayed, the smooth surface of the organ of course rested on the glass, the papillary surface looking upwards.

To expose the submucous tissue, Cohnheim found it necessary to divide the mucous membrane to the extent of an eighth of an inch; by doing so he was able to obtain a sufficient surface for microscopical examination, in which, if care was taken to keep it constantly moist with serum and to avoid undue stretching, the circulation could be observed for many hours. Although, as compared with the one to be immediately described, the method was imperfect, it was much superior to any which had been employed before for the study of the textural changes which are associated with the process of inflammation.

In the stratum of tissue thus exposed, the objects which first attract attention are, it need scarcely be said, the arteries, veins, and capillaries, and the rapidly circulating blood. In addition to these, various fibrous structures present themselves, namely striped muscular fibres, single or in groups, some entire, others broken; nerves, each consisting of a variable number of dark-bordered nerve-fibres, bundles of white fibrous tissue, and very numerous single fibrils. In the spaces between these structures a number of bodies are seen scattered without apparent regularity in the fine transparent membrane of areolar tissue. With reference to these bodies, which were first described by Cohnheim, and constituted the principal subject of the paper now referred to, he remarks that although they differ considerably in form and appearance from the fixed elements of areolar tissue elsewhere, they can only be regarded as "connective-tissue corpuscles."

Cohnheim found that when this tissue, immediately after having been exposed in the manner above described, was observed continuously for many hours under the microscope, the circulation became much accelerated, and the vessels (veins and arteries) became dilated. Soon the dilatation of the arteries diminished, while the motion of the blood became slower, especially in the veins of which the diameter was still larger than in the natural state. In a short time the colourless corpuscles began to hug the walls of the veins, and soon after emigration set in with great vigour. As this went on, it was seen that in numerous capillaries stasis was either commencing or complete, a state of things which rapidly led to diapedesis, affecting both capillaries and veins.

These facts having been ascertained, and being moreover in complete accordance with what Cohnheim had himself described in inflamed parts elsewhere, it remained to inquire what part the fixed elements played in the active changes going on around them. For our present purpose it is sufficient to state that Cohnheim concluded that they took no part whatever in those changes; and he used this fact in support of his general position, that fixed elements of tissues do not participate in any inflammatory processes of which those tissues may be the seat.

But since 1868, as is well known, Cohnheim's conclusions on this sub-

ject have been warmly disputed. On the one hand the pathological histologists of the Vienna School have maintained, on the basis of much laborious work done by Prof. Stricker and his pupils, the previously received belief as to the textural origin of those young cells the presence of which is the most essential characteristic of inflammation. On the other hand Cohnheim, supported by Axel Key and many others, has strengthened his view of the case by extending the research in new directions.

As regards the tongue of the frog, Prof. Stricker has published observations in which, following Cohnheim's own method, he arrived at opposite conclusions.

I have thought it desirable to publish the observations here recorded, because the methods now adopted appear preferable to any previously employed, the tongue of the toad being much better adapted for the study of the tissues than that of the frog.

The organ, when protruded, extends nearly an inch out of the mouth; and in this state exhibits near the mouth the form of a flattened cylinder, of which the cross section is oval. Towards its extremity it becomes flattened, and exhibits a tendency to bifurcation, ending in two short tips, often called cornua. Of its two principal surfaces, of which one is beset with papillæ, the other smooth, the former (supposing the animal to be in the supine position) is undermost. But when the organ is retracted, and occupies its usual position in the mouth, it is bent back in such a way that the papillated surface looks towards the palate.

The arrangement and anatomical relations of the structures which constitute the substance of the tongue may be most readily understood by the examination of transverse sections. In any vertical section of a properly hardened tongue across the thicker part of the organ it is seen that immediately underneath the mucous membrane of the smooth surface there is a large cavity, which, from its lining of flat cells, the anatomist at once recognizes as a lymph-sac. The floor of this lymph-sac is formed towards the middle line by a mass of muscular fibres, of which the direction is longitudinal, and from which the liquid contents are only separated by the cellular lining. The under surface of the muscular mass is also covered by cells which form the lining of a second lymphatic cavity, which is in a similar relation to the papillated mucous membrane to that in which the principal lymph-sac stands to the mucous membrane of the smooth surface. There is, however, between the lymphatic cavity and the mucous tissue a superficial stratum of muscular fibres. In fresh preparations it can be easily made out that the deeper muscular fibres, which are nearest the attachment of the tongue, form a single bundle on either side of the middle line, spread out towards the double tip in finger-like processes having spaces between them. Through these spaces the two lymphatic sacs freely communicate, so that when liquid is injected into either sac, the other also becomes distended.

In my method of observation I followed in the main that employed by Prof. Cohnheim. That method was, however, modified in the following important particulars:—1. The toad being preferred to the frog on the grounds already stated, I found it necessary to employ very much larger quantities of curare. The dose used by Cohnheim did not exceed 0·001 grain, a quantity which is well known to be sufficient for the frog. But in the toad I found that 0·004 grain was required, and that it was necessary to repeat the injection every 36 or 48 hours during the course of each observation. 2. The support on which the body of the animal

Outline sketch of the cork support used in all the experiments (actual size). The dotted lines represent the outline of the tongue and head.

rested was not of glass, but of cork. On either side of it is a block of cork, which answers the purpose of Cohnheim's cork border. The small oblong plate of glass used by him is dispensed with. 3. In order to prepare the tongue for observation, it is necessary first to distend the lymph-sac by injecting into it $\frac{3}{4}$-per-cent. solution of common salt with the aid of a hypodermic syringe, and secondly to divide the mucous membrane which constitutes its roof with fine scissors. This having been accomplished, the cut edges are drawn aside so as to expose the surface of the septum of muscular fibres which divides the lymph-sac into two parts. A well-lighted field is thus obtained, in which the most delicate details of structure can be satisfactorily observed, even under high powers.

The injury thus inflicted on the organ is so trifling that, provided that care has been taken to guard against the production of hæmorrhage, there is at first no evidence of any pathological disturbance. Soon, however, the changes (of which an account has been already given) begin to present themselves, the several phenomena following each other in the order in which they were originally described by Prof. Cohnheim. I would only remark that the vascular changes can be studied very advantageously, and in particular that the process of emigration displays itself before the observer with wonderful beauty and distinctness.

As in my observations I confined myself entirely to the behaviour of the fixed elements of the tissue, I shall say nothing more of the vascular

changes, the interest of which to me consisted principally in that their presence afforded the evidence that the part observed was in a state of active inflammation. The question I had to answer was, whether or not this state, even when prolonged, is attended with any change whatever in the anatomical characteristics of the preexisting elements.

For this purpose more than a dozen series of observations were made on as many different animals, each series being continued for several days. At the beginning of each series a group of connective-tissue corpuscles, such as the one represented in Plate 8. figs. 3 and 4, was selected and (with the vessels and other structures in relation with it) accurately drawn with the aid of the camera lucida. The preparation was then removed from the microscope and placed in a vessel in which the air was kept saturated with aqueous vapour. The next day, after removing the layer of exuded colourless corpuscles (pus) which covered the exposed surface of the lymph-sac, by directing upon it a gentle stream of salt solution, the outlines of the group of connective tissue were again traced with the aid of the camera.

In this way several daily observations were taken in respect of each animal. It usually happened that on the fourth or fifth day the circulation became impaired or ceased; but in one instance it continued in vigour as long as nine days, during the whole of which period the same group of corpuscles was kept from time to time under observation.

The result may be stated in a single line. So long as the circulation continued, "no change whatever took place in the connective-tissue corpuscles, either as regards form or appearance," notwithstanding that the tissue of which they formed part was beset with innumerable emigrant colourless corpuscles, *i. e.* (to use ordinary language) was infiltrated with pus.

In order that the reader may be put in possession of certain facts which have not been sufficiently noticed in the summary I have now given of the results of my investigation, I will add a few short notes relating to particular experiments.

The first two experiments differed from the others in this respect, that immediately after beginning my observations I touched the observed part with a drop of water acidulated with hydrochloric acid (1 part of strong acid to 100 of water). The vascular changes of the early stage exhibited themselves in intensity, and resulted in a very abundant emigration of leucocytes; but as the observation was only continued for two days, the results were of less value as regards the special question under investigation. The fixed corpuscles were remarkably distinct, and these underwent no alteration.

In the fifth experiment (see description of fig. 3) the observation was continued for 5 days, at the end of which period the animal was killed. At that time the connective-tissue corpuscles, which were distinct and presented very remarkable contours, remained entirely unchanged.

In experiment 6 (see fig. 4) the observations were continued successfully for seven days, during the whole of which period the circulation was vigorous, although active emigration took place. Neither in this nor in any of the other cases was any extravasation of coloured blood-corpuscles, either from veins or capillaries, distinctly viewed. In this respect there may be a difference between the frog and toad.

In experiment 8 no exudation took place during the first day, the circulation going on apparently normally. Subsequently leucocytes began to escape, and exhibited their usual character and behaviour. The observation was continued for three days, but no change occurred in the fixed corpuscles.

In experiment 11 the observation was as successful as in experiment 6. The circulation was vigorous until the sixth day; emigration was abundant, and commenced immediately after the commencement of the observation. In the course of the sixth day it became feeble, and it was then observed that the connective-tissue corpuscles, although retaining their form, lost their transparency and became granular.

EXPLANATION OF PLATE 8.

Fig. 2. Diagram of vertical section of tongue, distended. A, papillated surface; B, submucous muscular layer; C, smooth under surface of mucous membrane, forming wall of the larger lymph-sac, D; G, principal venous trunks; H, principal arterial trunks, which are accompanied by nerves not shown; F, F muscular bundles; E, fine transparent membrane of connective tissue lining the lymph-sacs, and forming a continuous sheath to the bundles of muscular fibre. In this membrane are the fixed corpuscles, the subjects of observation.

Fig 3. Field of view in Experiment 5 at the commencement of the observation. Emigration has not commenced, but in the vein A the leucocytes (F, F) begin to tend towards the internal surface of the wall. Through the capillary B a few coloured corpuscles are passing. C, C are the fixed corpuscles of the tissue. The fine lines are single fibres of connective-tissue. E, E are the red blood-corpuscles.

In this experiment, in which, as already stated, the observations were continued for five days (from Oct. 23 to Oct. 29), I was able to bring the same field into view from time to time during the whole period. The vein marked D was at the beginning of the observation obliterated, having been injured in preparation. Towards the third day blood began to pass through it, and soon the circulation through it was completely reestablished. In this case the connective-tissue corpuscles represented (C C C C) were watched with the most minute attention. Notwithstanding that the emigration was most abundant, so that before each observation it was necessary to cleanse the surface of the lymph-sac by irrigation, as above described, there was no alteration of form whatever, either in the corpuscles themselves or in their nuclei, nor did they exhibit the slightest tendency to divide.

Fig. 4 represents the appearances exhibited by a vein and the neighbouring textural elements, at a later stage. In the vein V, notwithstanding that the circulation is still vigorous, an abundant emigration is in progress. Some colourless corpuscles adhere to the walls, others have already escaped and are crossing the field, mostly clinging to the bundles of connective tissue, and exhibit various

and active amœboid movements. C, C are connective-tissue corpuscles, of which one is of such remarkable form and appearance that the least change in it could be very readily observed. It contains a conspicuous vacuole, and it sends its processes along the fibrils of elastic tissue, as formerly described. N is a small nerve-trunk. M, striated muscular fibres. L, L are leucocytes—migratory colourless blood-corpuscles.

This body was kept under observation for eight days, during the whole of which emigration continued. It remained absolutely unchanged, with the exception that the vacuole varied somewhat in size. Thus on the fifth day it became somewhat more distinct than it had been before. About the same time highly refractive granules and bodies resembling *Bacteria* appeared, and the leucocytes present seemed also to contain granules. On the seventh day it was observed that the circulation was growing feeble, and the tissues were losing their transparency, a change in which the fixed corpuscles obviously participated. On the morning of the eighth day it was found that circulation had ceased.

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This specimen was kept under observation for eight days, during the whole of which emigration continued. It remained absolutely unchanged, with the exception that the vacuole of the corpuscle above described varied somewhat in size. Thus on the fifth day it became somewhat more distinct than it had been before. About the same time highly refractive granules and bodies resembling *Bacteria* appeared, and the leucocytes present seemed also to contain granules. On the seventh day it was observed that the circulation was growing feeble, and the tissues were losing their transparency, a change in which the fixed corpuscles obviously participated. On the morning of the eighth day it was found that circulation had ceased.

December 7, 1876.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

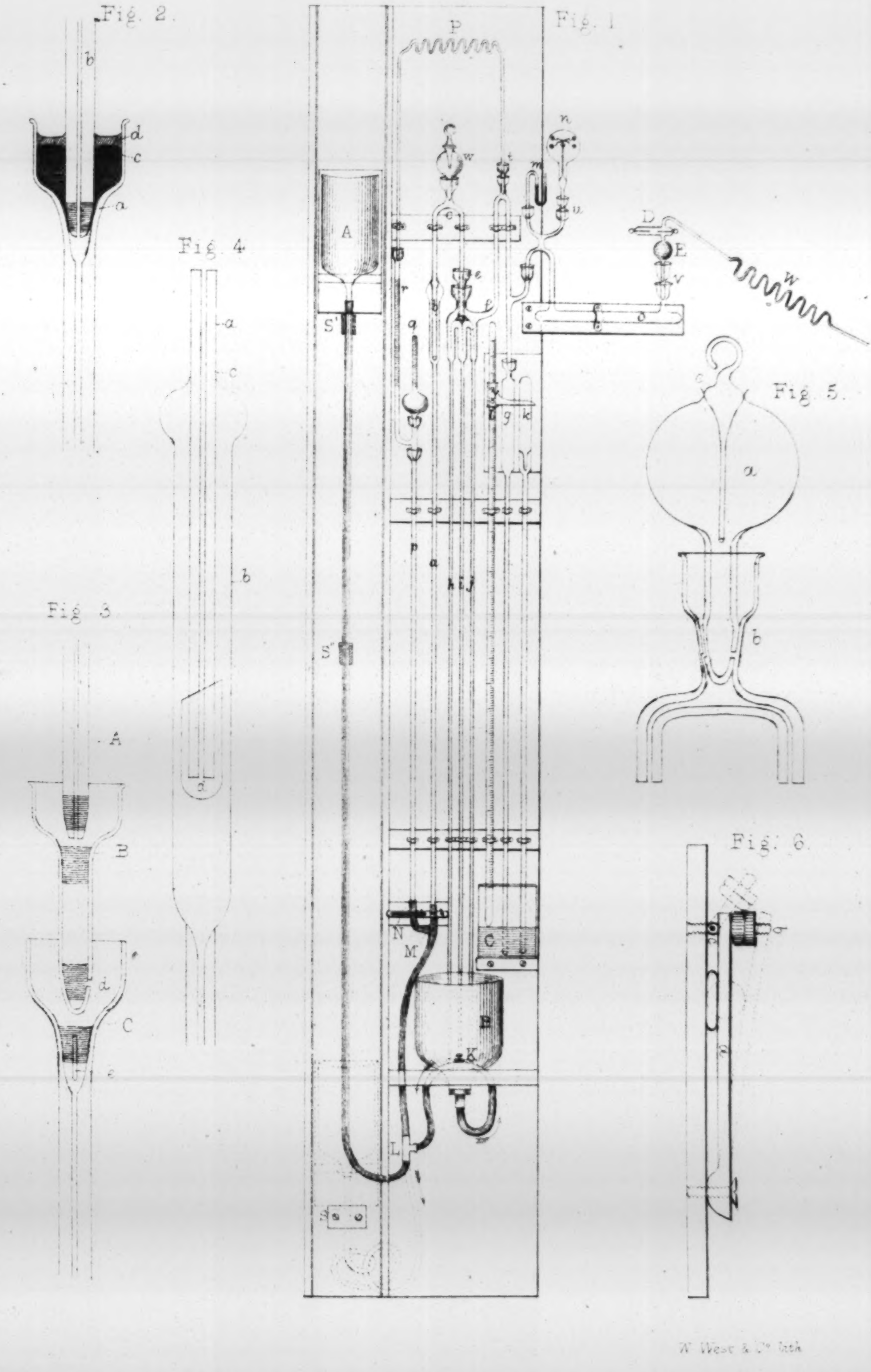
I. "On a new Form of the 'Sprengel' Air-pump and Vacuum-tap." By Charles H. Gimingham. Communicated by William Crookes, F.R.S. &c. Received August 30, 1876.

[Plate 9.]

Having had the honour of being with Mr. Crookes during the whole of his recent researches on Radiation, and knowing the importance of obtaining the highest degree of rarefaction possible, I have latterly devoted much attention to the improvement of the "Sprengel" mercury-pump. Having now succeeded in constructing an instrument yielding very satisfactory results both in degree of exhaustion and rapidity of working, I purpose giving a detailed description of it, together with a new form of vacuum-tap which has been found exceedingly useful while working with vacua.

The instrument, owing to the number of accessories, at first sight appears complicated. I will therefore first explain the principle of the pump, tracing the mercury and exhaustion through the different tubes, and then describe each adjunct separately.

Assuming the pump to be empty, the reservoir A (fig. 1) is lowered till its support rests on the stop (S) at the bottom of the stand (as shown by dotted lines in the figure), the position of the latter being so arranged



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that when A is full the level of the mercury shall be just above the ends of the fall-tubes (h, i, j) in the fixed reservoir B.

A sufficient quantity of mercury being poured into B, the pinch-cock (K) is opened, when the mercury flows through the flexible tubing* into A, in the direction shown by the arrows, till the level in each reservoir is the same; K is then closed and A raised to the stop S′. S″ is another stop for the reservoir A, placed halfway up the stand, so that when the exhaustion has proceeded sufficiently to cause the mercury to rise to the necessary height, by the excess of external pressure, much labour may be saved by only raising the reservoir to this middle stop.

The pinch-cock K being closed and the reservoir A raised, the mercury passes the three-way connexion (L) up a small length of flexible tube to the pinch-cock M, thence up the glass tube a, through the air-trap b, and, rising over the point c, falls into the jet e, where it is divided into three columns, supplying the three fall-tubes (h, i, j), and, having taken out a certain amount of air in its passage down these, is collected in B.

The exhaustion is carried through the arm f, with which it will be seen are connected, by means of mercury joints and the author's device for a vacuum-tap, the different gauges, &c. considered useful when working with high exhaustions. g is an ordinary barometer-gauge dipping into a separate reservoir C, having the barometer (k) on one side and the measuring-rod (l) on the other; the latter is a glass tube divided into millimetres measured from a point at the bottom, which is always made to touch the surface of the mercury in C before taking an observation of the height of the gauge or barometer.

m represents a small siphon-gauge, but is only of little value, owing to its small bore and the consequent interference of capillarity; this, however, could at any time be replaced by one of a larger bore where occasion to require its accurate use.

n is one of Mr. Crookes's radiometers, made on a small scale, constituting a most valuable gauge as to the exhaustion within the pump, as its rate of motion increases almost to the highest exhaustions obtained by the pump alone; and even up to an absolute vacuum, the radiometer would give under any circumstances valuable indications with regard to the state of exhaustion, as will be seen in future papers by Mr. Crookes.

The wide tube o is the sulphuric or anhydrous phosphoric acid reservoir.

Connected to the exhaust arm by the spiral P is McLeod's beautiful apparatus for measuring the exhaustion by condensing a large quantity of the residual gas into a small graduated tube, and then by allowing calculation for the different pressure.

The letters r, q, p represent this apparatus: q is the small graduated

* This tubing is specially made to stand great pressure by having a tube of canvas between two tubes of india-rubber.

tube in which the residual gas is condensed, and *r* another graduated tube, where the pressure is taken.

The supply of mercury for condensing the gas is taken from the reservoir A of the pump, passing through the pinch-cock N up the tube *p* *.

The mercury joints used to connect the different parts of this pump are small blown funnels carefully stoppered, shown in section (fig. 2): *a*, represents the funnel; *b*, the stopper; *c*, mercury; *d*, sulphuric acid.

The stopper is ground by hand with fine emery and water into the neck of the funnel, using "rotten-stone" to finish with. Stoppers made carefully in this way need no grease, as the mercury and sulphuric acid will not pass between surfaces fitting so closely. Sulphuric acid is used to ensure perfect contact between the glass and mercury.

Before going further, I will describe the vacuum-tap, which constitutes one of the principal improvements in this pump, and will, I think, prove very valuable to many who may conduct research at high exhaustions.

It consists of three parts (A, B, C, fig. 3): A is an ordinary stopper ground to fit perfectly in B; the lower end of the funnel B is a closed stopper fitting very accurately in C.

A neat hole (*d*), drilled through the centre of the stoppered or ground part of B, meets a rather deep groove (*e*) cut in the funnel C rather more than halfway up the ground part. When these three are put together (lubricating the stoppers with a very little grease or burnt india-rubber), the funnel B turns independently of A and C, so that the latter parts can be fixed in any way necessary.

The tap is closed with the funnel B in any position, except that in which the aperture *d* is opposite the groove *e*, when of course A is in connexion with C and any apparatus attached. The stoppers are proof against leakage on account of their working under mercury and sulphuric acid placed in the funnels. When the tap is turned off, in order to make it absolutely proof against leakage, supposing the tube A or any thing connected with it has to be cut or opened after exhaustion, before doing so the stopper A is slightly lifted to let a drop of mercury fall from the funnel and cover up the little aperture *d*. The tap is now a perfect mercury joint, allowing any apparatus to be taken off or fastened to A without the slightest fear of deteriorating the vacuum below C.

To again connect the tap with the pump (and therefore any apparatus to which it is blown) the stopper A is removed, and the mercury covering the small aperture *d* taken out with a fine pipette; a small globule of mercury will still remain in the little aperture, which, if desired, may be taken out with an amalgamated copper wire; otherwise, when the tap is turned on, it will fall on the side of least pressure, viz. the side best exhausted.

* For detailed description of this apparatus see McLeod's paper in Phil. Mag. for August 1874, p. 110.

After having removed the mercury, the stopper A is replaced and connected with the pump. When the exhaustion of the latter is complete the tap may be turned on without admitting any air into the apparatus.

Where this tap is used in connexion with the pump itself, the mercury covering the little aperture *d* need not be removed, as it merely runs into the pump when the tap is turned on.

There are three of these taps used in the instrument (fig. 1, *t, u, v*):—*v*, placed at the exhaust arm of the pump, is useful to turn off after the exhaustion of an instrument, to prevent air entering the pump when another apparatus is blown on (a certain amount of time is thus saved); *t* and *u* connect with the pump the two instruments having the largest cubic contents, viz. the radiometer and McLeod's apparatus, enabling them to be cut off when not in use, thereby greatly diminishing the space within the pump.

The air-trap (*b*, fig. 1) is the same as used and described by Mr. Crookes, in his first papers on "Radiation;" it is enlarged in fig. 4. The tube *a* is blown into *b* at the point *c*, and passes some distance down inside, the end being covered by a small glass cap (*d*). This cap must have sufficient room to entirely fall off the end of the tube descending inside *b*, in order to enable one to empty the trap of air into the pump when necessary, which is done by opening the pinch-cock M (fig. 1) when the reservoir A is down, thus allowing the mercury to fall in the tube *a*. The cap then drops off the end of the inner tube, and the air rushes into the pump, the latter of course having been previously exhausted. It will be easily seen how any air carried mechanically up the tube *a* (fig. 1) is caught by this trap and collects round the joint *c* (fig. 4).

Following the mercury up from the trap, we come to the sulphuric acid tap, used to lubricate and clean the fall-tubes when required. This operation, and also that of admitting air when necessary, used to be done by raising a simple stopper, kept covered with mercury and sulphuric acid; but on account of the many accidents happening through the unsteady raising of the stopper, since devising the vacuum-tap I have adapted a modification of the latter to this purpose. Fig. 5 shows the arrangement on a large scale: *a* is a bulb for the reception of sulphuric acid; *b*, a stopper and funnel, drilled and grooved as for a vacuum-tap, but the stopper having, on the opposite side to the aperture, a groove cut so far down the stopper that it shall slightly overlap with the groove in the funnel when turned so that they meet.

Now if the stopper is turned to bring the aperture opposite the groove in the funnel, sulphuric acid runs from the bulb *a* into the pump, and is carried with the mercury to the jet, where it is distributed to the three fall-tubes. On the other hand, when it is so turned that the two grooves come together, the mercury in the funnel first runs in, followed by air, of which the rate of influx and amount admitted is, by this means, under

perfect control. *c* is a simple stopper to prevent the sulphuric acid in *a* from absorbing moisture.

We now come to the jet (*e*, fig. 1): it is made with a double-stopper arrangement, so that the jet may be easily separated from the pump, should it require altering, or any stoppage occur.

The jet itself, where the mercury divides, is made of platinum and blown to the glass; the centre hole is straight, the two side ones being drilled at a slight angle, to direct the mercury into the side tubes.

Owing to the double stopper the jet may be turned, at any time, so as to direct all three streams of mercury down the centre tube if desired. Whether any advantage is gained by so doing is perhaps rather doubtful, though I think that a great force of mercury will sometimes carry entirely down minute traces of air, which a less force will only take part of the way down, the little bubble then rising again to the surface, when the work has to be recommenced.

At first the division of the one column of mercury into three streams presented rather a difficulty; but after numerous experiments I found a platinum jet most simple and successful.

The spiral (W, fig. 1) is used as a flexible joint between the apparatus and the pump; it enables one to use levelling-screws, &c. without fear of breaking the connecting-tube. Of course I am speaking with the understanding that all instruments are put on the pump by means of a mercury or blown joint, as no other is perfect when working at the highest exhaustions. India-rubber connexions, covered with glycerine, are very excellent joints, but are very disagreeable to work with, and cannot be compared with a blown or mercury joint.

The gauge (*g*, fig. 1) is fixed to the pump by a mercury joint in order that it may be easily removed and cleaned when necessary, accurate heights being much more easily taken with a perfectly clean surface of mercury.

The height of the gauge and barometer is taken by means of the slide *y* moving rather stiffly on the divided rod; the slide carries an arm projecting across both the gauge and barometer at perfect right angles to the divided rod.

Behind the top part of these instruments is placed a mirror, in order to prevent the interference of parallax while taking the heights, by using the reflected images to level the eye. A cathetometer is of course preferable; but very great accuracy may be obtained in this way.

Just above the tap at the end of the sulphuric acid reservoir is placed an electrical vacuum-tube (D), the terminals of which are made of aluminium, and only separated by one eighth of an inch. It is very convenient to have this fixed to the pump, as it is thus always in readiness to test the vacuum as regards its conductivity with the induction-coil.

Below this tube, and between it and the tap, is placed a small bulb (E) packed rather tightly with gold leaf, intended to stop any mercury vapour

that may reach here before entering the apparatus attached to the pump.

I will now refer to some of the mechanical arrangements fixed to the stand for the convenient working of the instrument.

The reservoir A is fixed on a slide running up and down the stand in grooves cut in separate pieces of wood and screwed to the stand. The stops S′ and S″ are of brass, and kept jutting out from the stand by a spring from behind; they are pressed in when the slide passes over them, and fly out again directly underneath it. When lowering the reservoir, the stops are pressed back by the thumb.

The pinch-cock K is especially contrived for rapid working. It is represented in fig. 6. The lever (*a*) is made of oak, one end being loosely screwed to the stand with an ordinary screw; the other end is cut into a fork to receive the fine threaded screw (*b*), which carries two nuts, and moves backwards and forwards on the pinned joint (*c*). Now when once the nuts on the screw (*b*) have been screwed down on the fork of the lever, so as to sufficiently compress the pipe, it is then entirely opened or shut by simply working the screw on the joint (*c*): the open position is shown by dotted lines. When closing this pinch-cock, the lever is pressed down by the thumb while the first finger places the nuts over the fork.

The pinch-cocks N, M are made in the same way as the above, but without the joint *c*, the gradual opening and shutting by means of a screw being necessary.

The stand is 5 feet 6 inches high and 11 inches broad. The fall-tubes measure about 36 inches from the highest level of mercury in B to the jet: the two side ones have a bore of about 2 millims.; the centre one is smaller (about $1\frac{1}{4}$ to $1\frac{1}{2}$ millim.), being the size according to my experience to produce the most complete vacuum. The auxiliary side tubes are made of the larger bore for the sake of rapid exhaustion up to the point at which the barometer and gauge appear to be level, when the centre tube does the principal work, although the side ones still aid considerably.

The reservoir A contains, when full, rather more than 20 lbs. of mercury, being a convenient weight to lift. With regard to the capabilities and advantages of this pump it is unnecessary for me to dwell at any length, as the vacuum that the "Sprengel" principle is capable of producing is so well known at the present time; although I may safely say that the exhaustion produced by this instrument will at least rival, if not considerably exceed, that produced by any other air-pump yet brought to bear upon research in vacua. The rapidity of working is of course the chief advantage of an instrument of this construction.

Experiment has shown that the raising of the reservoir and passing of its contents through the pump five times will reduce the pressure within an apparatus having a cubic content of about 80 cubic centims. to ·041 millims., measured by McLeod's apparatus. But with regard to these

points, I would rather refer to an elaborate series of experiments by Mr. Crookes, in which this pump plays a most important part, shortly to be brought before the Royal Society.

NOTE.—Since writing the above description, while working with McLeod's apparatus it has been found necessary to introduce an air-trap similar to *b*, fig. 1, between the pinch-cock N and the apparatus, as small bubbles of air were frequently seen creeping up the pipe *p*.

II. "The Diurnal Variations of the Wind and Barometric Pressure." By FRED. CHAMBERS, Meteorological Reporter for the Bombay Presidency, and Acting Superintendent of the Colába Observatory, Bombay. Communicated by CHARLES CHAMBERS, F.R.S. Received November 9, 1876.

In a paper which was read before the Royal Society in 1873, and which was honoured with a place in the 'Philosophical Transactions' of that year *, I discussed the diurnal variations of the wind and barometric pressure at Bombay, and deduced therefrom the fact that a system of diurnal wind-currents moves synchronally with the diurnal variation of barometric pressure. Reasons were given for believing that that system of diurnal wind-currents is a universal phenomenon; and on that hypothesis I showed how the diurnal variations of the barometer could be explained as a result of those currents.

I have lately examined closely the "Discussion of the Anemometrical Results furnished by the self-recording Anemometer at Bermuda," which forms Appendix II. of the 'Quarterly Weather-Report of the Meteorological Office, London,' July to September 1872. Those results support the conclusions arrived at in my former paper in such a remarkable manner as to justify the readvancement of some of them in a form which will prominently exhibit their relation to the diurnal variation of the barometer.

The following Table (p. 403) exhibits the mean diurnal variations of the north and east components of the wind, in miles per hour, at Bermuda, for the whole year and for the summer and winter half-years. It is compiled from Table II. of the Appendix to the Quarterly Weather-Report already referred to.

These variations are also represented graphically by figures 1 to 9 (p. 404), the variations of the north and east components for each of these periods being exhibited separately by figures 1 to 6, and in a combined form by figures 7 to 9.

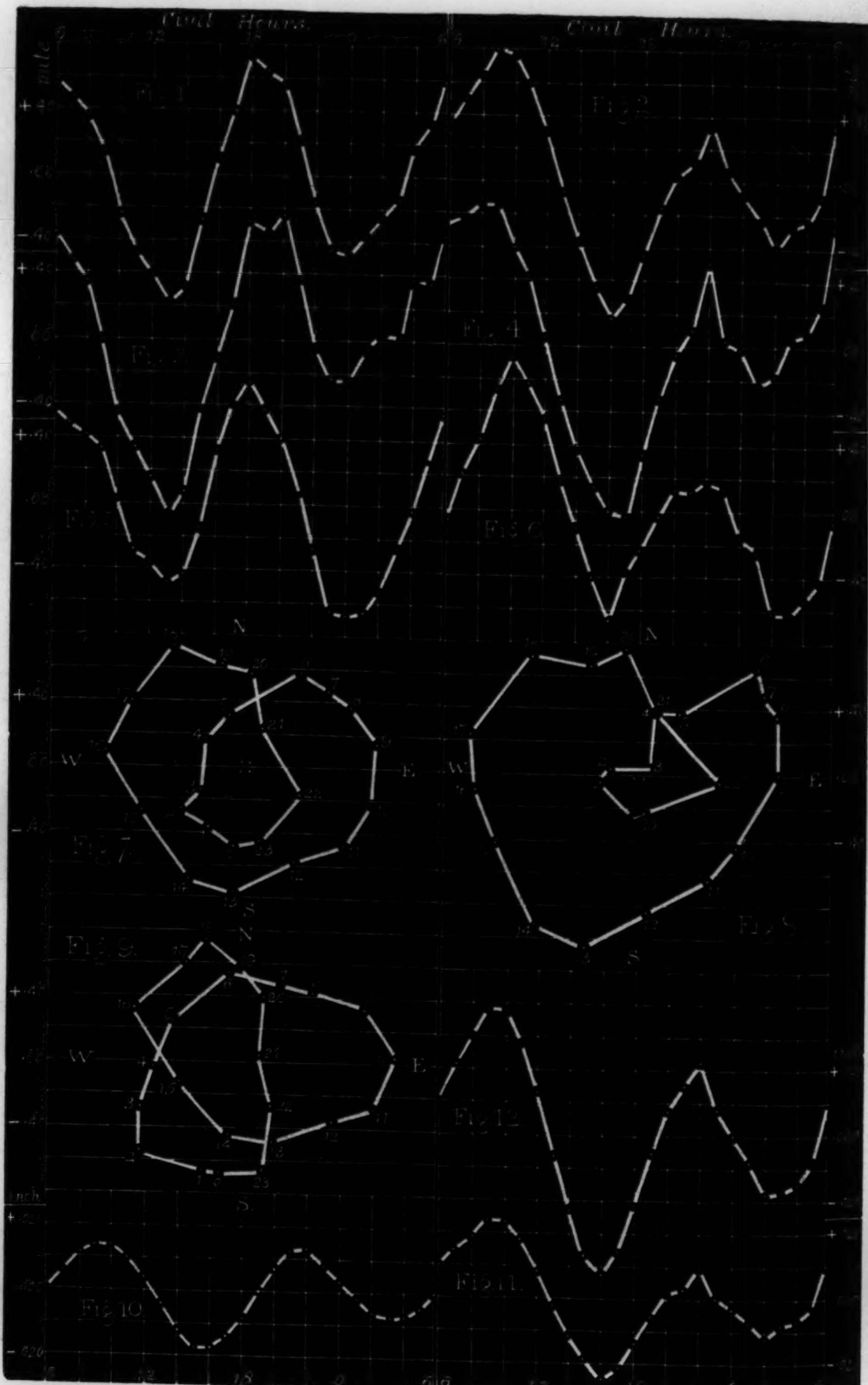
Comparing figure 7 with figure 9 (plate xxxiv.) of the 'Transactions' for 1873, we see that the figures are remarkably similar in form. Both curves exhibit a double right-handed rotation in the twenty-four hours, and all the hours of the day are respectively similarly situated on both

* Pages 1 to 18.

TABLE I.

Civil hours........	Components	0	1	2	3	4	5	6	7	8	9	10	11
Whole year.	N.	−·47	−·36	−·26	−·13	+·18	+·31	+·59	+·48	+·38	+·16	−·21	−·47
	E.	−·08	−·23	−·40	−·28	−·23	−·09	+·34	+·50	+·63	+·80	+·77	+·60
April to September.	N.	−·68	−·66	−·53	−·27	−·01	+·26	+·57	+·49	+·41	+·34	+·02	−·29
	E.	−·16	−·24	−·62	−·62	−·55	−·43	−·06	+·21	+·41	+·74	+·94	+·79
October to March.	N.	−·25	−·04	+·02	+·02	+·39	+·37	+·63	+·48	+·36	·00	−·43	−·63
	E.	−·01	−·22	−·19	+·05	+·09	+·24	+·73	+·78	+·85	+·86	+·60	+·41

Civil hours........	Components.	12	13	14	15	16	17	18	19	20	21	22	23
Whole year.	N.	−·59	−·78	−·68	−·27	+·10	+·40	+·73	+·62	+·58	+·21	−·17	−·44
	E.	+·30	−·06	−·36	−·63	−·83	−·70	−·43	−·16	+·03	+·11	+·35	+·07
April to September.	N.	−·35	−·48	−·43	−·12	+·32	+·60	+·76	+·59	+·40	+·04	−·28	−·65
	E.	+·54	+·19	−·10	−·40	−·67	−·38	−·22	−·01	+·12	+·11	+·19	+·12
October to March.	N.	−·82	−1·06	−·91	−·41	−·10	+·22	+·71	+·66	+·78	+·39	−·05	−·22
	E.	+·05	−·32	−·62	−·86	−1·00	−1·02	−·65	−·31	−·06	+·10	+·50	+·01


Civil Hours.
Civil Hours.
Fig. 1
Fig. 2
Fig. 3
Fig. 4
Fig. 5
Fig. 6
Fig. 7
Fig. 8
Fig. 9
Fig. 10
Fig. 11
Fig. 12
N
S
E
W
inch.

curves, implying that the inferences drawn from the Bombay observation, which led to the construction of the former curve, are essentially true.

The evidence which the Bermuda wind results afford, taken together with that which I have previously advanced, is, I think, sufficient to warrant the conclusion that the existence of such a system of diurnal wind-currents should no longer be regarded as a working hypothesis merely, but that it may indeed fairly claim to be regarded as an observed fact. Accepting this conclusion, I proceed to show, in greater detail than was justifiable from the evidence advanced in my former paper, what kind of barometric variation may be expected to result from such a system of wind-currents. For this purpose the only results that will be used are the mean diurnal variations of the north and east components of the wind for the whole year at Bermuda. Strictly speaking, similar results for at least one other station in a different latitude, but otherwise similarly situated, are essential to the completeness of the explanation; but, as suitable observations for such a station are not yet available, their place will be supplied by the simplest suppositions that can be made. The results, however, will be seen not to depend wholly on those suppositions, but it will be evident that similar conclusions will follow without them.

Bermuda, being a small island in mid-ocean, is specially favourably situated for the investigation of such movements of the atmosphere as form part of a general system of diurnal wind-currents affecting the whole surface of the earth; for in this case the investigation is not complicated by the great difficulty of having to eliminate those peculiarly local winds, such as the land- and sea-breezes, which are always found on the coasts of extensive tracts of land, and such as the secondary systems of diurnal wind-currents, of which we have indications in the hot winds that blow in the daytime from the interior of large continents like India, and which are to be attributed to local causes similar in character to those which produce the primary system of diurnal wind-currents over the whole of the earth's surface—these secondary systems originating in outward movements from the middle of heated continents, in the same way that the primary system originates in outward movements from the middle of the heated hemisphere.

The quantity of air (measured by the number of square miles of surface on which it rests) which enters or leaves the meteorological blockade formed by the two contiguous full-hour meridian lines $h-1$ and h, and by any two parallels of latitude θ and θ', may, with sufficient approximation, be calculated by the following formulæ, provided that θ and θ' do not differ very largely :—

$$M_h = M_h^N + M_h^E \quad \ldots \ldots \ldots \ldots \ldots \quad (1)$$

where

$$\left.\begin{array}{l} M^N_h = \left\{ \left(\dfrac{N_h+N_{h-1}}{2}\right) 15^\circ \times 69{\cdot}16 \times \cos\theta \right\} \\[2ex] \qquad -\left\{ \left(\dfrac{N'_h+N'_{h-1}}{2}\right) \times 15^\circ \times 69{\cdot}16 \times \cos\theta' \right\} \end{array}\right\} \quad . \quad . \quad . \quad . \quad (2)$$

and

$$M^E_h = \left\{ \frac{E_h + E'_h}{2} - \frac{E_{h-1} + E'_{h-1}}{2} \right\} \times (\theta-\theta') \times 69{\cdot}16 \quad . \quad . \quad . \quad (3)$$

in which N_h is the north component of the wind variation for the hour h and the parallel of latitude θ, and N'_h the corresponding north component for the parallel of latitude θ', E_h the east component of the wind variation for the hour h and the parallel of latitude θ, and E'_h the corresponding east component for the parallel of latitude θ'.

Suppose, then, the existence of twenty-four blockades formed by the twenty-four meridian lines corresponding to the twenty-four full hours, and by the equator and the parallel of north latitude, taken as 32° 23′, on which Bermuda stands. Bearing carefully in mind that only the diurnal variations of the wind are being considered, the mean diurnal variation of the north component of the wind for the whole year at Bermuda affords the means of calculating how much air flows into or out of each of these blockades on the north, and the corresponding variation of the east component of the wind affords the means of calculating the quantity of air which flows into or out of them on both their east and west boundaries, *i. e.* across each of the meridian-hour lines, on the supposition that the variation of the east component of the wind is the same between Bermuda and the equator as at Bermuda, an assumption which is doubtless only approximately true, and though somewhat rough, yet sufficiently exact for our present purpose. But we have no observations to show how much air flows across the equator into or out of each of them. To tentatively supply the place of such observations the most simple supposition that can be made is, that there is no transfer of air across the equator either into or out of any of them, or, in other words, that on the equator there is no diurnal variation of the north component of the wind. It seems probable that this assumption is not strictly true, but that the position of the line at which the outward currents from the neighbourhood of the equator may be said to originate, or at which the return inward currents may be said to meet, not only has an annual variation about its mean position, but that it also varies from hour to hour, or, in other words, that there is no single line which satisfies the assumption made for all the hours of the day. But, for our present purpose, the assumption that the equator does so will probably not very essentially affect the results arrived at.

Assuming, then, that $N'_h = 0$ for all values of h, and that $E_h = E'_h$ for all values of h, we obtain the following values of M^N_h and M^E_h, and, by their addition, those of M_h:—

TABLE II.

h	0	1	2	3	4	5	6	7	8	9	10	11
M^N_h	−399	−364	−272	−171	+ 22	+215	+394	+469	+377	+237	− 22	−298
M^E_h	−336	−336	−381	+269	+112	+314	+963	+358	+291	+381	− 67	−381
M_h	−735	−700	−653	+ 98	+134	+529	+1357	+827	+668	+618	− 89	−679

h	12	13	14	15	16	17	18	19	20	21	22	23
M^N_h	−464	−600	−640	−416	− 11	+219	+495	+591	+526	+346	+ 18	−267
M^E_h	−672	−806	−672	−605	−448	+291	+605	+605	+426	+179	+537	−627
M_h	−1136	−1406	−1312	−1021	−459	+510	+1100	+1196	+952	+525	+555	−894

Now if the whole atmosphere participates in the movements which are observed in the lower strata, an increase or decrease of the pressure of the air, very nearly in direct proportion to the quantity of air which enters or leaves any blockade, must result. Assuming that the whole atmosphere does participate in these movements, and taking 30 inches as the mean barometric pressure for each of the blockades, the increment of pressure i for each hour of the day is obtained by the proportion

$$a : \mathrm{M} :: 30 : i; \text{ or, } i = \frac{\mathrm{M} \times 30}{a},$$

where a is the area of a blockade in square miles (2,287,336 square miles in the present case), and M is the number of square miles covered by the air which enters or leaves the blockade per hour. Adding successively these hourly increments, we obtain the barometric variation which results from the variation of the wind from which the values of M were calculated. The variations corresponding to the successive values of $\overset{\mathrm{N}}{\mathrm{M}}_h$, $\overset{\mathrm{E}}{\mathrm{M}}_h$, M_h of Table II. are given in the following Table (p. 409), and graphically represented by figures 10 to 12.

Figure 10 (p. 404) shows the diurnal variation of the barometer that would result from the diurnal variation of the north component of the wind only. It is remarkably regular in form, and its turning points correspond almost exactly with those of the observed barometric variation in low latitudes. The diurnal movement is greater than the nocturnal one, as with the observed variation; but the morning maximum, in accordance with what might be expected from the notion of a gradually decreasing oscillatory movement of the atmosphere, is perhaps not as proportionally high as in the curve showing actual barometric variation. It is, however, higher than the evening maximum, a fact which could not be explained by the supposition that the movements of the N component of the wind are simply of the nature of oscillatory movements resulting from the midday disturbance, but which requires that some other cause should also be in action at the same time. This is probably to be found in the gradual cooling of the air during the night hours resulting in a gradual return towards the equator of the air which had moved outwards during the day hours, causing the outward oscillatory movement of the night to be smaller than the subsequent inward movement. And this is probably also the reason why at places near the equator, like Batavia, the diurnal barometric inequalities for the night hours have greater positive or smaller negative values than at places further removed from the equator, like Bombay. Fig. 10 has, as might be expected, a smaller range than the actual variation of the barometer.

Fig. 11 is of the same general form as fig. 10, with the exception that the morning minimum occurs at two hours instead of between three and four hours as in the latter figure. This is probably an irregularity which would disappear with a longer continuation of the observations. The

TABLE III.—Calculated Barometric Variations.

h	0	1	2	3	4	5	6	7	8	9	10	11
From M_h^N	+·003	−·002	−·006	−·008	−·008	−·005	·000	+·006	+·011	+·014	+·014	+·010
From M_h^E	−·003	−·007	−·012	−·008	−·007	−·003	+·010	+·015	+·019	+·024	+·023	+·018
From M_h	·000	−·009	−·018	−·016	−·015	−·008	+·010	+·021	+·030	+·038	+·037	+·028

h	12	13	14	15	16	17	18	19	20	21	22	23
From M_h^N	+·004	−·004	−·012	−·017	−·017	−·014	−·008	·000	+·007	+·012	+·012	+·008
From M_h^E	+·009	−·002	−·011	−·019	−·025	−·021	−·013	−·005	+·001	+·003	+·010	·000
From M_h	+·013	−·006	−·023	−·036	−·042	−·035	−·021	−·005	+·008	+·015	+·022	+·008

important peculiarity of fig. 11 appears to be the high morning maximum, which is higher comparatively than the morning maximum of the actual

curve of barometric pressure. This implies that the greater height of the morning maximum of the barometer is due chiefly to an influx of air from the eastward, coming from the sun.

Fig. 12, which is formed simply by the addition of the ordinates of figs. 10 and 11, so closely corresponds to those derived from actual observation of the barometer, and its range approximates so nearly to the actual diurnal range of the barometer in low latitudes, as to leave little room to doubt that the true explanation of the large features of the diurnal variation of the barometer is to be found in the diurnal variation of the wind.

This explanation requires that in high latitudes a reversal of the barometric variations should be found to compensate that part of the diurnal barometric variation of low latitudes which is due to the variation of the north component of the wind. It also leads to the anticipation that that part of the barometric variation which is due to the variation of the east component of the wind will, speaking generally, have the same character from equator to poles, having a maximum range at the equator and vanishing at the poles, like the variation of that component of the wind on which it depends. A reason is herein found why the reversal of the actual barometric variation should not occur until very high latitudes are reached. But it will probably be found, when other wind-observations of higher latitudes than Bermuda are similarly discussed, that the part of the barometric variation which is due to the variation of the north component only, is reversed in character at a much lower latitude than the actual total variation. The latitude in which the variation may be expected to be *nil* will be that where the reversed barometric variation, due to the variation of the north component of the wind, is equal to and therefore neutralized by the direct variation due to the variation of the east component.

Other causes, such as the variation of vapour pressure and the dynamical reactions pointed out by Espy, which are the equivalents of the energy expended by or upon the atmosphere in expanding and contracting under the influence of gravity, doubtless affect the actual observed variations of the barometer to some extent; but, since the diurnal variations of the wind are sufficient to account for nearly the whole of the barometric variation, the inference that other causes really exercise but a minor influence seems inevitable.

If we could suppose the centre of gravity of each atmospheric column to remain vertically undisturbed while the increasing or decreasing tension of the air on different parts of the earth's surface, caused by the diurnal variation of temperature, was being equalized solely by lateral expansions and contractions, it could be shown that a very small lateral velocity of the air would suffice for such equalization, and that the dynamical reactions resulting from the creation of these lateral movements would be small compared with those which would result from a vertical

elevation and depression of the centre of gravity of each atmospheric column. Herein may lie the reason why lateral expansive and contractive currents are found to exist.

A reference made by Dr. Andrews, in the course of his inaugural Address to the British Association at the recent Glasgow Meeting, to the work of General Menabrea, appears to give weight to this conjecture. He says—"His great work on the determination of the pressures and tensions in an elastic system is of too abstruse a character to be discussed in this address; but the principle it contains may be briefly stated in the following words:—When any elastic system places itself in equilibrium under the action of external forces, the work developed by the internal forces is a minimum."

December 14, 1876.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

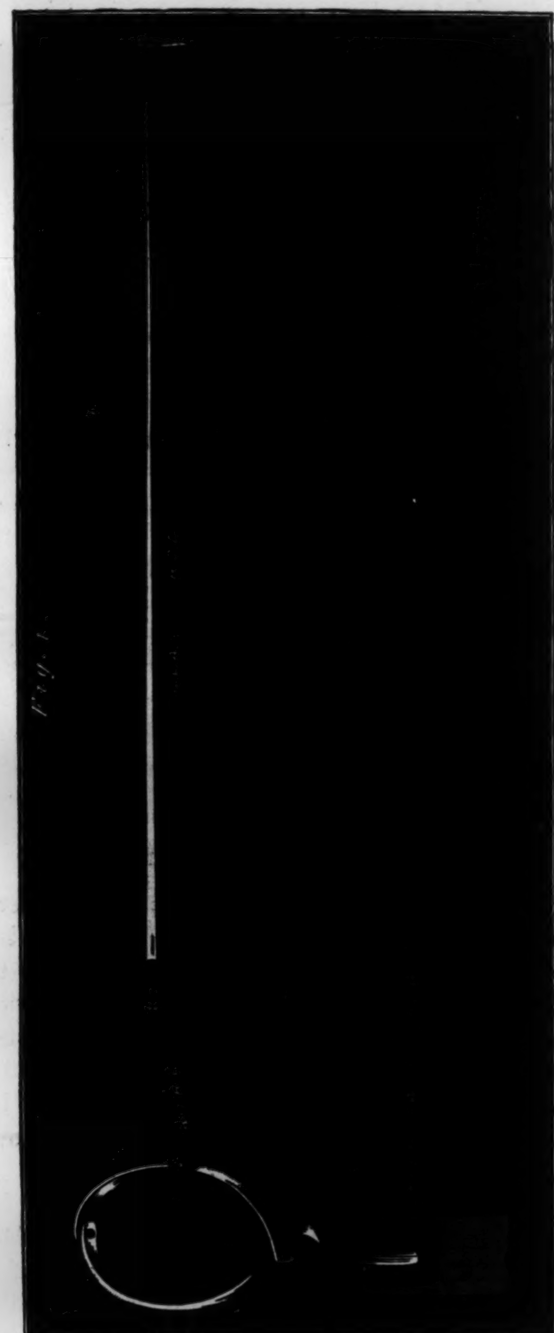
I. "On the Mechanical Effects and on the Electrical Disturbance consequent on Excitation of the Leaf of *Dionæa muscipula.*" By J. BURDON-SANDERSON, M.D., F.R.S., Professor of Physiology in University College, and F. J. M. PAGE, B.Sc., F.C.S. Received November 23, 1876.

Part I.—MECHANICAL EFFECTS.

The mechanism by which the leaf of *Dionæa* closes after mechanical excitation has been already studied by Mr. Darwin and many other naturalists. It was, however, necessary, in order to connect the electrical phenomena which form the principal subject of this paper with this mechanism, to study the successive changes of form which the leaf undergoes in the act of closing. The investigations we have made relating to this subject have brought to our knowledge facts which have an important bearing on the general question of the nature of the excito-contractile process in plants and animals.

The smooth green outer surface of a leaf of *Dionæa* in full vigour is concave, and the marginal hairs are thrown back so that they are nearly in the same plane with the lobe from the edge of which they spring. If one of the sensitive hairs of a leaf in this condition is carelessly touched the leaf usually closes. If, however, a hair is touched

very cautiously, with the aid of a camel-hair pencil, it can be predicted with certainty that no visible effect will be produced; and a similar gentle contact may be repeated several times before the leaf begins to answer to the irritation by any movement. Sooner or later,



however, the marginal hairs bend inwards, and if the leaf is carefully watched it may be observed that each touch is followed by a slight approach of the lobes to each other. If the observation is continued, it is seen that each approach exceeds its predecessor in extent, until at last the lobes suddenly come together in the manner which is, by this time, familiar to every one.

It being our primary purpose to determine the time which elapses between each several touch and the immediately resulting jerk-like approach, and to measure the extent of such approach, so as to learn how much it contributes to the final result, we made it our first business to devise some method of measurement by which we could verify the conclusions to which we had come from rough observation, namely, that each several excitation of the leaf is attended by some mechanical effect. With this view, we constructed an instrument of the form shown in figs. 1 & 1*a*. It consists of a hinged screw-clamp by which the leaf is held, as between the thumb and fore finger. On the lower jaw of the clamp, the one corresponding to the thumb, which is made of glass, the midrib of the leaf rests by its under surface. The upper jaw consists of an arch of copper wire, of which the curvature corresponds to that of the margins of a lobe of the leaf. The two jaws meet each other in such a way that when they are brought together by the screw the two ends of the arch are in apposition with the ends of the midrib close to its upper surface. Each end is pierced by a pin: the points of these pins are directed towards each other, so that together they serve as an axis of rotation for a second similar arch of which the curvature is made a little larger than the other, so that it may comprise it. From the middle of the second arch a wire springs at right angles, to which a light glass lever is attached. By means of this lever it can be rotated outwards on its axis, and thus made to diverge from its fellow at any desired angle.

Clamp ordinarily used for holding a leaf during prolonged electrical exploration.

The instrument is used as follows:—A leaf having been placed in position, that is with its midrib resting on the glass support, the two arches are brought down by the screw until they all but touch the trough of the leaf at opposite ends. This done, the arches are made to diverge

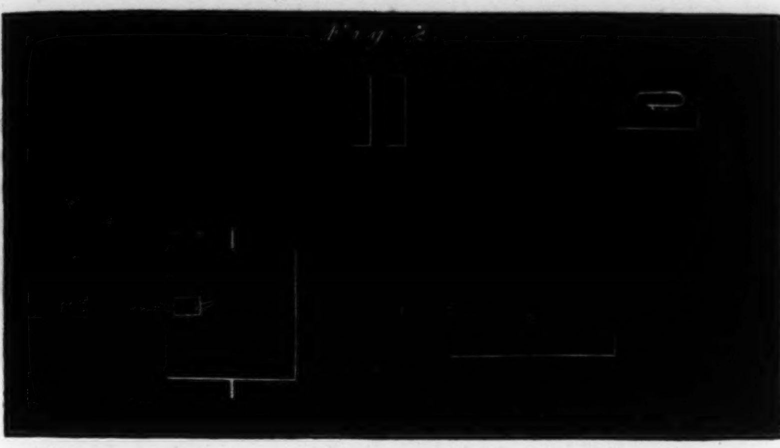
until both are in contact with the internal surface of each lobe close to its border. All that remains is to fix a graduated arc against the lever, to select a sensitive hair for excitation, carefully to touch it at regular intervals, and to record the movements of the lever. Each approach of the leaf is thus indicated in magnified proportion by the end of the lever. The following Table gives the results of an observation made in this way.

TABLE I.

Showing the result of mechanical successive excitations of the hair γ, at intervals of two minutes, continued until the leaf closed.

Number of excitations.	Angular measurement of effect.	Time in seconds which elapsed between contact and the first perceptible approach.
	°	
1 to 7	0	∞
8	0	∞
9	0	∞
10	$\frac{1}{4}$	15·5
11	$\frac{1}{4}$	10·8
12	$\frac{1}{2}$	7·3
13	1	5·8
14	$1\frac{1}{2}$	5·0
15	$1\frac{3}{4}$	4·5
16	$2\frac{1}{2}$	5·4
17	3	4·5
18	2	7·6
19	$3\frac{1}{4}$	3·8
20	$3\frac{3}{4}$	3·7
21	$4\frac{3}{4}$	3·3
22	$5\frac{1}{2}$	4·0
23	7	2·7
24	$8\frac{1}{2}$	2·5
25	8	Not observed.
26	10	2·2
27	At the 27th excitation the leaf closed.	

From this experiment, which was repeated several times and always gave similar results, it was learnt:—(1) that the first half-dozen excitations were absolutely without mechanical effect; (2) that the first effectual excitation was followed by so slight a movement that if it had not been enlarged by the lever it would have been imperceptible; and (3) that after this each successive approach of the lobes, in most cases, exceeded its predecessor. The numbers recorded in the third column relate to the time of interval between excitation and effect, and were obtained by the following method.



The apparatus (fig. 2) employed for this purpose consists of:—1, a recording-cylinder; 2, an electro-magnetic chronograph made for us in Paris by M. Verdin, the perfection of which we owe to the kindness of our friend Prof. Marey; 3, signalling-keys, of which one closes, the other opens, the signal-circuit—that of a battery of two Daniells. The cylinder, of which the surface is blackened in the usual way, revolves with great regularity five times per minute, and has a circumference of half a meter, so that the rate of horizontal movement of its surface is two and a half meters per minute (4·166 centims. per second); consequently a hundredth of a second (0·4166 millim.) is readily measurable.

In the present experiment it was necessary that the touching of the sensitive hairs should be accomplished with great gentleness, care, and exactitude, and always in the same manner. The touch in each experiment was made by one observer at an expected signal from the other, the arrangement being that A should count aloud 1, 2, 3, 4, 5, that B should touch at the moment 5 is said, and that at the same moment A should close the signal-circuit, having his eye on the lever, and being ready to break the circuit at the first perceptible movement. Considering that the periods to be measured were of several seconds' duration, this method was quite accurate enough for the purpose.

The time-measurements, as will be seen at a glance, stand in a remarkable relation to the mechanical effects, showing that the delay between excitation and effect diminishes as the extent of the effect increases, both facts having the same meaning—namely, that in the plant, as in certain cases well known to the animal physiologist, inadequate excitations when repeated exercise their influence by what has been termed summation, *i. e.* that when any number of such stimulations, say *a*, *b*, *c*, *d*, *e*, &c., follow each other in succession, the effect of each is prepared for and aided by its predecessors: so that although, as in the present instance, *a*, *b*, *c*, *d*, may seem to produce no effect whatever, each of them really produces a change in the excited structure, and each contributes, when

summed with its predecessors and successors, to the bringing about of the visible effect which follows *e*. During the remainder of the process the operation of the same law shows itself in the gradual augmentation of the increments, the last contraction, that by which the leaf closes, being the result of the summation of the excitation which immediately preceded it with all the previous excitations. Our conception of the nature of the process may be otherwise expressed by saying, that under the influence of successive excitations the latent excitability of the leaf gradually increases; for whereas before it either made no response or postponed its response indefinitely, it now answers to the same stimulus by a visible motion of which the promptitude and the extent increase together.

In one of our experiments we arranged our apparatus in such a manner as to obtain a graphic record of the successive approaches of lobe to lobe by which closure is ushered in; a reduced, but otherwise accurate, copy of this record is given in fig. 3. It shows a fact which we had already ascer-

tained by observation, namely, that in each approach the rate of motion augments rapidly at the beginning, and then very slowly subsides. It was for this reason that we allowed two minutes to elapse between each excitation and its successor; for if the interval were less, the effect of the excitation began before that of the previous one had ceased. In the experiments represented graphically the excitations were repeated every minute, so that the lever was still rising at the moment that each new ascent commenced.

It appeared important to ascertain whether, after the leaf is closed, it still continues to make mechanical efforts. We had already observed that a leaf which is repeatedly excited after closure seems to be clenched with greater and greater force, and we thought it probable that mechanical work would continue to be done by a leaf after closure if it had the opportunity. To test this, all that was necessary was to attach weights to our lever sufficient to keep the lobes expanded. The result of a single

experiment was quite conclusive. Each time, without exception, that the leaf was excited the weight (one gramme at a distance of 10 centims.) was slightly lifted, the extent of movement of the lever varying from one to three degrees.

As regards the interval between excitation and the resulting movement, our observations show that it varies from $2\frac{1}{4}$ seconds (the shortest observed after numerous excitations) to 10 seconds. The last estimate, however, is probably exaggerated; for in the early stages, when the motion is of extremely small extent, it is not possible to determine exactly when it commences. In the later stages this source of inaccuracy does not exist, so that we may confidently take two seconds as the inner limit of the period in question.

Part II.—Electrical Disturbance of the Normal Leaf.

Section 1.—*Electrical Condition of the Leaf in the unexcited State.*

The electrical condition of the leaf of *Dionæa* in the unexcited state has very recently been made the subject of a minute investigation by Prof. Munk, of Berlin. He has found (1) that if we conceive the external surface of the leaf divided into strips by parallel lines crossing the midrib nearly at right angles, and coinciding in their direction with the veining, the external surface of each lobe is negative to the midrib; (2) that in comparing different points of the midrib with each other there is one, of which the position is two thirds of the distance from the near to the far ends of the midrib, which is positive to the rest. He has further (3) stated that the potential of any point on the internal surface of the lobe is exactly equal to that of the corresponding and opposite point on the external surface.

These three statements may be generalized as follows:—On the external surface of the leaf three lines may be distinguished—one which may be called the positive line, which corresponds to the midrib; and two others, the lines of greatest negativity, which lie between midrib and margin and are nearly parallel to the first. Although it is not the special purpose of this paper to investigate this part of the subject, we may state generally that we have found the first two propositions above enumerated to be true in all normal leaves with the following exceptions, namely:—first, that although the central part of the midrib is positive to either end (more positive to the near than to the far end), the position of the point of greatest positivity is not so definite as Dr. Munk states, but differs in different leaves; secondly, that the different points in his isoelectrical negative line are never found to be absolutely identical. As regards the third proposition we are compelled to say, generally, that it is without foundation. We have found that so far from its being generally true that opposite and corresponding points of the two surfaces exhibit the same potential, such identity occurs so rarely and exceptionally, that it may be regarded as abnormal. In a future paper

we propose to discuss the electrical condition of the leaf of *Dionæa* when unexcited as compared with that of other non-excitable leaves. For our present purpose it is sufficient to state as the general result of our observations on this subject, first, that the part of the midrib which lies nearest the two central sensitive hairs is positive to every other part of the external surface of the leaf, but has usually the same potential as the petiole and other inactive parts of the plant; and secondly, that the external surface, so long as the leaf is in vigour, is *always positive to the internal surface.* These two statements, and particularly the second, may be accepted with confidence; but with reference to the first it must be borne in mind that, inasmuch as unexplained differences of potential often present themselves between symmetrical points of opposite lobes, even in leaves which appear to be in a normal state, the determination of the difference between any point and the midrib must necessarily be a matter of great difficulty.

Section 2.—*Method.*

The method employed in the present research differs from that generally used in previous investigations relating to animal or plant electricity in two important particulars, viz.:—first, in the adoption of the electrometer as a means of investigating the electrical changes; and secondly, in the substitution of a constant for a variable potential.

The electrometer used is that of Lippmann. We became acquainted with this instrument through the kindness of Prof. Marey, who had already adopted it in physiological investigations relating to animal electricity. We append the following description of the instrument, referring the reader for further information to the original paper of the author*.

The instrument consists of two glass tubes, A and B, fig. 4, of which the former is drawn out into a capillary point γ, the lumen of which is about $\frac{1}{100}$ millim. At the end opposite the capillary, A communicates

* "Beziehungen zwischen den capillaren und electrischen Erscheinungen von G. Lippmann," Poggendorff's Annalen, 1873, Bd. 149.

with a *cul-de-sac* of thick india-rubber (not shown in the diagram), the cavity of which, as well as that of the tube itself, is, with the exception of a small bubble of air, filled with mercury. The *cul-de-sac* can be compressed by a screw-clamp, so that the mercury in A can be subjected to the pressure required in order to force it through the capillary, or to any less pressure. The tube B also contains mercury at its closed end: the remainder of its cavity is occupied by dilute sulphuric acid, into which the end of the capillary γ is plunged. Each of the two tubes A and B has a platinum wire fused into it, by which the two masses of mercury can be severally brought into connexion with any two surfaces of which it may be desired to compare the electrical condition.

The instrument is prepared for use by first increasing the pressure in A until mercury escapes from γ, and then diminishing it, until it stands at a point previously fixed upon (which may be called the zero point), *care being taken that the wires α and β are in contact with each other.* If, instead of touching each other, any electromotive arrangement or structure is interposed between them, as (*e. g.*) a muscle-cylinder, the mercurial meniscus in the capillary alters its position, retreating from γ if the surface with which it is connected is negative to the other, and *vice versâ.* The difference of tension thus indicated may be measured, either by increasing or diminishing the pressure in A, so as to bring back the meniscus to its zero position, and measuring the change of *pressure* required for the purpose ("compensation pressure"), or by measuring the *distance* of the point at which the mercurial column in the capillary stands when the terminals are in contact with each other from that at which it stands when the electromotive structure is interposed. For physiological purposes the latter plan is the most convenient. In order to carry it out, the capillary must be mounted on the stage of a suitable microscope, and furnished with micrometrical arrangements capable of measuring accurately to a hundredth of a millimeter.

The electrical values to be assigned to the measurements so obtained must be learned in respect of each instrument by a preliminary process of empirical graduation.

Each capillary used as an electrometer must be graduated. The graduation is effected by Poggendorff's method of compensation, with the aid of Mr. Latimer Clarke's potentiometer. As a standard cell we have used a silver-chloride element, kindly given us for the purpose by Mr. De La Rue. The coil of our potentiometer consists of 60 turns of platinum wire of 0·26 millim. in thickness. The operation of graduating is rapidly performed; consequently it can be repeated frequently to ensure accuracy. The reasons why for our purpose we preferred the electrometer to the galvanometer in the present investigation are easily made clear. In all investigations relating to the electrical phenomena of plants and animals the object in view is to ascertain in what way any changes in the electrical condition

of a living part are correlated with other vital phenomena; and for this purpose, what is required is always to measure the electromotive force between two points differing in function (in the physiological sense). By the electrometer this measurement is made directly. In employing the galvanometer a result is obtained which acquires no absolute value until another investigation of very great difficulty has been gone virtually through—that of measuring the electrical resistance of the tissues which intervene between the two points investigated. In addition to this obvious reason for preferring the measurement of tension difference to the measurement of current, there are other reasons why the capillary electrometer above described is specially adapted to the purposes of the physiologist. One of the principal is that its indications are sensibly instantaneous, on which account it is admirably suited for the investigation of electrical changes of extremely short duration, and further that it is portable and little liable to be injured by being moved from place to place. A third advantage that it possesses is that it can be made and graduated by the investigator himself. When in addition it is further remembered that for every measurement made with the galvanometer at least ten can be made in the same time with the electrometer with greater accuracy, it does not appear unreasonable to anticipate that the latter will in future be much used for physiological investigations.

The second respect in which our method differs from those previously employed is purely physiological. It had long been known as regards the living animal body that the only tissues which are electromotive are the nervous and muscular. All others behave, so far as has been ascertained, as ordinary moist conductors. Investigated electroscopically they exhibit, so long as they are in the living state, no variation of potential. In the plant it is the same. An ordinary stem of a herbaceous plant has the same potential as the soil in which it grows, or exhibits such trifling variations that it may be said to be constant. In most of the innumerable researches which have been made in the domain of animal electricity since the early discoveries of Du Bois-Reymond, the method has been adopted of comparing the electrical state of the part to be investigated not with some other part of the organism outside of the area of electrical change, and therefore possessing in relation to such change a constant potential, but with some other part of the electrically active organ itself. Thus, in the case of muscle, the cut surface has been compared with the natural surface, the tendon with the muscular surface, &c. These considerations led us to begin our investigation by comparing those parts of the leaf of *Dionæa* which appear to be the seat of electrical change, not with other parts of the same organ, but with the earth or with some other part of the same plant which we had previously ascertained to be electrically indifferent and constant, *i. e.* free from electrical vicissitudes. The way in which this was carried out will be explained in the next section.

The greater number of the observations were made at Kew during the month of August of this year, the plants being obtained from the hothouses of the Royal Gardens, through the kindness of the Director, and brought to a conservatory in the house in which one of us resided several days before they were used. The parts to be compared were connected with the electrometer by means of electrodes of the same construction as those which we always employ for electro-physiological purposes. Each consists of a U-tube supported by a convenient holder, and half filled with saturated solution of zinc sulphate. Into one arm is plunged a zinc rod, of which the immersed end is amalgamated; into the other a glass tube filled with kaolin made into a paste with 0·75 per cent. solution of chloride of sodium. The upper end of this mass of clay forms a cushion, which can be moulded to any required shape. Electrodes of this form are convenient to work with, and possess manifold practical advantages, the greatest of all being (1) the facility with which they can be renewed in case they are found (by testing with the galvanometer) to exhibit polarity, and (2) the facility with which they can be brought into any desired position.

The arrangement most usually adopted is as follows:—The pot containing the plant had been previously kept plunged in water. Three electrodes are used; by one of them (called the fixed electrode) the damp surface of the pot is connected with a gas-pipe. The other two (called movable electrodes) are in contact with any two surfaces of the leaf which it is desired to investigate. By means of a switch either can be brought into connexion with the larger mercurial surface of the electrometer. The capillary surface is connected directly with the earth. In many of the experiments the capillary surface was connected directly with the fixed electrode.

Section 3.—*General Characters of the Electrical Disturbance.*

When the capillary mercurial surface of the electrometer is connected by the fixed electrode with the surface of the pot or of the petiole, and the movable one is in contact with any part of the surface of the leaf, whether inside or out, the effect of touching a sensitive hair is (with certain exceptions to be mentioned hereafter) to produce a transitory advance of the mercurial column in the capillary tube towards its orifice. Such a movement will hereafter be referred to as a negative excursion. Its extent may be readily measured with the aid of the vertical hair-line which crosses the field of the microscope, and the result recorded in a number which may express either the change of position of the mercurial meniscus in millimerets, or the change of potential as compared with some unit of electromotive force.

Considering that the value of the direct measurement varies according to the part of the capillary measured, it is clearly more convenient to

write all results in terms of the electromotive force of a standard cell, of which the letter d is understood to denote the hundredth part.

When the whole of the outer surface of the leaf is covered with a mass of clay moistened with salt-solution, and the mass is brought into contact with the movable electrode, the fixed contact being, as before, with pot or petiole, the effect of mechanical excitation is to produce a *negative* excursion, indicating a change of potential at the movable contact of from $3{\cdot}5d$ to $5{\cdot}0d$: when a similar plug is applied to the internal surface, so as to cover the whole of it, the result is the same, but the extent of the excursion is somewhat less. Hence it may be generally stated that the electrical disturbance (which, as will be afterwards shown, lasts for about a second) consists in this, that the surface of the leaf becomes more negative as compared with any other surface of which the potential is constant, and that, on the external surface, the change is greater than on the internal.

The electrical disturbance is strictly limited to the surface of the leaf. If, the fixed electrode being in contact with the petiole, the movable one is brought into contact first with the midrib at its middle and then in succession with points nearer and nearer to the petiole until at last the line is crossed which divides the petiole from the isthmus or bridge by which it is united with the leaf, and the leaf is excited after each change of contact, it is found that each excitation is followed by a negative excursion *so long as the contact is on the leaf side of the line referred to*, but that as soon as that line is passed no sign of electrical disturbance manifests itself. Hence if two points of contact, a and b, are selected on opposite sides of the limiting line, and the movable electrode so shifted alternately from one to the other, an excursion of $1d$ or $2d$ is observed in the one case, while in the other the mercurial column remains absolutely motionless. We shall hereafter see that the fact that the bridge or isthmus is electromotive is of importance to the understanding of certain phenomena.

Section 4.—*On the relative Intensity of the Excitatory Electrical Disturbance at different parts of the Surface of the Leaf.*

We have already seen that the area of disturbance does not exceed that of the leaf itself. In our endeavour to limit that area further, or at all events to determine the position of the *centre of greatest intensity*, we have allowed ourselves to be guided by the consideration that as the disturbance itself must be regarded as correlative with the property possessed by the leaf of contracting when excited, that centre is likely to have its seat in the excito-contractile part of the organ. Now there can be little doubt that the excito-contractile property is localized in that part of the internal surface on which the sensitive hairs are planted, and further that the tissue which takes an active part in the changes of form by which that property manifests itself is the parenchyma, of which an

abundant stratum stretches from one lobe to the other across the midrib. This being so, we have a region roughly mapped out for us comprising so much of the internal surface of the leaf as lies within and around the sensitive hairs on either side along with the whole of the space which lies between these parts. If, now, we proceed to contemplate the region thus indicated, in relation to its structure, and limit it by two parallels crossing the midrib and following the direction of the veining, we have the whole leaf divided into three structurally analogous parts, of which the middle part may, with reference to the excitatory changes of form, and therefore, it may be presumed, with reference to the excitatory changes of potential, be regarded as representative of the other two. That we are justified in thus regarding the leaf is rendered strikingly evident by the result of an experiment in which a leaf is mutilated by first cutting away entirely the further third in the line *c* (fig. 6), and then removing all of the nearer zone, with the exception of the midrib, so that the leaf is reduced to the form exhibited in fig. 5. In such a

mutilated leaf the electrical phenomena of the whole leaf are unaltered, there being at first no difference between it and the entire leaf, either as regards the potentials of the surfaces or the changes which are produced by excitation.

It having, on these grounds, been determined that we should direct our attention to the middle third of the leaf, the next step was to fix on contact points for electrical investigation. On the internal surface of each lobe we selected the point (evidently the most important) which is equidistant from the three sensitive hairs, on the external surface the point opposite and corresponding to the first, and similarly two points on the external and internal surface respectively of the midrib, situated in the line of junction of the before-mentioned points on the lobes. We now proceed to state what results are obtained when these several points are investigated, it being understood that each of them exhibits a negative variation when it is compared with some other part of the plant lying outside of the area of excitation. We shall employ the following abbreviations—viz. the letters *l* and *m* to denote the points already indicated

on the lobes and midrib respectively; *el* for the external surface (fig. 6) and *il* for the internal surface of each lobe: *em* the external surface, and *im*

the internal surface of the midrib, and *p* for the petiole. We shall further use the capital letter P for potential, and the capital letter V for variation of potential, *em*P for the potentials of the outer surface of the midrib, and *em*P−*p*P for the difference of potential between that surface and the surface of the petiole, in other words, for the result of an observation made with the fixed contact at the petiole and the movable contact at *em*.

a. *Mechanical Excitation.*—In four leaves (*a*, *b*, *c*, *d*) observed in succession, the potentials and variations of the external surfaces of the midrib and lobe were severally, in hundredths of the standard cell, as follows:—

*em*P−*p*P	0	0	0	0
*el*P−*p*P	1·6	0	0	1·6
*em*V	−5·0	−6·5	−4·2	−4·5
*el*V	−2·0	−6·5	−4·0	−4·0

The observations given, although taken in succession, exhibit great differences of result. They serve to exemplify the general statement that, as regards the external surface, the variation is greatest near the midrib.

When the variation is compared on opposite and corresponding points, it is almost always found that the external variation exceeds the internal. Thus in nine observations the *el*V and *il*V were respectively −6·5 (*el*V) and −2·0 (*il*V), −4·5 and 0, −6·5 and −1·2, −3·5 and −2·0, −3·5 and −1·8, −6·5 and −3·0, −4·5 and −2·2, −5·5 and −2·5, −4·5 and −2·5. The following were the actual results obtained in two of these cases in which successions of alternate measurements were made:—

Leaf *a* (12 measurements) *il*P−*p*P = −1·2, *el*P−*p*P = 0.
*el*V = −3·6, −4·0, −4·2, −4·0, −4·0, −4·5.
*il*V = −1·5, −1·7, −1·6, −1·8, −2·2, −2·2,

Leaf *b* (20 measurements) *el*P−*p*P = −1·0−1·5, *il*P−*p*P = −3·5.
*el*V = −5·0, −6·2, −6·5, −5·5, −5·1, −5·0, −4·8, −4·9, −4·9, −4·9.
*il*V = −2·0, −2·0, −2·0, −3·0, −3·0, −2·1, −2·5, −2·5, −2·5, −2·4.

Similarly in a vigorous leaf specially investigated with reference to the internal and external surfaces of the midrib, the results were as follows:—

$$im\mathrm{P}-p\mathrm{P}=-3{\cdot}0,\ em\mathrm{P}-p\mathrm{P}=0,\ im\mathrm{V}=-3{\cdot}0 \text{ to} -3{\cdot}5,\ em\mathrm{V}=-5{\cdot}5.$$

In at least a dozen excitable leaves which we have investigated with reference to the conditions of the external and internal surfaces, we have met with no exception to the statement made above as to the predominance of the external variation. But in two leaves which, though apparently healthy, failed to close when excited, the external variation not only did not exceed the internal, but was actually positive, ranging from $0{\cdot}5d$ to $1{\cdot}0d$, the internal variation being normal, *i. e.* negative, and ranging from $-0{\cdot}5$ to $-1{\cdot}2$. In both instances the potential of the outer surface was unusually negative, both as compared with the petiole ($el\mathrm{P}-p\mathrm{P}=-4{\cdot}5d$) and as compared with the opposite internal surface ($il\mathrm{P}-p\mathrm{P}=-3{\cdot}5d$).

These were the only instances in which the external surface exhibited a positive variation. Other leaves, however, were observed in which the same condition presented itself on the internal surface, and was again associated with an extremely negative potential. Thus in leaf 51, in which the internal variation was $+1{\cdot}2d$ and the external $-3{\cdot}5d$, the difference of potential was $3{\cdot}8d$; and in leaf 52, in which the internal variation was $+6{\cdot}0d$ and the external $-3{\cdot}5d$, the difference of potential was $4{\cdot}5d$.

Before stating in what way we think that these apparent anomalies may be explained, we must give an account of a phenomenon of the same kind but of much more frequent occurrence. In perfectly fresh leaves, *i. e.* in leaves which have not yet been touched, it sometimes happens that when a single hair is excited at regular intervals, the first few excursions obtained with the movable contact on the internal surface of the lobe differ from the succeeding ones in being preceded by what may be provisionally called a preexcursion in the opposite direction. Thus in leaf 22 the first effect was a mere shudder of the mercurial column, followed by a very small negative excursion. The second was larger ($il\mathrm{V}=-1{\cdot}2d$) preceded by a very perceptible movement of the mercury in the opposite direction. The next two were still larger, but the preexcursion was still perceptible. After this the excursions were purely negative. Three other instances were met with of the same kind.

That any part of the surface of the leaf should exhibit a positive excursion seems, at first sight, inconsistent with the statement which has been made that the variation both of the external and internal surface of the leaf is negative. That it is not so, may, however, be readily understood from the following considerations.

We have already seen that the petiole or other part of the plant with which the fixed electrode is directly or indirectly in contact is electrically indifferent, or, in other words, serves the purpose of an ordinary moist conductor, just as if it were part of the clay plug by which the contact

is made. By means of the petiole, the whole plant and the electrode which is applied to it and the capillary tube are in contact with an electromotive part of the leaf, namely, the "bridge" already mentioned. The bridge, as we have seen, is within the area of electrical disturbance, and participates in the general negative variation; but by reason of its position, *i. e.* of the fact that it is virtually in contact with the fixed electrode, and through it with the capillary mercurial surface, its action on the electrometer, is opposed to that of the surface to which the movable electrode is applied. Supposing the variation at the two contacts to be simultaneous and to have the same sign, they must counteract each other. In general the variation at the surface of the leaf far exceeds that at the bridge, so that the sign of the excursion is purely negative; but whenever the disturbance in the neighbourhood of the bridge happens to exceed that which occurs at the movable contact in intensity, the excursion becomes partially, or entirely, positive. That this is so may be easily shown by the following experiment:—If in a leaf of which the internal lobe-variation is negative, the fixed contact is shifted from the petiole to the end of the midrib next the bridge, it is observed either that the excursion diminishes or becomes at once positive, or that there is a short negative excursion followed by a longer positive one, this signifying that although at first the lobe-variation has the better, it yields immediately after to the more intense disturbance at the midrib.

The principle thus illustrated applies not merely to the particular case now under consideration, but to every case in which the difference of potential is measured between any part of the surface of the leaf and any part of the surface of the plant assumed not to be electromotive. In every such case it must be assumed that the difference measured represents not the whole of the electromotive force exerted at the surface of the leaf, but only so much of it as is in excess of the electromotive force exerted at the end of the midrib next the bridge.

We propose to apply the term "interference excursion" to the effect observed whenever, as in the experiment referred to at the close of the last Section, contact is made with both electrodes *within the area of disturbance*; so that the electrical state of the surface is compared, not with another surface of which the potential is constant, but with a surface which is itself a seat of change.

The characters of such excursions are much more complicated than those we have hitherto considered—a fact which can be readily understood when it is borne in mind that, as will be shown in a future section, the electrical disturbance, although it exhibits the same general character everywhere, does not either begin or attain its maximum at the same moment in different parts of its area. To a great extent the characters of interference excursions may be understood, if along with the want of synchronism the differences of intensity of the changes going on at the two contacts during the period of disturbance be taken into account;

but it will be readily understood that, in some instances, the conditions are so complicated that we can scarcely hope to explain their operation satisfactorily or completely*.

b. *Electrical Stimulation.*—In the experiments of which the results were communicated to the Royal Society in 1873 it was found that the passage of induction-shocks in rapid succession (faradization) through the tissue of the leaf in the immediate neighbourhood of the external surface of a lobe produced effects which resembled those which follow mechanical excitation. It also appeared that this mode of excitation was exhausting; for although a leaf could be excited at 2 minutes' intervals for an hour or two without any appreciable impairment of its excitability, it soon ceased to respond when excited very frequently.

During the last summer we have confirmed these observations, using not only faradization but single induction-shocks. For both purposes we have found it convenient to employ as exciting electrodes steel needles sheathed in glass and bound together so that their exposed points were about a millimeter apart. These points were thrust through the epidermis of the leaf either on the internal or external surface.

For the production of single closing shocks the primary circuit of Du Bois's induction-apparatus (that of a single Daniell) was closed at regular intervals by a Donder's fall-apparatus (a modification of Pflüger's), the opening shocks being suppressed by a suitable contrivance. The experiment was begun with a fresh leaf and with the secondary coil at a distance of from 15 to 20 centims. The electrometer indicating that there was no effect, the distance was gradually shortened until the electrical disturbance took place. The excitation was then repeated at one minute intervals until the leaf ceased to respond. It was always found that it was necessary to push up the secondary coil to within a couple of inches of the primary—in fact, to use induction-currents of such strength as would be sufficient to awaken reflex action from the cutaneous surface of the frog. There was an appreciable delay between the passage of the induction-shock and the electrical disturbance which resulted from it, a delay of which the duration varied according to conditions to be discussed in the next section.

When a leaf is excited at intervals of a minute, or oftener, by closing shocks which are of just sufficient intensity to produce a response, it invariably happens that after a time the effect ceases, *i. e.* that the mercurial column remains motionless. The effects can, however, be reproduced either by shifting the needle-points to a new spot or by diminishing the distance of the secondary coil. The same result can be attained without interfering either with the coil or the electrodes, by allowing the leaf to rest for a longer interval.

* Compare Du Bois-Reymond, "Ueber die neg. Schwankung des Muskel-Stromes" (Arch. f. Anat. u. Physiol. 1873, p. 549). In this paper is described an analogous effect (doppelsinnige Schwankung) observed in the gastrocnemius and triceps of the frog.

We next proceed to compare the effects of single shocks with that of faradization. The difference is striking. If the same experimental method is employed as before, with the exception that each excitation consists of the passage through the tissue not of a single induction-shock, but of a rapid succession of such shocks in alternately opposite directions, it is found that an effect is obtained with currents of relatively small intensity, so that if the experiment is begun with the coil at 20 centims. distance, and pushed up a centimeter at a time, the leaf begins to respond after one or two approximations.

This observation appeared to us to indicate plainly that, with relation to stimuli, the excitability of the leaf resembles that of the terminal organs of the higher animals, with reference to which it has been lately ascertained that they can be so excited as to awaken reflexes by relatively feeble electrical stimuli, if applied at very short intervals and repeatedly. We therefore proceeded to make experiments in which each excitation consisted of a definite number of induction-shocks in rapid succession. The results were in accordance with our expectations. It having been ascertained that with the electrodes in a certain position a single instantaneous contact had no effect when the secondary coil was at 7 centims., but was followed by an excursion when it was pushed into 6·5 centims., it was set once more at 7 centims. Again a single contact was without result; two such contacts at $\frac{1}{5}$-second intervals were also futile; but after 3 contacts there was an excursion. This experiment having been several times repeated, the coil was pushed back to 8 centims., and successive experiments were made with series of 2, 3, and 4 contacts at $\frac{1}{5}$ intervals, which were in each case without effect. After 5 contacts, however, there was an excursion. On now interposing the vibrator of the induction-apparatus, of which the rate of vibration was 30 per second, it was found that an excursion was readily produced at a distance of 11 centims., thus showing that the feebler the excitation the more frequently must it be repeated in order that a result may follow.

These facts afford the key to the understanding of the phenomena when the leaf is excited at short intervals by faradization, the excitation being continued each time until an excursion is produced. When this plan is adopted we have the opportunity of observing the combined influence of summation and of gradually increasing exhaustion. At first the leaf responds after eight or ten excitations, and a series of results present themselves quite analogous to those related in the preceding paragraph; but as the tissue immediately surrounding the electrodes loses its excitability, the number of excitations required to awaken it to action rapidly increases, the effect being postponed for longer and longer periods until it finally fails to occur.

Thus, in eleven successive series of excitations at one-minute intervals,

the number of double shocks which preceded the excursion increased as follows:—

18, 18, 20, 23, 21, 24, 24, 27, 33, 32, 74.

After the latter no further effect was producible.

The result may be thus interpreted. Each excursion is an effect to which each of the induction-shocks which precedes it contributes. At first the summation is rapid; for the tissue traversed by the currents is still prompt to enter into that molecular change of which the excursion is the visible sign. As this promptitude to change or, as it might be called, explosiveness, diminishes, the number of individual stimuli required in order to bring about the discharge becomes rapidly larger and larger, until at last the result is indefinitely postponed.

When a leaf is excited at regular intervals by single shocks of such intensity as to be just beyond the limit of adequacy, so that the slightest diminution would render them futile, it is sometimes observed that the effects become rhythmical. Thus in a series of 54 successive excitations at half-minute intervals, we obtained the following results:—Excitations 1, 2, 3, and 4 were effectual; but of the sixteen excitations following, every other was futile, the alternate ones being followed by excursions; then followed during 8 minutes a series of futile excitations, after which the leaf was allowed to rest for 2 minutes. On resuming, the alternate rhythm again appeared for six excitations, then becoming modified so that an excursion followed every fourth instead of every third excitation, a state of things which continued for a quarter of an hour. In other instances the same tendency showed itself, but less distinctly, the usual result being (as has been already stated) that no further effect was produced so long as the same spot was acted upon by currents of the same intensities.

The fact that by changing the seat of insertion of the needle-points the excursions could at any time be reproduced we regard as of importance, as showing that the excitability of the plant is a property possessed, so to speak, independently by the protoplasm of every cell in the excitable area. When, after repeated excitations at any particular point, effects cease to manifest themselves, their absence denotes, not that the whole leaf is exhausted (for if it were so, change of insertion would not renew them), but merely that the excitability of the tissue in the immediate contact with the needle-points has been blunted.

Section 5.—*The Electrical Disturbance considered in Relation to the Time which it occupies.*

It has already been stated that the change of form consequent on excitation does not begin until electrical disturbance is entirely over. In other words, the latter occupies a period during which, while no visible changes are taking place, molecular changes must certainly be in progress in the excited part—a period which, with reference to muscle, Du Bois-

Reymond has fitly termed the "period of latent excitation." In the plant the time occupied by this preparatory and invisible change is, as might be expected, many times as great as it is in animal muscle; and in consequence of this greater prolongation the electrical phenomena, the only ones with which we are as yet acquainted, which accompany it can be studied with much greater completeness. In muscle the electrical disturbance begins, according to the researches of Bernstein, confirmed by Du Bois-Reymond, about 0″·005 after electrical excitation of the nerve*. In the plant this period, which may be called the period of electrical delay, is always of perceptible duration, and may last over a second. To measure it we have employed the apparatus previously described as used for investigating the time of commencement of the first mechanical effects, with the exception that the signalling-key is so modified that the same act which closes the signal-circuit excites the leaf. This is effected by fixing to the spring of the closing-key a lead wire which carries at its end a fine camel-hair pencil, so that when the spring is depressed contact is made, and the hairs of the leaf are touched at the same moment.

The time which intervenes between excitation and the beginning of the electrical disturbance varies in different leaves according to their vigour, but is very much affected by variations of temperature. In summer weather and with normal leaves the variation on the external surface at the midrib, or at the outer surface of the lobe, when the sensitive hairs on the same side are touched, is found to begin about one eighth of a second after excitation. When the opposite hairs are excited, the period is increased to a quarter of a second. In six fairly normal leaves under various conditions, in which the delay was measured with the fixed contact on the petiole, and the movable one on the outer surface of one lobe, the results given in the following Table were obtained.

TABLE II.

No. of Leaf.	Date.	Mean temperature of day.	Hairs of same side excited.		Hairs of opposite side excited.	
			Mean delay in seconds.	Number of observations.	Mean delay in seconds.	Number of observations.
a	Aug. 10	64°·8 F.	0·13	4	0·24	3
b	„ 11	62·4	0·09	4	0·23	2
c	„ 15	76·2	0·17	4	0·23	3
d	„ „	76·2	0·34	4	0·51	3
e	„ 21	64·7	0·23	14	0·56	6
f	Oct. 23		0·43	3	0·65	3

In six other leaves in which the delay was measured at the outer surface of the midrib, the mean results were as follows:—

* Du Bois-Reymond, *loc. cit.* p. 575.

TABLE III.

No. of Leaf.	Date.	Mean temperature of day.	Mean delay in seconds.	Number of observations.
g	Aug. 21	64°·7 F.	0·24	5
h	" "	64·7	0·26	13
i	Oct. 11	57·7	0·23	3
k	" 19	56·4	0·23	3
l	" 21	46·4	0·48	6
m	" 22		0·33	2

Combining the numbers contained in the two Tables, we have 0·295 as the mean delay at midrib, 0·231 as the mean delay at the outer surface of the lobe when the seat of excitation is close to the contact at which its effect is observed, and lastly 0·403 as the delay when the electrical disturbance has to make its way through the midrib from the opposite side of the leaf; so that 0·17, or one sixth of a second, is the time required for the transmission of the effects from one side to the other. All the observations on which these numbers are founded were made by mechanical stimulation. In a few experiments we substituted stimulation by single induction-shocks, modifying our apparatus by introducing the electro-magnetic chronograph into the primary circuit, so that the moment of its closure was recorded on the cylinder. The results we obtained were remarkably uniform, and confirmed those already recorded. Thus in four observations in which the needle-points were inserted into the outer surface of one lobe close to the movable contact, the intervals were respectively 0·25, 0·25, 0·28, 0·25. When they were inserted into the opposite lobe of the same leaf they were 0·42, 0·49, 0·52, 0·48, 0·56, 0·45, 0·54, 0·52, 0·50, 0·46, 0·55. The mean gives 0·18 as the time which the variation takes in order to cross from one side to the other. If we assume the distance thus traversed by what we may call the wave of negative variation to be 8 millims., more or less, we have the rate of propagation about 4·4 centims. per second, that is 600 times as slow as in nerve. This estimate is, no doubt, too low; for some of the observations were made in cool weather, and we now know that the process is much affected by temperature. If we take as our basis, observations made under the most favourable circumstances in this respect, we have for the outside of the leaf close to the seat of excitation 0·13, and for the opposite side 0·24, which gives 0·11 instead of 0·18 as the time required for transmission. In one instance, indeed, we observed by repeated careful measurements as short a period as 0·06 under normal circumstances. In the experiments to be mentioned in the next section, when the leaves were artificially warmed, the delay was similarly abbreviated. This subject requires further investigation *.

* It is to be remembered that the measurements were not made in the hothouse, but in an ordinary room. At the time they were made we were not aware of the remarkable influence of temperature.

In the few cases in which we determined the moment at which the variation commenced (by mechanical stimulation) on the internal and external surface of the leaf respectively, we found that there was a difference in favour of the outside. Thus in one leaf (89) the delay at the outer surface of the lobe was 0·18, at the inner 0·30, at the outer surface of the midrib 0·34, and at the inner 0·38.

When, however, electrical stimulation is used, the difference becomes very obvious. In one of the experiments already referred to, relating to faradization with weak currents, we employed two movable electrodes, one on the external, the other on the internal surface of a lobe of a leaf, the fixed contact being on the petiole, and the needles being inserted into the opposite lobe. When by this means the excursions were taken alternately from the outer and inner surface, it became very obvious that in every instance the outside excursion began first. At the commencement of the experiment when the condition of the excited part remained fairly normal, the delays in seconds were as follows :—

Outside .... 0·48, 0·50, 0·52, 0·65, 0·49.
Inside .... 0·71, 0·61, 0·68, 0·75, 0·95.

As exhaustion came on, the difference was more and more obvious. The following observations were made at intervals of a minute :—

Outside .... 0·69, 0·82, 0·93, 0·78, 0·98, 2·26.
Inside .... 0·89, 0·99, 1·13, 1·08, 1·43, 3·57.

After which no further results were produced until the distance of the secondary coil was reduced from eight to seven centims. On then exciting every half minute the results were :—

Outside .... 0·56, 0·66, 1·32.
Inside .... 0·71, 1·43, 1·89.

After bringing the coil to six centims. we had :—

Outside .... 0·60, 1·08.
Inside .... 0·82, 2·07.

When, instead of investigating contacts comprised within the central parts of the leaf, a comparison is made between the time at which the excursion begins at the centre with that at which it becomes appreciable towards the margin of the area of excitation, delays very much greater than those we have been considering present themselves. Thus, when mechanical stimulation is used, and the two movable electrodes are placed severally on the under surface of the midrib, a little beyond the middle and on the bridge, the delays were :—

On the midrib .... 0·27, 0·25, 0·22, 0·26, 0·32, 0·32, 0·32, 0·25, 0·34, 0·18;
On the bridge .... 0·66, 0·56, 0·55, 0·52, 0·51, 0·50, 0·70, 0·74, 0·49, 0·56,—

giving, as means, for the midrib 0·27, for the bridge 0·58. In another leaf similar comparisons were made between the excursion at the outer surface of the left lobe opposite the hairs and the bridge, with these results :—

On the outside .... 0·26, 0·24, 0·12, 0·18.
On the bridge.............. 0·87, 0·65, 0·85, 0·83.

In these instances the distance traversed by the wave of negative variation was not even so great as in the cases in which its progress was investigated from one side of the leaf to the other; so that it might, at first sight, be inferred from the much greater prolongation of the delay that the rate of transmission was slower towards the root of the leaf than across it. It is possible that it is so, but it cannot be inferred from the measurements; for in the case of the observations relating to the bridge two excursions are compared of very different intensity; and when this is the case the weaker one appears to be behind the other, and is, in fact, seen last even when the two culminate simultaneously.

The time at which the mercurial column reaches its furthest point (acme of excursion), and the time at which it returns to its original position, have been severally determined in a considerable number of instances in normal leaves. The results are embodied in the following Table. All of them relate to observations in which the fixed contact was at the external surface of the leaf, viz. in four cases on the outer surface of a lobe, in four cases on the midrib. In all the cases the excitation was mechanical, and the hairs touched were in the immediate neighbourhood of the contact.

TABLE IV.

Time, in seconds, after excitation of

	No. of leaf.	Beginning of excursion.	Maximum of excursion.	End of excursion.	Number of observations.
Outer surface of lobe.	*a*	0·17	Not observed.	2·16	7
	b	0·12	1·06	2·01	4
	c	0·20	0·99	1·81	8
	Do. diff. contact.	0·22	1·44	2·69	7
Midrib.	*a*	0·27	Not observed.	1·51	7
	d	0·19	Do.	1·59	4
	e	0·17	1·34	Not observed.	3
	f	0·23	1·46	2·22	3
	Means	0·19	1·26	1·99	

From this Table the general conclusion may be drawn that in normal leaves, in which the excursion begins to be appreciable by the electrometer at the external surface, at about a sixth of a second after mechanical excitation, the excursion attains its maximum in one second, and that its

return occupies about the same time, so that we may regard two seconds as the duration of the whole process of latent stimulation. We have already seen that the mechanical effect of excitation, viz. the change of form of the leaf, does not begin until at least two seconds and a half after the excitation; consequently it may be concluded that in every instance the electrical effect is entirely over before the mechanical effect begins.

Section 6.—*Influence of Temperature.*

The conspicuous analogies which, throughout our investigations, have presented themselves between the phenomena of excitation in the leaf and those which occur in nerve and muscle, rendered it of great importance to ascertain whether those physical agents which are known to exercise a decisive influence on the excitation process in animal tissue have a similar influence on that in the plant. This subject we propose to investigate more fully next season. In the mean time we submit the results of two experiments, either of which would be sufficient to show that the influence of temperature on the plant corresponds completely with what is known as to its effect on the contractile tissue of animals.

In each case a leaf, which for this purpose was necessarily detached from the plant, was investigated at the ordinary temperature of a room, then placed for twenty minutes in a chamber warmed to 45 C., and then cooled by placing a block of ice in its neighbourhood. The results of the chronometrical comparison of its condition, under these circumstances, are as follows:—

TABLE V.

Time, in seconds, after excitation of

		Beginning of excursion.	Maximum of excursion.	End of excursion.
Expt. 1.	Leaf at ordinary temperature, 18°–20° C.	0·22	0·86	Not observed.
	After 10 minutes in chamber	0·17	0·51	" "
	After 22 minutes in chamber	0·12	0·42	" "
	Cooled 5 minutes	0·29	0·92	" "
Expt. 2.	Leaf at ordinary temperature	0·23	1·46	2·2
	After 20 minutes in chamber	0·11	0·79	1·37
	Cooled 5 minutes	0·44	1·48	3·1
	Cooled 20 minutes	0·44	1·68	2·94

II. "Note on the Electromotive Properties of Muscle." By J. BURDON-SANDERSON, M.D., F.R.S. Received December 6, 1876.

In the great work entitled 'Untersuchungen über thierische Electricität,' of which the first volume was published by Professor du Bois-Reymond in 1848, the author promulgated, as the result of the remarkable investigations undertaken by him during the previous six years, certain propositions relating to the electromotive properties of muscle. These propositions (which in the original work were printed in large type) were termed by the author collectively the "Law of the muscle-current." They have been accepted by all later observers as fundamental truths. They are as follows :—

" *The Law of the Muscle-current.* I. *Active arrangements.* A. *Strong Currents.* If any point of the natural or artificial longitudinal section of a muscle is brought into connexion with any point of the natural or artificial transverse section of the same muscle, so that no tension is thereby produced, a current is indicated by any galvanoscopic apparatus introduced into the inactive conducting circuit, of which the direction in the circuit is from the longitudinal to the transverse section.—B. *Weak Currents.* a. *Currents of the transverse section.* Further, if any point of a natural or artificial transverse section of a muscle is connected in the manner already described with another point of the same transverse section, or with a point of another natural or artificial transverse section of the same muscle, which we will regard as a cylinder, and if the points are at unequal distances from the centre of the circular area of the transverse section, the galvanoscopic apparatus again indicates a current, though much weaker than the previous one, of which the direction is from the point more distant from the centre to the nearest point.—b. *Currents of the longitudinal section.* Thirdly, if a point of the natural or artificial longitudinal section, lying nearer to the geometrically central transverse section of the cylinder formed by the muscle, is brought in the same way into relation with a point of the natural or artificial longitudinal section of the same muscle more distant from the central transverse section, the galvanoscopic apparatus again indicates a current, which is, however, much weaker than that between any point of the natural or artificial longitudinal and any point on the transverse section, but is equal in strength to that between different points on one or two natural or artificial transverse sections. Its direction in the circuit is from the point lying nearer to the middle transverse section to that further removed from it.—II. *Inactive arrangements.* The galvanoscopic apparatus, on the contrary, remains at rest when two points in one or two natural or artificial transverse sections connected through the inactive conducting circuit are at an equal distance from the centre;

or when one or two points in the natural or artificial longitudinal section so connected are at an equal distance from the central transverse section."

The purpose of the present note is to put on record observations which prove

(1) That the unmutilated gastrocnemius muscle of the frog possesses electromotive properties which have not before been correctly described;

(2) That in the "muscle-cylinder" of Du Bois-Reymond, the electromotive phenomena are dependent on and determined by those of the muscle, and exhibit electrical properties which, in many important respects, are inconsistent with the "law;"

(3) That the electrical change consequent on excitation, and termed by Du Bois "negative variation," in like manner possesses characters different from those assigned to it by him;

(4) That it is not true, as averred by Professor Hermann, that the electrical state of muscle becomes negative during contraction as compared with that of muscle at rest. On the contrary, the contracting parts become positive.

The following statements refer to the gastrocnemius muscle of the frog.

1. When one terminal of the Lippmann's capillary electrometer (described in a previous paper by Mr. Page and myself) is connected by a suitable electrode (to be called the fixed electrode) with the surface of the back of a previously decapitated and pithed frog, and the other (the movable electrode) with the surface of the gastrocnemius muscle at a distance of from two to five millims. from the upper end, it is found that the position of the mercurial column is nearly unaltered, indicating that there is no, or only a slight, difference of potential between the two surfaces thus compared. If the potential of the surface of the muscle be positive, the neutral point can be found by shifting the electrode further down; if negative, by shifting it further up.

2. If now the movable electrode is gradually shoved down the surface of the muscle without breaking contact, the mercury alters its position so as to indicate greater and greater negativity of the muscular surface, until at last a point is reached near the tendon beyond which the surface again becomes positive, the positivity increasing as the tendon is approached.

3. If, the fixed contact remaining unaltered, the points already investigated are compared with other parts of the muscle, it is found that, as a rule, (*a*) any two points equidistant from the origin of the muscle exhibit the same potential, and (*b*) that the ends of the muscle exhibit potentials which are identical with the potentials of the inactive tissues (*e.g.* subcutaneous cellular tissue, bone, &c) in the neighbourhood, this being true both as regards the insertion (tendon) and the origin of the muscle.

4. If the tendon is divided and the muscle, remaining attached by its origin, is separated from its other connexions, so that it is out of contact with surrounding tissues, and in this state is reinvestigated, it is found that the negativity increases from its origin towards the negative point, and diminishes from that point towards the tendon in the same way as before, the only change observable being that the absolute difference of potential between that spot and the fixed contact is greater than before, *i.e.* that all the other less differences are proportionally exaggerated.

5. If the muscle is thrown into tetanic contraction by faradization of the sciatic nerve for a period of two seconds' duration, the excitation is accompanied by an excursion of which the direction is *positive*, and which attains its maximum during the second half of the period during which the excitation lasts, beginning to decline immediately after its cessation. The extent of the excursion is such as to indicate that the electromotive force of the muscular tissue diminishes by about a third. This phenomenon is observed before as well as after separation of the muscle from its connexions.

6. If now the muscle is severed in its upper third by a cut transverse to its axis, and again in the region of greatest negativity by a second cross cut, a muscle-cylinder is obtained having the following properties, viz. :—(1) The *upper* cut surface is negative to the natural surface in its neighbourhood, but positive to the more distant parts. Between these parts an intermediate zone may manifestly be found where the tension of the natural surface is equal to that of the upper cut surface.

(2) As compared with any point of the natural surface in this zone the *lower* cut surface is strongly negative; any point of the natural surface nearer the lower cut surface is also negative, its negativity increasing with its distance from the zone; and in like manner any point nearer the upper cut surface is positive, the more so the further it is from the zone.

(3) When the muscle-cylinder is thrown into contraction by contact with platinum wires in connexion with the ends of the secondary coil of a Du Bois's induction-apparatus, excitatory variations are observed, having the following characters :—*a.* If one terminal of the electrometer is in connexion with the natural surface at a point equidistant from both ends (Du Bois's equator), and the other with the upper cut surface, the position of the mercurial column indicates that the cut surface is negative to the natural surface, *i. e.* the former to the latter. On excitation the difference of tension between the two increases—*i. e.*, in Du Bois's language, a *positive* variation occurs. *b.* If the same part of the natural surface is now compared with the lower cut surface, and the excitation repeated, the difference of tension between the two is found to be in the same direction as in the former case, but much greater. On excitation it diminishes—*i. e.*, in Du Bois's language, a *negative* variation occurs.

7. From the foregoing facts as to the muscle-cylinder prepared in the

manner directed, it appears that it possesses properties entirely inconsistent with the "law of the muscle-current" in the following respects:—

(1) That the electrical tensions of the upper and lower cut surfaces, instead of being identical, differ widely;

(2) That the equatorial zone of the natural surface presents none of the properties assigned to it by the "law";

(3) That the excitatory variation is not in conformity with the "law."

8. On the ground of these experiments I conclude that in the gastrocnemius of the frog the part nearest the tendon is (in the natural state) negative to the opposite end, and from the investigation of other muscles of the limbs I infer that in them the distribution of electrical tension on the natural surface is governed by a similar law. I have further shown that the phenomenon hitherto known to physiologists as the "negative variation" cannot be described as "diminution of a previously existing current" between cut and natural surface, but may, in all its phases, be completely explained as a temporary diminution of the electromotive force of those parts of the muscle which are in the natural state negative as compared with the electrically inactive tissues of the body of the animal; and, as a direct consequence of this, that the excitatory variation of animal muscle is opposed in sign to that of the excito-contractile tissue of *Dionœa*.

9. This being understood, all that is observed in the muscle-cylinder can be explained as resulting from the properties exhibited by the muscle itself before its mutilation; so that it is not only true that the general properties of the cylinder can be deduced from those of the muscle as a whole, but that (as the Tables show) the special peculiarities of each particular case may be accurately foretold if the whole organ has before been investigated.

TABLE I.—Showing the Results of Observations as to the Electromotive Properties of Eight Gastrocnemius Muscles.

Muscle.	Length of muscle in centm.	Zone of greatest negativity.						Zone of greatest positivity.			Tendon.	Inside of skin &c. close to tendon.
		Muscle *in situ*.			Muscle detached.							
		Distance from insertion.	Potential.	Variation.	Distance from insertion.	Potential.	Variation.	Distance from insertion.	Potential.	Variation.		
Frog 1:												
Gastr. *r*	4·2	1·9	−4·7	+1·7	1·2	−5·5	−4·0	3·8	−1·6	0		
„ *l*	4·2	2·2	−6·2	+2·3	2·0	−5·3	−4·8	4·0	+0·5	0	−2·6	[illegible]
Frog 2:												
Gastr. *r*	4·2	1·6	−4·8	+3·2	1·2	Above −8·0	−6·0 (cir.)	4·2	+0·0	0	−2·6	[illegible]
„ *l*	4·2	1·4	−4·0	+1·4	1·7	−5·0	−5·0	4·1	+0·6	0	−1·4	[illegible]
Frog 3:												
Gastr. *r*	3·6	1.5	−8·0 (cir.)	−3·0	1·5	Above −8·0	−3·0 (cir.)	3.5	−1·0	−0·6?	−4·7	[illegible]
„ *l*	3·6	1·3	−5·5	−2·5	0·6	8·0 (cir.)	3·5	3·5	−0·3	0	−4·0	[illegible]
Frog 4:												
Gastr. *r*	3·4	1·5	−1·7	−0·8	1·5	3·7	2·0	3·4	0·0	0		
„ *l*	3·4	1·4	−3·6	−1·5	0·9	4·9	2.5	3·4	0·0	0	−2·3	[illegible]

TABLE II.—Showing the Electromotive Properties of Muscle-cylinders cut from the Eight Muscles in Table I.

Muscle.	Distance of lower cut surface from tendon.	Length of cylinder.	Distance of point F from lower cut surface.	Potentials of undermentioned points compared with F=0·0.		
				Most positive point of natural surface.	Most negative point of natural surface.	Centre of lower cut surface.
Frog 1:						
Gastr. *r*	1·6	2·0	0·5	+4·0	−2·5	−5·7
,, *l*	1·2	1·7	0·8	+4·5	0·0	−3·3
Frog 2:						
Gastr. *r*	1·4	1·5	0·85	+2·6	−3·4	−4·8
,, *l*	1·6	1·4	0·0	+2·5	−1·0	−3·4
Frog 3:						
Gastr. *r*	1·5	1·5	0·8	+0·8	−4·1	−4·8
,, *l*	1·2	1·4	0·6	+1·8	−1·5	−4·0
Frog 4:						
Gastr. *r*	1·2	1·6	0·0	+1·4	0·0	−2·9
,, *l*	1·2	1·6	0·0	+0·0	0·0	−2·9

III. "Preliminary Notice of Investigations on the Action of the Vaso-motor Nerves of Striated Muscle." By W. H. GASKELL, M.A., Trinity College, Cambridge. Communicated by Dr. MICHAEL FOSTER, F.R.S. Received November 23, 1876.

(Abstract.)

When a muscle is thrown into a state of tetanus by stimulation of its nerve, it seems, at first sight, reasonable to suppose that the contraction of the muscle substance must cause a considerable pressure on the vessels of the muscle, and, therefore, that for this reason less blood must pass through; and, if at the same time that the motor fibres are stimulated vaso-constrictor fibres are also stimulated, one must conclude that during the tetanus of a muscle there is a very much less volume of blood flowing through.

On the other hand, in order for the muscle to do work for any length of time, it is necessary that there should be a greater facility for the removal of the waste products and a more active supply of nutritive material during the state of contraction than when the muscle is at rest. This hypothesis necessitates, therefore, a greater flow of blood through the muscle during the tetanus of that muscle.

Which of these two statements is the true one, Sadler (Ludwig's 'Arbeiten,' 1869) has already indicated. As, however, his method and his results are not absolutely satisfactory, I, at the suggestion and with the help of Professor Ludwig, carried out last year, in Leipzig, a series of experiments of the same nature as his, and, by means of much improved

apparatus, was enabled to obtain much more satisfactory and trustworthy results than he did.

Speaking briefly, we found that, in the case of the quadriceps extensor of the dog, stimulation of the anterior crural nerve, by means of the interrupted current for a short time, say 15 seconds, caused a considerable outspurt of blood from the muscle-vein, followed by a complete cessation of flow, and that at the end of the tetanus there was an immense outpouring from the vein.

In the case of a longer tetanus we saw that, following upon the temporary cessation of flow, blood again began to stream out, gradually and continuously increasing in volume, until at last, even while the muscle was still in a state of tetanus, there was much more blood flowing from the vein than before the commencement of the stimulation.

On my return to England, wishing to examine more closely this phenomenon, I determined to investigate it, if possible, under the microscope in the muscles of the frog; and rejecting the tongue, for reasons stated below, I found that the mylohyoid muscle was the most suitable one for my purpose, it being easy to prepare it for microscopic observation without damaging the circulation through it, and, in fact, without even touching the muscle; whilst, owing to its thinness, the small amount of connective tissue in the neighbourhood of the vessels, and the absence of pigment-cells, it is possible here to measure with a micrometer eyepiece the diameter of vessels more accurately and easily than in any other preparation.

Upon placing this muscle under the microscope, without having previously touched the nerve, it is seen that the circulation presents much the same character as in the web, the median red-corpuscle stream with an inert layer on each side being plainly visible, although, perhaps owing to the manipulation, the arteries at first are slightly fuller and more dilated than the corresponding vessels in the web. The calibre of the smaller arteries does not, as a rule, remain for any length of time the same, variations taking place somewhat similar to what has often been described in the vessels of the web, but with this difference, that whereas in the so-called "rhythmic contractions" of the arteries in the web the artery appears to contract to a certain point and then to return to its original calibre or beyond it, in the arteries of the muscle the vessel appears to dilate from the normal calibre, and then gradually to return to that calibre or below it. These dilatations vary considerably in extent and are absolutely irregular in time, being much less marked both in frequency and extent in some frogs than in others, and depend, so it seems to me, probably upon some chance stimulation of the vessels, such as exposure to the air, &c.

Upon direct stimulation of the web by means of the interrupted current there occurs a most marked constriction, not only of the arteries between the electrodes, but extending over the whole web, both during

the stimulation and for some little time after the stimulation is over. If, however, the electrodes are applied directly to this muscle, even to that part which is furthest removed from the point of entrance of the nerve, it is possible, by careful focusing, to see that even during the tetanus of the muscle, provided that that tetanus is slight, instead of a diminished flow, instead of a marked constriction of the arteries throughout the muscle, there is, not only after the stimulation has ceased, but even during the tetanus itself, a most marked increase in the fullness of the vessels, a much greater rapidity of stream, and a very considerable dilatation of the smaller arteries, even to a larger extent than the doubling of the diameter; and if at the same time the circulation through the muscle is very languid, the arteries constricted, and many of the capillaries empty, a slight stimulation of the muscle itself is all that is necessary to cause a rapid full flow through the whole muscle. Whether the arteries immediately between the electrodes contract, I cannot yet say; I can, however, affirm positively that there is no contraction of the smaller arteries situated but a slight distance from the electrodes, or if there is, it must take place in the very short space of time necessary for refocusing on the artery under observation, as in every case, as soon as I have been able to measure the calibre again, I have found it considerably dilated. Here, then, is a marked difference between the web and mylohyoid on direct stimulation.

As to the effect of section of the nerve, I have always noticed that it is followed by a decided reddening of the corresponding muscle, the difference of colour being manifest, as previous to the section the two mylohyoid muscles are always equally pale. Upon closer examination, by first putting the muscle in position under the microscope and then cutting the nerve, it is seen that about 5 to 6 seconds after section the arteries dilate very rapidly, the dilatation soon reaching a maximum, in perhaps 20 or 30 seconds, and then gradually diminishing until the original calibre is reached, some 4 or 5 minutes after section—that is, the dilatation caused by section of the nerve is not a lasting one, but is exceedingly similar to that caused by slight mechanical stimulation of the nerve; for whether its peripheral extremity is pinched by a pair of forceps, or dipped into concentrated salt solution, or still more markedly when cut and torn by scissors and forceps, there always occurs after a brief latent period of a few seconds, during which there is no trace of constriction, a considerable rapid dilatation of the artery, which lasts but a short time, and then gradually gives way to a return to the original calibre, and is always accompanied by a more active very full stream, the inert layer having wholly disappeared, and the red corpuscles being crowded together to the very edge of the vessel. Here, then, is another marked difference between the web and the muscle.

If the peripheral end of the nerve is stimulated with a fairly strong interrupted current, so slight a dose of curare having previously been

given as thereby to cause a decided tetanus of the muscle, it is possible to observe, under a low power of the microscope, similar phenomena to those that take place in the experiments on the blood-stream in the quadriceps extensor of dogs that I have already referred to. Upon the commencement of the tetanus there is a sudden onward propulsion of the blood in the large veins, followed by a complete stoppage of the blood-stream in them, even sometimes a retrograde stream; while in the arteries the stream flows steadily on, increasing in rapidity and increasing in fullness; the arteries dilate, the capillaries on the arterial side become large, filled with blood, very active, and, finally, after a few spasmodic attempts to move onwards, the blood in the veins seems to burst the restraining barriers, moves on more and more rapidly, continually increasing in fullness, even though there is still a steady tetanus of the muscle; and at last, on ending the stimulation, there is seen an extremely rapid, greatly dilated circulation throughout the whole muscle; gradually and slowly the stream slackens, the arteries contract, and at last there is again a quiet axial stream in the arteries and a slow steady flow in the veins. At the moment of commencing and ending the stimulation there is an instantaneous stop in the arterial flow; except at these times, the blood flows continuously in the arteries during the whole stimulation. This phenomenon confirms in every particular the observations made by me at Leipzig, and explains most satisfactorily the nature of the curves obtained there.

By employing larger doses of curare it is possible to examine the effects of nerve stimulation apart from all contraction of the muscle. The following facts are observed then, in a thoroughly curarized muscle, upon stimulation of the nerve by means of an interrupted current. Whether the stimulus is long or short, there is always a rapid and very marked dilatation of the artery under examination, which does not commence until some 5 or 6 seconds after the beginning of the stimulation. During this latent period there is not the slightest trace of any constriction, the calibre of the vessel remaining either the same as before the stimulation, or if the stimulation is applied while the artery is dilating or constricting, then this dilatation or constriction continues during this period; and even if the stimulation is applied at a time when the artery is considerably dilated, there is no trace of constriction, but, on the contrary, a still further dilatation. The maximum of dilatation occurs about 20 or 30 seconds after the commencement of the stimulation, and can be so great that the diameter of the artery may attain to nearly three times that which it possessed originally; it is always accompanied by a greater rapidity and fullness of the blood-flow, the whole circulation throughout the muscle becoming much more active; it lasts, as a rule, only a few seconds at this maximum height, and then the size of the vessel gradually diminishes to the normal, the blood-stream becoming thinner and rather slower, until, as before the stimulation, there is a steady normal axial

flow. It is not possible to keep up the dilatation for any length of time, so that if the stimulation is long, say a minute or more, before the end of it the vessel may have regained its normal calibre; and if the stimulus is long enongh and strong enough, then it is possible for a secondary effect to be produced in the form of a decided constriction of the vessel following upon the dilatation and occurring after the stimulation is ended, this, again, being followed by a recovery to the normal diameter. It is thus seen that, while in the case of the web stimulation of the sciatic always causes constriction, followed after strong stimulation by dilatation, in the case of the muscle stimulation of its nerve always causes a dilatation of the vessels, followed, after strong stimulation, by a decided constriction of the same; so that it seems highly probable that, when the sciatic in the frog is stimulated, constriction in the web is accompanied by dilatation in the muscles of the leg, and dilatation in the web by constriction in those muscles. Moreover, as it is possible to keep up the constriction in the web for a much longer time, by commencing with a weak stimulus and gradually increasing its strength, so I think, too, that the dilatation in the muscle can be made more enduring by the same method.

Rhythmic stimulation of the nerve, by means of single induction-shocks, repeated at intervals of 2 or 5 seconds, produces the same kind of dilatation as the interrupted current.

If, as sometimes occurs, owing perhaps to the muscle being overstretched or some other cause, the circulation through it is found to be nearly stagnant, the arteries constricted, the capillaries barely visible, it is only necessary to stimulate the nerve in order to produce a full active circulation throughout; and this occurs even during the stimulation; while, under the same circumstances, in the web there is still further stagnation produced, still greater constriction, and it is only after the stimulation has ceased that an increased and more active flow takes place.

A marked dilatation of the vessel is often seen to occur in an apparently empty artery before the first rush of blood-corpuscles makes its appearance; and this dilatation does not always occur over the whole vessel at once, but rather parts of the previously constricted vessel appear to bulge out, the intermediate parts remaining still constricted; so that the vessel has somewhat the appearance of a string of pearls, and gradually as the vessel dilates more and more, and the blood-stream increases in volume and rapidity, the walls of the vessel lose this uneven appearance and become uniformly dilated.

Seeing, then, that even when the blood-current in the muscle is feeble (that is, when the pressure in the vessel is diminished) stimulation of the nerve always causes a marked dilatation, I determined to observe the effect of stimulation when the arterial pressure had been removed by compressing the aorta.

Directly after the aorta is compressed by a clip a steady, rather rapid con-

striction of the artery takes place, which soon reaches its limit, and the hitherto slowly moving corpuscles remain stationary, the vessel appearing empty, except for a few corpuscles stationed here and there. Upon now stimulating the nerve the vessel is seen steadily to dilate, a slow stream of corpuscles appears in it moving in the reverse direction (that is, from the veins to the artery), and this occurs without the slightest trace of muscular contraction. The dilatation is very appreciable, though not, so far as I have seen, of as great an extent as the stimulus produces when there is a normal blood-stream in the vessel; and if now the aorta is unclipped, there is at first a slight constriction, followed by a much greater dilatation.

At present it appears to me that the pressure in the vein is sufficient to account for this phenomenon; I intend, however, to carry out further experiments on this point.

As to reflex stimulation, I have never been able to cause any dilatation in the arteries of the mylohyoid by stimulation of the central ends of either the sciatic or vagus nerves; but, on the contrary, I have always seen either no effect produced, or a decided though slight constriction of the vessel—slight, that is, in comparison to the marked constriction occurring in the arteries of the web under the same circumstances.

Lovén having noticed the occurrence of dilatation in the saphena artery and in the vessels of the ear of the rabbit upon stimulation of the central end of the tibial nerve and the great auricular respectively, I have attempted to obtain similar dilatation in the web by stimulating the central ends of either the peroneal or posterior tibial nerves, the other nerve in each case being left intact, and, in the mylohyoid and pectoralis major muscles, by stimulation of the central end of the opposite mylohyoid and brachial nerves respectively, but in each case have seen no trace of dilatation, but always constriction.

As to the effect of direct stimulation of the spinal cord upon the vessels of a muscle, I think it probable that dilatation occurs, as Hafiz has asserted; but as I have not yet made any systematic experiments to determine this point, I think it best to leave this question for future consideration.

Atropin does not impair the action of the nerve on the vessels of the mylohyoid; for after repeated injections of sulphate of atropin subcutaneously, until, with very strong stimulation of the vagus, no effect could be produced on the heart, it was still easy to cause dilatation of the arteries in the muscle by stimulation of the nerve. So, too, large and repeated doses of curare produce no such effect here, as they are said to do on the vagus fibres in the heart.

As Claude Bernard has described a dilatation of branches of the facial artery and an increase of secretion in the submaxillary gland upon stimulation of the mylohyoid nerve in dogs, I have examined other muscles in the frog, and have found that the same phenomena can be produced in

the lateral portion of the rectus abdominis muscle and the abdominal portion of the pectoralis major muscle.

As to the tongue, it seemed to me that having here a more complicated organ, supplied with so many more nerves than a simple muscle, and one that had been already frequently examined without much success, it was not so suitable to the object in view. From the few experiments, however, that I have made on this organ, it seems to me that stimulation of the glossopharyngeal nerve, rather than the hypoglossal, causes dilatation of its vessels.

In concluding this short sketch of my experiments I would venture to say that as, from my own observations, stimulation of the anterior crural nerve of dogs causes dilatation of the vessels in that group of muscles known by the name of quadriceps extensor, while Sadler has shown the same for the biceps and semitendinosus muscles and for the flexor communis digitorum of the forearm, and as, in frogs, stimulation of their respective nerves causes dilatation of the vessels in the mylohyoid, pectoralis major, and rectus abdominis muscles, it is reasonable to suppose that this holds good for simple voluntary muscles throughout the body.

I would further add that the beauty of the circulation and the extent of the dilatation that may be observed in the arteries of the mylohyoid muscle lead one to hope that further investigations here may materially assist in solving the vexed question, "What is the mechanism by which dilatation of a vessel is caused?"

All the foregoing observations on the circulation in the muscles of the frog were carried on in the Physiological Laboratory of the University of Cambridge.

IV. "Note on the Photographic Spectra of Stars." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S. Received December 6, 1876.

In the year 1863 Dr. Miller and myself obtained the photograph of the spectrum of Sirius.

"On the 27th January, 1863, and on the 3rd March of the same year, when the spectrum of this star (Sirius) was caused to fall upon a sensitive collodion surface, an intense spectrum of the more refrangible part was obtained. From want of accurate adjustment of the focus, or from the motion of the star not being exactly compensated by the clock movement, or from atmospheric tremor, the spectrum, though tolerably defined at the edges, presented no indications of lines. Our other investigations have hitherto prevented us from continuing these experiments further; but we have not abandoned our intention of pursuing them" *.

I have recently resumed these experiments by the aid of the 18-inch

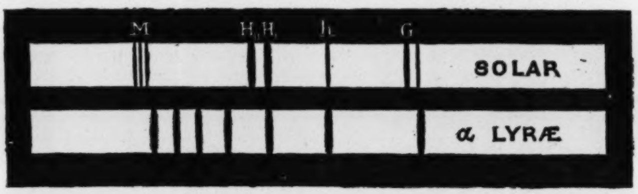
* Phil. Trans. 1864, p. 428.

speculum belonging to the Royal Society's telescope in my possession. Considerable delay has arisen from the necessity, for these observations, of a more uniform motion of the driving-clock. For this purpose, Mr. Howard Grubb has successfully applied to the clock the control of a seconds pendulum in electric connexion with a sidereal clock. This system works quite satisfactorily.

The prisms employed are made of Iceland spar, and the lenses of quartz. After an extensive trial of different photographic processes, preference has been given to dry plates.

The apparatus is so arranged that a solar or electric spectrum can be taken on the same plate, for the purpose of comparison, with the spectrum of the star. Spectra have heen obtained of Sirius, Vega, Venus, the Moon, &c.

I do not purpose in this preliminary notice to describe in detail the arrangements of the special apparatus which has been constructed, nor to offer the results of the experiments in their present incomplete state to the Royal Society. Still I venture to hope that, even in this early stage of the inquiry, the enlarged copy of the spectrum of Vega (α Lyræ)



which accompanies this note may not be regarded as altogether unworthy of attention.

After exposure to the light of Vega, the dry plate was allowed to remain in the instrument until the following morning, when a solar spectrum was taken upon it, through the half of the slit which had remained closed when the instrument was directed to the star.

The photograph shows seven strong lines, all of them slightly shaded at the sides. The two lines which are least refrangible coincide with two known lines of hydrogen in the solar spectrum.

It is expected, by means of an apparatus now in the course of construction, to obtain also any finer lines which may be present in the spectrum of this star, as well as to extend the photographic method to stars which are less bright.

I need not now refer to the many important questions in connexion with which photographic observations of stars may be of value.

December 21, 1876.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Rotation of the Plane of Polarization of Light by Reflection from the Pole of a Magnet." By GEORGE FRANCIS FITZGERALD, M.A. Communicated by G. JOHNSTONE STONEY, F.R.S. Received November 14, 1876.

At a meeting of the Dublin Scientific Club on Monday the 6th November, Professor Barrett gave the Club an account of Mr. Kerr's experiments on the rotation of the plane of polarization of a ray of light when reflected from the surface of the end of a magnet, to which additional interest was attached by the reading of a letter from Mr. Kerr to Professor Barrett giving an account of the mode of making and of the last results of his experiments. At the time I proposed trying whether any similar effects would be produced by reflection from the surface of a crystal of quartz cut perpendicularly to the axis, as I was led to think there might be, owing to the similarity of the rotatory polarization of quartz and of substances under magnetic action. Following out that clue, I obtained the following explanation of Mr. Kerr's experiment, and was enabled, through Professor Barrett's kindness in helping me to verify my recollections of Mr. Kerr's letter, to make sure that my theory explains the facts.

Faraday has shown, in the nineteenth series of his experimental researches, that a ray of plane-polarized light, when transmitted through any solid (diamagnetic?) transparent medium under the action of a powerful magnet, has the plane of its polarization rotated in that direction in which a positive current must circulate round the ray in order to produce a magnetic force in the same direction as that which actually exists in the medium. Verdet, however, discovered that in certain *ferromagnetic* media (as, for instance, a strong solution of perchloride of iron in wood-spirit or ether) the rotation is in the opposite direction to the current which would produce the magnetic force.

Now Fresnel's explanation of the rotatory power of quartz has been applied by Professor Maxwell, in his 'Electricity and Magnetism,' vol. ii. p. 402, to explain the similar, though not identical, phenomenon of magnetic rotation of light. He there, in § 812, gives this explanation in the following words :—" A plane-polarized ray falls on the medium.

This is equivalent to two circularly polarized rays, one right- and the other left-handed (as regards the observer). After passing through the medium the ray is still plane-polarized, but the plane of its polarization is turned, say, to the right (as regards the observer). Hence of the two circularly polarized rays, that which is right-handed must have had its phase accelerated with respect to the other during its passage through the medium. In other words, the right-handed ray has performed a greater number of vibrations, and therefore has a smaller wave-length within the medium than the left-handed ray which has the same periodic time." This is the same as saying that the velocity of the right-handed ray is less within the medium than the left-handed, or that the refractive index for right-handed rays is greater than for left-handed in a medium that rotates light to the right. Hence, from what Verdet has shown, it appears that, in a ferro-magnetic substance, *for a ray of light travelling from the south to the north pole, the magnetic action is such as to make the refractive index for right-handed circularly polarized rays less than for left-handed ones*; for in this case the plane of polarization is turned to the left, for it is a right-handed current that would produce the magnetic force.

By applying this to the case of light reflected from the south pole of a magnet, we get what I believe to be the true explanation of Mr. Kerr's interesting experiment. In like manner, as in the case of a transmitted ray, I consider the incident plane-polarized ray to be the resultant of two circularly polarized ones, one right- and the other left-handed. Now, for the right-handed one, the refractive index at the surface of the south pole of the magnet, being a ferro-magnetic substance, is less than for the left-handed ray. Hence if each of the two circularly polarized rays be supposed to be the resultant of two plane-polarized rays, one polarized in the plane of incidence and the other at right angles to it, the intensities of these four rays being equal, it is evident that the intensities of the pair of reflected rays corresponding to the left-handed ray will be greater than the corresponding intensities of those due to the right-handed ray. Hence the two rays which were polarized perpendicularly to the plane of incidence, and which originally destroyed one another, will, after reflection, have a component in the direction of the vibration of the left-handed ray after reflection. Now, on account of the change of direction of the ray on reflection, this latter is towards the right. This is completely explained in M. Jamin's 'Cours de Physique,' vol. iii. part 2, p. 674, where he shows that a ray the azimuth of whose plane of polarization was originally towards the right is by reflection turned towards the left. Hence the result of reflection is to furnish two rays, one polarized in the plane of incidence, and the other at right angles to it. The phases of these rays will, in general, be different; for they differed by 90° before reflection, and, except at the polarizing angle for iron, this difference of phase would not be completely destroyed, so that

the resultant would generally be an elliptically polarized ray the direction of whose major axis would make a small angle towards the right with the plane of incidence; and at the polarizing angle for iron this ellipse would become a plane-polarized ray whose plane of polarization was turned towards the right, which I understand to be the direction in which Mr. Kerr observed it to be turned—although from some ambiguity as to the meaning of right and left rotations in a ray, arising from not specifying whether it is relative to the direction in which the ray is going or in which it is observed, I am not quite sure whether I understand Mr. Kerr correctly. Also from the fact that there are exceptions* to the rule that rotations are positive for diamagnetic and negative for ferro-magnetic substances, neutral chromate of potash being diamagnetic, yet producing a negative rotation, I should be rather inclined to deduce the direction of the rotation that would be produced, if iron were transparent, from Mr. Kerr's experiment.

It would be quite easy to deduce the difference of the refractive indices of iron for the two circularly polarized rays if we knew the amount by which the plane of polarization is turned; but it would be necessary to employ MacCullagh's or Cauchy's formulæ for the intensities of the reflected rays; and these are so complicated that it is hardly worth while going through the calculations, as the effect Mr. Kerr has observed seems only barely observable.

Similar effects must, of course, occur in the cases of diamagnetic substances, organic solutions, and quartz; but the amounts in these cases would be entirely beyond the range of observation of our present instruments; for in quartz, for instance, the difference of the refractive indices of the two circularly polarized rays is only 0·00008.

Received November 23, 1876.

Observations confirmatory of the foregoing Explanation.

Since sending my explanation of Mr. Kerr's experiment I have made some experiments in confirmation of it. The instruments, with the exception of the electro-magnet, which was kindly lent to me by Mr. Yeates, are the property of Trinity College, Dublin, and were placed at my disposal by Professor Leslie.

The electro-magnet I used is of the horseshoe pattern, with movable soft iron armatures, a face of one of these being well polished. The magnet was placed vertically, and the armatures were arranged on the poles so that the polished face was vertical and a vertical edge of the other armature parallel and very close to this face. A folded piece of paper was inserted at the top between the edge and the face to prevent their being drawn together when the magnet was set in action. Two Nicol's prisms were so placed that a horizontal beam of light traversing

* Unless, indeed, these are due to the nature of the solvent.

one of them was reflected down the other by the polished face from that part of its surface which was opposite the edge.

A beam of sunlight was now transmitted through the apparatus and observed on emerging from the second Nicol. The following results were thus obtained:—When the light was polarized by the first Nicol, either in or perpendicularly to the plane of incidence, and when it had been extinguished by the analyzer, as soon as the electro-magnet was set in action the light immediately reappeared. On now slightly moving the analyzer the light could be partly extinguished; but no motion of the analyzer could make the field as black as it had been before the magnetism was excited, thus conclusively proving that what was produced was an elliptically polarized ray, as I had anticipated. When the light was reflected from a south pole the plane of polarization was rotated to the right of the observer, which is the direction of rotation assumed in my explanation.

I next covered a portion of the polished face with gold leaf, as Professor Barrett had suggested; and now the light reflected from this diamagnetic substance was unaffected by the magnetism, as I had also anticipated. I exhibited all these effects to Mr. Stoney, who entirely confirmed my observations.

Received November 25, 1876.

The angle of incidence in the experiments described above was about 60°. If the incidence were either perpendicular or grazing, the theory which I have proposed would lead to the conclusion that the angle between the major axis of elliptic polarization and the original plane of polarization would vanish. If, accordingly, the observation can be made at a perpendicular incidence, and if the Nicol's prisms be so placed as to extinguish the light before magnetizing the iron, then on exciting it light ought to reappear, as it does at oblique incidences; but the field should not become darker on moving the analyzer.

I attribute great weight to the verification of my theory arising from the fact that the polarization of the reflected ray is found by experiment to be in general elliptic, and also from the fact that there is no appreciable effect when gold, a diamagnetic substance and therefore feeble, is substituted for iron.

Since communicating my paper, I learn, through Professor Stokes, that when Mr. Kerr's paper was read before Section A of the British Association, both he and Sir W. Thomson spoke of the possibility of connecting Mr. Kerr's result with a powerful double refraction of the same kind as the feeble double refraction shown by transparent substances under the influence of magnetism. It is a connexion of this kind which I have endeavoured to demonstrate.

II. "On the Increase in Resistance to the Passage of an Electric Current produced on Wires by Stretching." By Herbert Tomlinson, B.A., Demonstrator of Natural Philosophy, King's College, London. Communicated by Prof. W. G. Adams, F.R.S. Received November 14, 1876.

(Abstract.)

The object of this inquiry was

(1) To determine the relation between increased resistance to the passage of an electric current and stretching force.

(2) To ascertain how much of the increased resistance in each case is produced by mere increase of length and diminution of section of the stretched wire.

In order to determine the increase of resistance from stretching, the wires were each divided into two parts, about 14 ft. in length; one end of each part was fastened to a stout hook firmly fixed into a block of wood. These two hooks were about 8 inches apart, and the block of wood in which they were fixed was securely fastened across two uprights placed resting against a wall of the room, so that the weights, which were attached to the other ends of the wires, might swing clear of the table. The two parts of the wire were joined at the top, about 2 inches below each hook, by a small piece of copper wire, which was securely soldered on to each part of the wire so as to connect them. Towards the lower extremities of the two parts, about 5 inches above the points of attachment of the weights, two copper wires of small resistance were soldered so as to connect the wires with a Wheatstone-bridge arrangement. The increase of resistance was measured by means of a sliding scale of platinum wire divided into millimetre divisions, each equal to ·00166 ohm. As the object was to obtain the temporary, and not the permanent, increase of resistance, which permanent increase was found more or less with all the wires, weights slightly heavier than those intended to be used were first put on and then taken off. Afterwards the wire was balanced as nearly as possible by German-silver wire without the sliding scale, and then very exactly with the sliding scale, which was connected with one of two resistance-coils of 100 ohms each, which formed the other two sides of the bridge. The weights used were then carefully put on to the wires, and the increase of resistance measured by means of the sliding scale; the weights were next taken off again, and the sliding scale used for balancing once more. If there was any slight difference, as sometimes occurred, between the readings of the sliding scale before the weights were put on and after they were taken off, the mean of the two readings was taken. In order to secure still greater accuracy, as many as eight or ten trials were frequently made with each particular weight, and the mean of all the trials taken. In this manner

4 pianoforte steel wires, 1 wire of commercial steel, 3 iron wires, and 4 brass wires were examined with several different weights. The wires taken were of various sections, and it was found that in each case the increase of resistance was "exactly proportional to the stretching force," the stretching not being carried beyond the limit of elasticity of each wire. The resistance of a cubic centimetre of each wire was then determined, also the increase of resistance which a cubic centimetre of each wire would experience when stretched by a force of 1 gramme in the same direction as the passage of the current was calculated from the observations made. The former values varied from

$1574{\cdot}8 \times 10^{-8}$ to $1882{\cdot}4 \times 10^{-8}$ in the case of steel, from
$1200{\cdot}8 \times 10^{-8}$ to $1291{\cdot}0 \times 10^{-8}$ in the case of iron, and from
$656{\cdot}7 \times 10^{-8}$ to $782{\cdot}2 \times 10^{-8}$ in the case of brass;

the latter values varied from

2982×10^{-17} to 3511×10^{-17} in the case of steel, from
2557×10^{-17} to 2712×10^{-17} in the case of iron, and from
1565×10^{-17} to 1843×10^{-17} in the case of brass,

the numbers in each case representing so many ohms.

On dividing the latter values by the former, it was found that the increase per unit of resistance for a stretching force of 1 gramme on a cubic centimetre of each wire was nearly the same for wires of the same material, but differed with wires of different materials. The mean increase per unit of resistance was

for the steel wires $1875{\cdot}5 \times 10^{-12}$,
for the iron „ $2132{\cdot}2 \times 10^{-12}$,
and for the brass „ $2244{\cdot}9 \times 10^{-12}$,

the greatest departure from the mean value being

for the steel less than 2·7 per cent.,
for the iron about 3·0 per cent.,
and for the brass about 8·5 per cent.

The temporary increase of length which a cubic centimetre of each wire would experience on being stretched with a force of 1 gramme was then calculated from observations which had been made in the usual manner with the cathetometer; this increase of length was found to vary

in the case of 3 steel wires from 5082×10^{-13} to 5665×10^{-13},
in the case of the iron wires from 4896×10^{-13} to 5938×10^{-13},
and in the case of 1 brass wire was 10120×10^{-13}.

On dividing the increase per unit of resistance for a stretching force

of 1 gramme on a centimetre of the material by the increase of length produced by the stretching force, so as to obtain the increase per unit of resistance when the wires are stretched 1 centimetre, a mean value of 3·525 was obtained for the steel wires, 3·951 for the iron wires, and 2·203 for the brass wires—thus showing that, though the increase per unit of resistance for a given stretching force is greater in brass than in iron or steel, the increase per unit of resistance for a given lengthening of the wire is much greater both in iron and steel than in brass.

The torsional rigidity of the wires was next ascertained by the method of vibrations, several trials being made with different lengths of each wire; the results for different lengths of the same wire agreed very closely indeed.

From the values of torsional rigidity and the increase of length, the diminution of section was calculated for a cubic centimetre of each wire when stretched with a force of 1 gramme, assuming the wire to be isotropic. Next the increase of resistance which would result from mere lengthening of each wire and diminution of section was determined, and it was ascertained that, on subtracting this latter value from the total observed increase of resistance, there was a considerable residue in the case of the steel and iron wire, also a residue not so great in the brass. This residual increase of resistance probably arises from increased space in the line of flow of the current between the particles of the wire produced by the stretching force.

The conclusions to be drawn from the experiments are :—

1. That the temporary increase per cent. of resistance of a wire when stretched in the same direction as the line of flow of the current is exactly proportional to the stretching force.

2. That the increase per cent. of resistance, when a cube of each material is stretched by the same weight, is greater in iron than in steel wire, and greater in brass than in iron; also that the increase is nearly the same for different specimens of the same material.

3. That the increase per cent., when a cube of each material is stretched to the same extent, is much greater in iron and steel than in brass.

4. That there is a residual increase in each case over and above that which would follow from mere increase of length and diminution of section; that this residual increase is much greater in iron and steel than in brass, and greater in iron than in steel.

III. "Note on the Influence of Liquor Potassæ and an Elevated Temperature on the Origin and Growth of Microphytes." By Wm. Roberts, M.D. Communicated by Prof. Tyndall, F.R.S. Received December 18, 1876.

In a recent communication to the Royal Society, Dr. Bastian * brought forward some experiments to show that while an acid urine usually remains barren after being boiled a few minutes, the same urine becomes fertile when similarly treated if previously neutralized or rendered alkaline by liquor potassæ, especially if it be afterwards maintained at a temperature of 115° F. or 122° F. In this respect urine only conforms to the general rule observed by myself and formulated in my previous communication to the Society †—that "slightly alkaline liquids were always more difficult to sterilize (by heat) than slightly acid liquids."

This difference came out strongest in my own experiments in the case of hay-infusion—the acid infusion invariably remaining barren after a few minutes boiling, and the neutralized infusion invariably becoming fertile after a similar boiling. Accordingly I utilized hay-infusion to determine the cause of the difference in question. It could evidently only be due to one of two things—either (1) the change of reaction enabled germs preexisting in the infusion to survive the ebullition, or (2) the addition of the alkali exercised a positive influence in exciting a *de novo* generation of organisms. To decide which of these two interpretations was the true one, an experiment was contrived in which the liq. potassæ could be added to the infusion not before, but after it had been boiled, and thereby rendered permanently sterile. When added in this way, I found that liquor potassæ had not any power to excite germination. The infusions invariably remained barren when the alkali was added to them after they had been sterilized. I therefore concluded that the effect of the change of reaction consisted simply in enabling preexisting germs to survive a brief ebullition. Dr. Bastian, in repeating this experiment in the case of urine, arrived at an opposite conclusion: he found that whether the alkali was added before or after ebullition he obtained the same result—the urine in both cases became fertile; and he concluded that the alkali had a positive power of promoting the origin of organisms in the urine.

This experiment, if properly performed, is obviously a crucial one, and it is recognized as such by Dr. Bastian. But two conditions are essential to the validity of the experiment. In the first place, it must be ascertained beyond doubt that the boiled acid fluid has been really deprived of its germs—in other words, that the ebullition has been sufficiently prolonged to render it permanently barren; and secondly,

* "Researches illustrative of the Physico-Chemical Theory of Fermentation," &c. See *antè*, p. 149.

† "Studies on Biogenesis," Phil. Trans. vol. clxiv. p. 457.

that in adding the liquor potassæ due care is taken that no new germs are introduced at the same time. In repeating my experiment, Dr. Bastian appears to have departed from my procedure in two points, and he has thus possibly laid himself open to the two sources of fallacy just mentioned. In my own experiments, the acid infusion, after it had been boiled, was set aside in a warm place for a fortnight in order to test its sterility; and the liquor potasse was not added to it until the lapse of time had satisfied me that it had been rendered permanently barren. In Dr. Bastian's experiments the liquor potassæ was added as soon as the vessels had cooled, so that he had no certainty that their contents would not have germinated without the addition of the alkali *. In the second place, instead of heating the tubes containing the liquor potassæ (as I had done) to 250° F., and thus ensuring the destruction of all germs contained in the air imprisoned therein with the alkali, he contented himself with subjecting them for an inconsiderable period to the heat of boiling water.

Seeing these two possible sources of fallacy, I determined to repeat Dr. Bastian's experiments with urine, but taking care to avoid these defects. I proceeded as follows :—

A flask with a longish neck was charged with an ounce of normal acid urine. The due quantity of liquor potassæ requisite to exactly neutralize this (as ascertained by previous trials) was enclosed in a sealed glass tube drawn to a capillary portion at one end. The tube was then heated in oil up to 280° F., and maintained at that temperature for fifteen minutes. The tube was then introduced into the body of the flask. The neck of the flask was next drawn to a narrow orifice; then the urine was boiled for five minutes, and the orifice sealed in ebullition. Ten such flasks were charged and treated in the same manner. They were then set aside in a warm place (from 70° F. to 80° F.) for a fortnight. At the end of this time the contents of the flasks were found perfectly transparent; the urine was therefore assumed to be permanently sterilized. The liquor potassæ was then liberated by shaking the tubes against the sides of the flasks, and thus breaking their capillary points. The previously acid and barren urine was thus neutralized. The flasks were then placed in an incubator, and maintained at a constant temperature of 115° F. At the end of two days it was found that the urine in each flask had deposited a sediment of earthy phosphates; but the supernatant liquid was perfectly transparent. The flasks were again placed in the incubator, and maintained at a constant temperature of 122° F. for three days. At the end of this period they were withdrawn and opened for examination. Not one of them showed the slightest evidence of living organisms; the supernatant liquid was perfectly transparent, and no Microphytes could be detected under the microscope. The pre-

* It is not sufficient to rely in such a case on a control flask or retort. Each flask or retort should have its own individual sterility tested, because it is practically impossible to apply the heat exactly in the same degree in any two cases.

cipitated phosphate in some of the flasks presented a granular appearance, which might, by the unwary, be mistaken for *Micrococci*; but any such illusion was at once dissipated by adding a drop of hydrochloric acid, which instantly dissolved the phosphate and restored the perfect transparency of the urine. This acid has no effect on the turbidity caused by Microphytes.

These experiments therefore negative the conclusion that liquor potassæ, or a temperature of 115° F. to 122° F., or both conditions combined, have the power of exciting the generation of organisms in sterilized urine.

The effect of elevated temperature was also tested in another way. I had by me twenty-nine preparations of fermentible liquids which had remained over from my previous experiments in 1873–74. These consisted of

15 alkalized hay-infusions,
5 pieces of boiled egg-albumen in water,
1 pieces of turnip in water,
2 diluted ascitic fluid,
1 blood with water,
1 albuminous urine,
4 pieces of meat or fish in water.

These had all been sterilized by the heat of boiling water two or three years ago, and were contained in large bulbs with long necks. Ten of the hay-infusions were hermetically sealed; the rest were all open to the air, under the protection of a plug of cotton-wool. All possessed perfectly transparent supernatant liquids, and showed no signs of containing organisms, nor of having undergone any fermentive or putrefactive changes.

These twenty-nine preparations were introduced into the incubator, and maintained at a constant temperature of 115° F. for two days, and then at a temperature of 122° F. for three days. At the end of this period not one of them showed any signs of fertility. The supernatant liquid in each bulb was quite transparent, and some of them which were opened for microscopic examination showed no traces of living organisms.

I can, however, fully confirm the statement of Dr. Bastian, that *Bacteria*, or certain kinds of them, grow and multiply freely in (unsterilized) urine, both acid and neutralized, when exposed to a temperature of 115° F. to 122° F.

IV. "Note on the Deportment of Alkalized Urine." By Professor TYNDALL, F.R.S. Received December 18, 1876.

The communication "On the Influence of Liquor Potassæ and an Elevated Temperature on the Origin and Growth of Microphytes," which, at Dr. Roberts's request, I have had the pleasure of presenting to the Royal Society, causes me to say earlier than I should otherwise have done that the subject which has occupied Dr. Roberts's attention has also occupied mine, and that my results are identical with his.

In some of the experiments the procedure described by Dr. Roberts was accurately pursued, save in one particular which has reference to temperature. Small tubes with their ends finely drawn out were charged with a definite amount of caustic potash, and subjected for a quarter of an hour to a temperature of 220° Fahr. They were then introduced into flasks containing measured quantities of urine. The urine being boiled for five minutes, the flasks were hermetically sealed during ebullition. They were subsequently permitted to remain in a warm place sufficiently long to prove that the urine had been perfectly sterilized by the boiling. The flasks were then rudely shaken, so as to break the capillary ends of the potash-tubes and permit the liquor potassæ to mingle with the acid liquid. The urine thus neutralized was subsequently exposed to a constant temperature of 122° Fahr., which is pronounced by Dr. Bastian to be specially potent as regards the generation of organisms.

I have not found this to be the case; for ten flasks, prepared as above described towards the end of last September, remained perfectly sterile for more than two months. I have no doubt that they would have remained so indefinitely.

Three retorts, moreover, similar to those employed by Dr. Bastian, and provided with potash-tubes, had fresh urine boiled in them on the 29th of September, the retorts being sealed during ebullition. Several days subsequently, the potash-tubes were broken and the urine neutralized. Subjected for more than two months to a temperature of 122° Fahr. they failed to show any signs of life.

These results are quite in accordance with those obtained by Dr. Roberts. His potash-tubes, however, were exposed to a temperature of 280° Fahr., while mine were subjected to a temperature of 220° only.

With regard to the raising of the potash to a temperature higher than that of boiling water, M. Pasteur is in advance both of Dr. Roberts and myself. In a communication to the French Academy, on the 17th of last July, M. Pasteur showed that when due care is taken to add nothing but potash (heated to redness if solid, or to 110° C. if liquid) to sterilized urine, no life is ever developed as a consequence of the alkalization*.

* That alkaline liquids are more difficult to sterilize than acid ones was announced by Pasteur more than fourteen years ago. See 'Annales de Chimie,' 1862, vol. lxiv. p. 62.

M. Pasteur has quite recently favoured me with sketches of the simple but effectual apparatus by means of which he has tested the conclusions of Dr. Bastian. Since his return from his vacation at Arbois, he has carefully gone over this ground with results, he reports to me, not favourable to Dr. Bastian's views.

I may add that I have by no means confined myself to the thirteen samples of urine here referred to. The experiments have already extended to one hundred and five instances, not one of which shows the least countenance to the doctrine of spontaneous generation.

It gives me pleasure to refer to the skill and fidelity with which here, as in other cases, Mr. Cottrell has carried out my directions.

The Society then adjourned over the Christmas Recess, to Thursday, January 11, 1877.

Presents received, December 7, 1876.

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January 11, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Further Observations on the Locomotor System of Medusæ." By George J. Romanes, M.A., F.L.S., &c. Communicated by Prof. Huxley, Sec. R.S., &c. Received November 21, 1876.

(Abstract.)

I. Fundamental Observations.

§ 1. *Fundamental Observations on the Naked-eyed Medusæ.*—The observation which, in my last paper, I designated the "fundamental observation," has been this year repeated numberless times, both on the old species of naked-eyed Medusæ and also on some new ones. The result has been in every case to confirm the rule previously enunciated, namely, that "excision of the extreme margin of a nectocalyx causes immediate, total, and permanent paralysis of the entire organ."

§ 2. *Fundamental Observations on the Covered-eyed Medusæ.*—Regarding the covered-eyed Medusæ, it is necessary to say that I have this year confirmed all the observations which required confirmation from the fact of their having been at variance with those of Dr. Eimer. I have continued to find that the lithocysts are the exclusive seats of spontaneity, so far as the "primary movements" are concerned. I have failed to detect the slightest evidence of spontaneity on the part of the "contractile zones"; for, after allowing the animal to recover from the shock consequent upon removal of its lithocysts alone, and then observing the degree of spontaneity it manifested, I was in no case able to perceive that, by now removing all the "contractile zones," this degree of spontaneity was in the smallest degree diminished. Again, as regards the character of the "secondary contractions" which are manifested by the covered-eyed Medusæ after excision of their lithocysts or margin, I have found all my previous statements substantially correct. I am still unable to say that these secondary contractions appear to me either more "feeble" or "inefficient" than the primary ones, or that the former are any more of a "local" character than the latter. Neither can I modify my previous statements as to the time after removal of the lithocysts during which the secondary movements persist. So far have I found it from being true that these contractions "generally cease after a few moments, or, at any rate, after a few hours," that I am now able fully to

confirm my original statement to the effect that these contractions "usually last for several days." In most cases, indeed, they continue even after decomposition of some parts of the tissues has set in and proceeded to the extent of giving the latter a certain ragged appearance, which is due to certain portions disintegrating into shreds.

II. Rhythm.

§ 1. *Relation between Size and Rhythm.*—I am able to endorse Dr. Eimer's statement that in *Aurelia aurita* the rate of the natural rhythm has a tendency to bear an inverse proportion to the size of the individual. Size, however, is far from being the only factor in determining the difference between the rate of rhythm of different specimens, though what the other factors are I am unable to suggest.

§ 2. *Relation between the periods of Activity and the periods of Repose.*—Dr. Eimer affirms that the duration of the natural pauses, which in *Aurelia* alternate with bouts of swimming, bears a direct proportion to the number and strength of the contractions that occurred in the previous bout of swimming. In my postscript I pointed out that *Sarsiæ* are much better adapted than *Aureliæ* for determining whether any such precise relation obtains, and observed that here I was quite sure no such precise relation did obtain, although in a very general way it was true, as might be expected, that unusually prolonged bouts of swimming were sometimes followed by pauses of unusual duration. These statements have now been amply confirmed.

§ 3. *Effects of Artificial Segmentation on the Rhythm.*—I have also this year investigated Dr. Eimer's rule with regard to the half, quarter, and eighth-part segments of *Aurelia* manifesting respectively a rhythm one half, one quarter, and one eighth part the rate of the rhythm which had previously been manifested by the unmutilated animal. I am glad to observe that Dr. Eimer himself seems to regard this rule as a somewhat uncertain one, for I cannot say that my experiments have tended to confirm it. As stated in my previous paper, there is a general tendency for the smaller segments of an *Aurelia* divided in this way to contract less frequently than the larger segments; and more careful observations this year have not resulted in establishing any more precise relationship. On the other hand, these observations have resulted in confirming my previous opinion, that the potency of the lithocysts which happen to be left in any given segment has more to do with the rate of the rhythm than has the size of the segment; for it is not difficult to obtain an eighth-part segment with a more rapid rhythm than a segment four times that size, provided that the *Aurelia* is divided so as to leave a decidedly prepotent lithocyst to animate the smaller segment. In such cases, however, the endurance of the smaller segment will be less than that of the larger, notwithstanding the greater activity it displays during the first day or two after the operation. I can only attribute this fact to the more

rapid starvation of the smaller segment, owing perhaps to the greater proportional area of the nutrient-tube section in it than in the larger segment. It is interesting, however, to note that although a prepotent lithocyst thus situated shows less endurance than the other lithocysts towards deprivation of nourishment, it shows more endurance towards deprivation of oxygen; for in stale water the prepotent lithocyst will continue active after the others have ceased to discharge, or its action will continue rhythmical after that of the others has become irregular.

§ 4. *Effects of other forms of Mutilation on the Rhythm.*—Mutilation of a covered-eyed Medusa, in which the series of lithocysts is left intact—such as cutting off the manubrium or portions of the umbrella—causes marked changes in the rate of the rhythm. The first effect of the mutilation is usually to accelerate the rhythm; but this temporary acceleration gradually declines, and eventually the rhythm becomes stationary at a rate slower than that which was manifested by the unmutilated animal. Further mutilation will now be attended with a repetition of these effects. To show the degree in which these effects occur, I here quote a series of observations.

An *Aurelia* manifested per minute a regular and sustained rhythm of	26
Immediately after removal of manubrium rhythm rose to	36
Rate then gradually fell for ¼ hour, and became stationary at	20
Circular incision, just including ovaries, caused rhythm to rise to	26
Rate then gradually fell for ¼ hour, and became stationary at	17
Another circular incision, carried midway between the former one and the margin, caused rhythm to rise to	24
Rate again gradually declined, and became stationary at	12
Another circular incision was then carried round as close to the margin as was compatible with leaving the physiological continuity of all the lithocysts intact: rhythm rose to	14
Within a few minutes it fell to	6

Besides producing such marked effects on the rate of the rhythm, mutilation also frequently produces an effect in impairing the regularity of the rhythm. For instance, an *Aurelia* manifested a regular and sustained rhythm of 36 per minute. Immediately after removal of the manubrium, the rate of the rhythm in successive minutes was as follows:—40, 39, 37, 35, 32, 30, 29, 26, 24, 18, 14 (40 seconds' pause), 16, 15, 14, 15, 16 (40 seconds' pause), 22, 20, 19, 15, 16, 17, 14, 13, 13, 15, 16, 17, 18, 14, 12, 13, 11, 12, 9, 15, 16, 14, 12, 9, &c., the rhythm now continuing very irregular. An hour after the operation the following were the numbers of contractions given in one-minute intervals, the observations being taken at intervals of 10 minutes:—15, 15, 12, 22, 14, &c.

These experiments are of interest because tending to show that an

apparently automatic action on the part of ganglia* is really due to a constant stimulation supplied by other parts of the organism.

§ 5. *Effects of lessening the amount of Tissue adhering to a single Ganglion.*—The results of the following very similar experiment would seem to point to a similar interpretation. Excising an eighth-part segment of *Aurelia* containing a single lithocyst, I noted the rhythm manifested by this segment. I then proceeded to pare down the contractile tissue from around the lithocyst, and observed the effect of so doing upon the rhythm. I found that this process had no very marked effect on the rhythm until the paring reached within an inch or two of the ganglion. Then, however, the effect began to show itself, and with every successive paring it became more marked. This effect consisted in slowing the rate of the rhythm, though more especially in giving rise to exceedingly prolonged pauses; but if during one of these pauses a stimulus of any kind were applied to the remainder of the contractile tissue, the rhythmic discharges of the ganglion at once recommenced, and continued for a short time at a slow rate.

§ 6. *Effects of Temperature on the Rhythm.*—The effects of temperature on the rhythm of Medusæ are very decided. For instance, a specimen of *Sarsia* which in successive minutes gave the following numbers of pulsations—16, 26, 0, 0, 26—gave 60 pulsations during the next minute while a spirit-lamp was held under the water in which the Medusa was swimming. Again, if hot water be added to that in which *Sarsiæ* are contained until the whole is milk-warm, the swimming-motions become frantic. If the same experiment be performed after the margins of the *Sarsiæ* have been removed, the paralyzed bells remain quite passive, while the severed margins exhibit the frantic motions just alluded to.

In the case of the covered-eyed Medusæ, the greatest accelerating effect is produced by a more moderate temperature. For example, an *Aurelia* contracted with the greatest regularity 33 times per minute in water kept at 34° Fahr., while in water kept at 49° the rate varied from 37 to 49 per minute. On the other hand, an *Aurelia* whose rhythm in water at 40° was regular at 18 per minute, was suddenly transferred to water at 80°: in the immediately succeeding minutes the rhythm was 22, 20, 14. The latter rate continued for nearly half an hour, when the observation terminated. The effect of moderately warm water (50°–60°), therefore, is to cause permanent quickening and irregularity of the rhythm; while the effect of still warmer water (70°–80°) is temporarily to quicken and then permanently to slow the rhythm, as well, I may add, as greatly to enfeeble the contractions.

The slowing effect on the rhythm of a *diminution* of temperature is decided. For instance, an *Aurelia* presenting a regular rhythm of 20

* Having now satisfied myself concerning the presence of ganglion-cells and nerve-fibres in the marginal bodies of the Medusæ, I feel at liberty to discard the term "locomotor-centres," which in my former paper I everywhere employed to designate these bodies, and to substitute for it the term "ganglia."

per minute in water at 45° was placed in water at 19°: the rhythm almost immediately began to slow and the strength of the contractions to diminish, till the rate fell to 10 per minute (quite regular) and the contractions ceased to penetrate the muscular tissue further than an inch or two from the marginal ganglia; pauses now became frequent, but stimulation always originated a fresh bout of swimming. Next only single contractions were given at long and irregular intervals, and these contractions were so feeble that they were restricted to the immediate vicinity of the lithocyst in which they originated. Soon after this stage irritability disappeared. This process from first to last occupied rather less than five minutes. On now leaving the animal for 10 minutes more, and then transferring it to water at the original temperature of 45°, all the above-mentioned stages were passed through in reverse order.

Some specimens of *Aurelia* were frozen into a solid block of sea-water ice. On being released all their gelatinous tissues were seen to be pierced through in every direction by an innumerable multitude of ice-crystals, which had been formed by the freezing *in situ* of the sea-water which enters so largely into the composition of these tissues. Yet, on being thawed out, the animals recovered, although their original rate of rhythm did not fully return. Their tissues then presented a ragged appearance, which was due to the disintegrating effect produced by the formation of the ice-crystals.

§ 7. *Effects of certain Gases on the Rhythm.*—Oxygen forced under pressure into sea-water containing *Sarsiæ* has the effect of accelerating the rate of their rhythm. The following observation on a single specimen will serve to render this apparent, the numbers of pulsations being recorded in five-minute intervals. The progressive recovery from exhaustion during the last of the three observations deserves notice.

In ordinary water	472, 527, 470.
In oxygenated sea-water..........	800.
In ordinary sea-water	268, 350, 430.

Carbonic acid has the opposite effects to those of oxygen, and, if administered in too large doses, destroys both spontaneity and irritability.

Nitrous oxide at first accelerates the motions of *Sarsia*, but eventually retards them. I omitted, however, to push the experiment to the stage of complete anæsthesia.

Medusæ are very sensitive to such slight carbonization of the water in which they are contained as results from their being confined in a limited body of it for a few hours. The rhythm becomes slowed and the contractions feeble, while the pauses between the swimming-bouts become more and more frequent and prolonged. If the water is not changed, all these symptoms become more marked, and, in addition, the rhythm becomes more irregular. Eventually the swimming-motions entirely cease;

but almost immediately after the animals are restored to fresh sea-water they recover themselves completely.

III. Stimulation.

§ 1. *Mechanical stimulation.*—When the paralyzed swimming-organ of *Aurelia aurita* is stimulated with a single mechanical irritation, it often responds with two, and more rarely with three, contractions, which are separated from one another by an interval of about the same duration as the normal diastole of the unmutilated animal.

§ 2. *Chemical stimulation.*—Dilute spirit, or other irritant, dropped on the paralyzed swimming-organ of *Aurelia aurita*, sometimes gives rise to a whole series of rhythmical pulsations, the systoles and diastoles following one another at about the same rate as is observable in the normal swimming-motions of the unmutilated animal.

§ 3. *Thermal stimulation.*—Response to thermal stimulation may be obtained by allowing a few drops of heated sea-water to run over the excitable surface while the latter is exposed to the air.

§ 4. *Luminous stimulation.*—Light acts as a powerful and unfailing stimulus in the cases of some species of naked-eyed Medusæ. *Sarsiæ*, for instance, almost invariably respond to a single flash by giving one or more contractions. If the animal is vigorous, the effect of a momentary flash thrown upon it during one of the natural pauses is immediately to originate a bout of swimming; but if the animal is non-vigorous, it usually gives only one contraction in response to every flash. That it is light *per se*, and not the sudden transition from darkness to light, which here acts as the stimulus, is proved by the result of the converse experiment,—viz. placing a vigorous specimen in sunlight, waiting till the middle of one of the natural pauses, and then suddenly darkening. In no case did I thus obtain any response: indeed the effect of this converse experiment is rather that of inhibiting contractions; for if the sunlight be suddenly shut off during the occurrence of a swimming-bout, it frequently happens that the quiescent stage immediately sets in. Again, in a general way, it is observable that *Sarsiæ* are more active in the light than they are in the dark: it appears as though light acts towards these animals as a constant stimulus. Nevertheless, when the flashing method of experimentation is employed, it is observable that the stimulating effect of the flashes progressively declines with their repetition. The time during which the deleterious effect of one such stimulus on its successor lasts appears to be about a quarter of a minute. The period of latent stimulation is, judging by the eye, as short in the case of luminous as in that of other stimulation; but when the efficacy of luminous stimulation is being diminished by frequent repetition, the period of latency is very much prolonged. In this case the first effect of the flash is to cause retraction of the tentacles and manubrium, as occurs with other modes of feeble stimulation or stimulation of feeble specimens. Lastly, that the

stimulating influence of light is exerted solely through the sense-organs, is proved by the fact that when these are removed the swimming-bell, though still able to contract spontaneously, no longer responds to luminous stimulation; but if only one marginal body be left *in situ*, or if the severed margin alone be experimented upon, unfailing response to this mode of stimulation may be obtained.

Tiaropsis polydiademata responds to luminous stimulation in the same peculiar manner as it responds to all other kinds of stimulation, viz. by performing the spasmodic movements described in my previous paper. But the period of latency in this species is very much longer in the case of luminous than in that of other modes of stimulation; for while this period is, so far as the eye can judge, quite as instantaneous as it is in the case of *Sarsia* when the stimulus supplied is other than luminous, in response to light the characteristic spasm does not take place till slightly more than a second has elapsed after the first occurrence of the stimulus. Now, as my experiments on *Sarsia* proved that the only respect in which luminous stimulation differs from other modes of stimulation consists in its being exclusively a stimulation of central nervous matter, we have evidence, in the case of *Tiaropsis*, of an enormous difference between the rapidity of response to stimuli by the contractile and by the ganglionic tissues respectively. The next question, therefore, is as to whether the enormous length of time occupied by the process of stimulation in the ganglia is due to any necessity on the part of the latter to accumulate the stimulating influence prior to originating a discharge, or to an immensely lengthened period of latent stimulation manifested by the ganglia under the influence of light. To answer this question, I first allowed a continuous flood of light to fall on the Medusid, and then noted the time at which the responsive spasm first began. This time, as already stated, was slightly more than one second. I next threw in single flashes of light of measured duration, and found that, unless the flash was of slightly more than one second's duration, no response was given. That is to say, the minimal duration of a flash required to produce a responsive spasm was just the same as the time during which a continuous flood of light required to operate in order to produce a similar spasm. From this, therefore, I conclude that the enormously long period of latent excitation in the case of luminous stimuli is not, properly speaking, a period of latent excitation at all, but that it represents the time during which a certain summation of stimulating influence is taking place in the ganglia, which requires somewhat more than a second to accumulate, and which then causes the ganglia to originate an abnormally powerful discharge *.

Responses to luminous stimulation occur in all cases equally well whether the light employed be direct sunlight, diffused daylight, light

* This summation of stimulating influence in central nervous matter (electrical stimuli employed) has recently been described by Dr. Sterling, in the case of reflex action in the frog.—[Jan. 10, 1877.]

reflected from a mirror inclined at the polarizing angle, or any of the separate luminous rays of the spectrum. On the other hand, neither the non-luminous rays beyond the red, nor those beyond the violet, appear to exert the smallest degree of stimulating influence.

§ 5. *Electrical stimulation.* (A) *Latent period, and Characters of the Contractions.*—The period of latent stimulation in the case of *Aurelia aurita* is much longer than it is in the case of *Sarsia.* I have determined it with accuracy in the former case, and find it to be greatly modified by various conditions. To take, therefore, the simplest case first, suppose that the paralyzed *Aurelia* has been left quiet for several minutes in water at 45°, and that it is then stimulated by means of a single induction-shock: the responsive contraction will be comparatively feeble, with a very long period of latency, viz. $\frac{5}{8}$ of a second. If another shock of the same intensity be thrown in as soon as the tissue has relaxed, a somewhat stronger contraction, with a somewhat shorter latent period, will be the result. If the process is again repeated, the response will be still more powerful, with a still shorter period of latency; and so on for perhaps eight or ten stages, when the maximum force of contraction of which the tissue is capable will have been attained, while the period of latency will have been reduced to its minimum—viz. $\frac{3}{8}$ of a second, or, in some cases, slightly less.

The first of these effects is identical with that which has already been described by Dr. Bowditch as occurring, under similar circumstances, in the case of the heart-apex. There are, however, one or two points of difference as regards this summation of stimuli in the case of the heart and in that of the Medusæ; for in the latter, after a "staircase" has been built up by means of a series of stimuli, if a pause of not less than one minute be allowed to elapse and the stimulation be then again commenced, I find that the first step is only of the same height as the first step of a standard staircase. The tissue has, as it were, completely forgotten the occurrence of the previous series of stimuli. Now Dr. Bowditch has found, in the case of the heart, that an interval of five minute must be allowed to intervene between two series of stimuli before the effect of the first on the second series is thus wholly abolished, or, in the words of the metaphor just employed, the memory of the cardiac tissue is about five times as long as that of the medusoid tissue. But in the case of exhausted medusoid tissue the difference may be even greater than this; for in this case I have observed all memory to fade in the course of half a minute. Again, the medusoid tissue is more tolerant than is the cardiac tissue of rapidity in the succession of the stimuli; for while Dr. Bowditch found that the maximum staircase effect could be produced in the latter by throwing in stimuli at about 6-second intervals, I find in the case of the former that the shorter the intervals between the successive shocks, the greater is the staircase effect. And in this connexion I may also state that a staircase has more steps in it if caused by a weak than if caused by a strong current, and that if the

strength of the current be suddenly increased after the maximum level of a staircase has been attained by a feeble current, this level admits of being slightly raised. Lastly, I find in *Aurelia* that the staircase action is so pronounced that a stimulus which at the bottom of a staircase is of less than minimal intensity, is able at the top of a staircase to give rise to a contraction of very nearly maximum intensity. In such cases no response is given to the first three or four stimuli.

With regard to this interesting staircase action, two questions naturally present themselves. In the first place, we are anxious to know whether the arousing effect, which is so conspicuous in a staircase series, is due to the occurrence of the former stimulations or to that of the former contractions; and, in the next place, we should like to know whether, during the natural rhythm of the tissue, each contraction exerts a beneficial influence on its successor, analogous to that which may be so certainly shown to occur in the case of contractions due to artificial stimuli. As regards the first of these questions, it is evident that the fact of invisible steps occurring at the bottom of a staircase (as just described at the close of the last paragraph) proves that the staircase effect, at any rate at its commencement, depends on the process of stimulation as distinguished from that of contraction; for, as in this case the process of contraction does not occur at all, it clearly cannot have any part in the production of the effect. Nevertheless, that the process of contraction does assist in producing the visible steps of the staircase, is perhaps suggested by the result of the following experiment. Having built up a staircase in the ordinary way, I suddenly transferred the electrodes to the opposite side of the disk from that on which they rested while constructing the staircase. On now throwing in another shock at this part of the contractile tissue, so remote from the part previously irritated, the response corresponded in all respects with the one previously given, *i. e.* it was a maximum response. This fact conclusively proves that the staircase effect is a general one, pervading the whole mass of the contractile tissue, and not confined to the immediate seat of irritation*.

The second of the above questions was answered by cutting an *Aurelia* into a spiral strip of small width and great length, and removing all the lithocysts save one. It was then observed that after the occurrence of a natural pause of sufficient duration, the first discharge only penetrated a short way through the strip, the next a little further, the next further still, and so on, till finally the contractile waves passed from end to end. On now removing the ganglion and stimulating with successive induc-

* Since this paper was sent in, Dr. Burdon-Sanderson has published some further and highly interesting observations on *Dionæa* (see latest issue of the Proceedings of the Royal Society). His results, as regards summation of stimuli and several other points, are strikingly similar to my own; but as I was not acquainted with them while writing the text, I can only allude to them in this footnote.—[Jan. 10, 1877.]

tion-shocks, the same progressive penetration was observed as that which had previously taken place with the ganglionic stimulation. From these and other experiments (particularly those in which natural and artificial stimuli were allowed to alternate in the same staircase) there can be no doubt that during the natural rhythm of the Medusæ every contraction exerts a beneficial influence on its successor, which is the same, both in kind and degree, as that which is exerted by a contraction due to an artificial stimulus.

Returning now to the period of latent stimulation in *Aurelia aurita*, we have seen how profoundly this period is modified by the summation of stimuli. We have next to consider the other causes which modify this period. Of these causes the first which claims our attention is exhaustion; for, as it is evident that the effect of exhaustion on the latent period must be in direct antagonism with that of the summation of stimuli, it becomes interesting to observe what will be the total effect on the latent period when these antagonistic influences are both present together. Now tracings show that when this is the case the effect of exhaustion eventually overcomes that of summation, and, further, that the supremacy of the former over the latter shows itself in lengthening the period of latent stimulation before it shows itself in diminishing the amplitude of the contraction.

With regard to the effects of temperature on this latent period, the following table, setting forth the results of one among several experiments, explains itself:—

Temperature of water (Fahr.).	Period of latent stimulation.
°	sec.
70	$\frac{1}{5}$
50	$\frac{1}{3}$
35	$\frac{2}{5}$
20	$\frac{1}{2}$

In the case of each observation several shocks were administered before the latent period was taken, in order to decrease this period to its minimum by the staircase action. When this is not done, the latent period at 20° may be as long as $1\frac{1}{8}$ sec.; but soon after this irritability disappears. Moreover, by cold the duration of the contractions is enormously prolonged.

(B) *Tetanus.*—When *Aurelia aurita*, whether or not paralyzed, is submitted to tolerably strong faradaic stimulation, more or less well pronounced tenanus is the result. That this tetanus is due to summation may be very prettily shown by the following experiment:—An *Aurelia* is cut into a spiral strip, and all its lithocysts are removed. Single induction-shocks are then thrown in at one end of the strip—every shock, of course, giving rise to a contractile wave. If these shocks are thrown in at a somewhat fast rate, two contractile waves may be made at the same time to course, one behind the other, along the spiral strip; but if the shocks

are thrown in at a still faster rate, so as to diminish the distance between any two successive waves, a point soon comes at which every wave overtakes its predecessor, and, if several waves be thus made to coalesce, the whole strip becomes thrown into a state of persistent spasm. In such experiments it is interesting to observe that, no matter how long the strip may be, or how complicated the time-relations between the successive stimuli are made, whatever disturbances are set up at one end of the strip are faithfully transmitted to the other. This of course shows that the rate of transmission is so identical in the case of all the stimuli originated, that the sum of the effects of any series of stimuli is delivered at the distal end of the strip, with all its constituent parts as distinct from one another as they were at starting from the proximal end of the strip.

(C) *Artificial rhythm.*—When the swimming-organ of *Aurelia* has been paralyzed by removal of its lithocysts, and is then subjected to faradaic stimulation of minimal intensity, the response it gives is not tetanic, but strictly rhythmic. The rate of the rhythm varies in different specimens, but usually corresponds with that of rapid swimming. The artificial rhythm may be obtained with a portion of any size of irritable tissue, and whether a small or a large piece of the latter be included between the electrodes.

Progressively intensifying the strength of the faradizing current has the effect of progressively increasing the rate of the artificial rhythm up to the point at which the rhythm begins to pass into tetanus due to summation of the successive contractions. But between the slowest rhythm obtainable by minimal stimulation, and the most rapid rhythm obtainable before the appearance of tetanus, there are numerous degrees of rate to be observed.

The persistency of any given rate of rhythm under the same strength of current is wonderfully great; for it generally requires more than an hour of continuous faradization before the rhythm begins to become irregular, owing to incipient exhaustion. At first only one systole is omitted at long intervals; but afterwards these omissions become frequent and all the contractions irregular. Finally the contractions cease altogether; but a prolonged rest of half an hour or an hour restores the irritability.

The hypothesis by which I explain this artificial rhythm (a rhythm which in most cases is quite as regular as that of a heart) is as follows. Every time the tissue contracts it must, as a consequence, suffer a certain degree of exhaustion, and therefore must become slightly less sensitive to stimulation than it was before. After a time, however, the exhaustion will pass away, and the original degree of sensitiveness will thereupon return. Now the intensity of the faradaic stimulation, which is alone capable of producing rhythmic response, is either minimal or but slightly more than minimal in relation to the sensitiveness of the

tissues when fresh: consequently, when the degree of this sensitiveness is somewhat lowered by temporary exhaustion, the intensity of the stimulation becomes somewhat less than minimal in relation to this lower degree of sensitiveness. The tissue, therefore, fails to perceive the presence of the stimulus, and consequently fails to respond. But so soon as the exhaustion is completely recovered from, so soon will the tissue again perceive the presence of the stimulation; it will therefore again respond, again become temporarily exhausted, again fail to perceive the presence of the stimulation, and therefore again become temporarily quiescent. Now it is obvious that if this process occurs once, it may occur an indefinite number of times; and as the conditions of nutrition, as well as those of stimulation, remain constant, it is manifest that the responses may thus become periodic.

In order to test this hypothesis, I made the following experiments. Having first noted the rate of the rhythm under faradaic stimulation of minimal intensity, without shifting the electrodes or altering the strength of the current, I discarded the faradaic stimulation, and substituted for it single induction-shocks thrown in with a key. I found, as I had hoped, that the maximum number of these single shocks which I could thus throw in in a given time, so as to procure a response to every shock, corresponded exactly with the number of contractions which the tissue had previously given during a similar interval of time when under the influence of the faradaic current of similar intensity. For instance, to take a specific case, it was found that under the faradaic current the rate of the rhythm was one in two seconds. By now throwing in single shocks of the same intensity, it was found that the quickest rate at which these could be thrown in, so as to procure a response to every shock, was one in two seconds. If thrown in at a slightly quicker rate, every now and then, at regular intervals, one of the shocks would fail to elicit a response. The length of these intervals, of course, depended on the rate at which the successive shocks were thrown in; so that, for instance, if they were thrown in at the rate of one a second, the tissue would only, but always, respond to every alternate shock.

The following, and somewhat similar, experiment is still more conclusive. As already stated, the rate of the artificial rhythm under faradaic stimulation varies with the strength of the faradaic current. Well, by choosing at random any strength of faradaic stimulation between the limits where rhythmic response occurred, and by noting the rate of the rhythm under that strength, I was generally able to *predict* the precise number of single induction-shocks I could afterwards afford to throw in with the same strength of current so as to procure a response to every shock—this number, of course, corresponding exactly with the rate of the rhythm previously manifested under the faradaic stimulation.

Other experiments, which do not admit of being briefly detailed, have likewise confirmed the above hypothesis; so I think the latter may be

considered almost as demonstrated. Upon this hypothesis, therefore, I have constructed a theory concerning the rhythmic action of organic tissues in general. The details of this theory cannot be rendered in the present abstract; but in its main outlines it is very simple, viz. that all such rhythmic action is due to the alternate process of exhaustion and recovery of contractile tissues, which has just been explained. Therefore the particular case of rhythmic action of ganglionated tissues is supposed by this theory to be due, not to any special resistance mechanism on the part of the ganglionic tissues, but to the primary qualities of the contractile tissues. In other words, the function of the ganglia is supposed to be merely that of supplying a constant stimulation, the rhythm being supposed due to the same causes as is the artificial rhythm of *Aurelia aurita*. From this it will be seen that the essential point of difference between the current theory of rhythm as due to ganglia and the theory now proposed consists in this—that whereas both theories suppose the accumulation of energy by ganglia to be a continuous process, the resistance theory supposes the discharge of this accumulated energy to be intermittent, while the exhaustion theory supposes it to be constant. According to the former theory, therefore, the rhythm results because the stimulation is periodic; according to the latter theory, the rhythm results because the alternate process of exhaustion and recovery, or the fall and rise of excitability, is periodic.

Without here waiting to discuss the *à priori* merits of these rival theories, I will proceed at once to mention some further experiments which were designed to test the new theory, and which have so far confirmed it as to show that the causes which modify the natural rhythm of *Aurelia* likewise modify, in the same ways and degrees, the artificial rhythm.

(*a*) Other modes of constant stimulation besides that supplied by faradaic electricity likewise cause rhythmic action on the part of the deganglionated tissues of Medusæ. As stated in my former paper, the voltaic current causes this action*; and, as stated in an earlier part of this abstract, dilute chemical stimuli, and even mechanical irritation, tend to produce the same effect. Again, the remarkably sustained rhythmic motion which is manifested by the paralyzed bells of *Sarsia* in acidulated water, which last year appeared so anomalous, is clearly to be referred to the same category.

(*b*) As regards the effect of temperature on the rate of the artificial rhythm, it is only necessary to state the following details. With each increment of temperature the rate of the artificial rhythm increases suddenly, just as it does in the case of the natural rhythm. Moreover, there seems to be a sort of rough correspondence between the amount of influence that any given degree of temperature exerts on the rate of the natural and of the

* Thus far the results are strikingly similar to those obtained by Dr. Foster in the case of the apex of the heart.

artificial rhythm respectively. Further, it will be remembered that in warm water the natural rhythm, besides being quicker, is not so regular as it is in cold water: thus also it is with the artificial rhythm. Lastly, water below 20° or above 85° suspends the natural rhythm; and the artificial rhythm is suspended at about the same degrees.

(*c*) Oxygen accelerates, while carbonic acid retards and eventually suspends the artificial rhythm, in just the same way as these gases act on the natural rhythm.

(*d*) When the marginal ganglia of *Sarsia* are removed, the manubrium shortly afterwards relaxes to five or six times its normal length. There can be no doubt that this effect is due to the muscular fibres of the manubrium having been previously kept in a state of tonic contraction by means of a continuous ganglionic discharge from the margin. Now physiologists are unanimous in regarding muscular tonus as a kind of gentle tetanus due to a persistent ganglionic stimulation, and against this opinion nothing can be said. But, in accordance with the accepted theory of ganglionic action, physiologists further suppose that the only reason why some muscles are thrown into a state of tonus by ganglionic stimulation, while other muscles are thrown into a state of rhythmic action by the same means, is because the resistance to the passage of the stimulation from the ganglion to the muscle is less in the former than in the latter case. On the other hand, the new theory of ganglionic action explains the difference by supposing a different degree of irritability on the part of the muscles in the two cases; for it will be remembered that in my experiments on paralyzed *Aurelia*, if the continuous stimulation were of somewhat more than minimal intensity, tetanus was the result, while if such stimulation were but of minimal intensity, the result was rhythmic action. Now I find in the case of *Sarsia* that the muscular tissue of the manubrium *is* more excitable than the muscular tissue of the bell; so that, for this and other reasons, the facts here accord more closely with the exhaustion than with the resistance theory of ganglionic action. But, in conclusion, I should like to say that as yet I regard the former theory as of a merely provisional character, and that I have published it thus prematurely in order that if, as my experiments strongly suggest, it is the true theory of rhythm, other physiologists may be able to test it on rhythmically contracting tissues in general.

IV. Section.

§ 1. (A) *Reflex action.*—The occurrence of reflex action in the Medusæ is of a very marked and unmistakable character. For instance, if the manubrium of *Sarsia* or of *Aurelia* be irritated, the swimming-organ responds to the irritation by giving one or more contractions; if the marginal ganglia be now removed, the swimming-organ no longer responds even to the most violent irritation of the manubrium. Again, in *Aurelia*, if only one lithocyst be left *in situ*, and if, during a pause in the

activity of the latter, any part of the irritable surface of the swimming-organ be very gently touched, the resulting contractile wave does not start from the immediate seat of irritation, but from the ganglion which still remains *in situ*.

(B) *Nervous connexions in* Sarsia.—When one of the four tentacles of *Sarsia* is very gently irritated, it alone contracts. If the irritation be slightly stronger, all the four tentacles, and likewise the manubrium, contract. If one of the four tentacles be irritated still more strongly, the bell responds with one or more contractions. In the latter case, if the specimen operated on be non-vigorous or partly anæsthesiated, it may be observed that a short interval elapses between the response of the tentacles and that of the bell. Lastly, the manubrium is much more sensitive to a stimulus applied to a tentacle, or to one of the marginal bodies, than it is to a stimulus applied at any other part of the bell.

These facts clearly point to the inference that nervous connexions unite the tentacles with one another and also with the manubrium. This inference agrees with the histological observations of Hæckel on *Geryonia*, and with those of Schultz on *Sarsia*, as well as with the results of my own explorations by stimuli already published. But, to place the matter beyond doubt, I tried the effect of introducing a minute radial cut between each pair of adjacent marginal bodies. This operation, as a rule, completely destroyed the physiological connexions between the tentacles.

But between the tentacles and the manubrium no such definite nervous tracts can be demonstrated by section—that is to say, severance of all the four nutrient tubes, in addition to the marginal incisions just mentioned, has no effect in destroying the physiological connexions between the tentacles and manubrium. The nervous tracts in this case, therefore, appear to be more or less diffused in the form of a plexus over the surface of the bell; and that this is the case is further indicated by explorations with graduated stimuli: for during such explorations it may very frequently be observed that the manubrium is more sensitive than the bell to stimuli applied to the latter; and in such cases there is evidence of the manubrium being more sensitive to irritation of certain tracts of bell-tissue than to that of other tracts. These excitable tracts, however, are not constant as to their position in different individuals.

(C) *Character of the Excitable Tissues of* Sarsia.—In my former paper I employed the term "physiological continuity" to designate such a condition on the part of contractile tissues as admits of an uninterrupted passage along their substance of what I called "contractile waves." I must now introduce another term, viz. "physiological harmony," by which I mean such a condition of contractile tissues as admits of one part responding to stimuli applied at another part, whether or not contractile waves are able to pass along the intervening parts. It will be observed that the distinction between these terms has reference to the

most fundamental quality wherein the function of nerve is distinguished from that of muscle—viz. the power of setting up responsive contractions at a distance from the seat of irritation. In my former paper I described a number of experiments in section, which were devised in order to test the tolerance towards section of physiological continuity: similar experiments, having reference to the tolerance towards section of physiological harmony, are described in the present paper. The experiments just detailed in the foregoing paragraphs were really experiments of this kind; but such experiments derive a special interest when conducted on the general contractile sheet of swimming-organs. They do so, because it will be remembered that the most interesting questions with which my previous paper was concerned were, first as to the presence of a rudimentary nerve-plexus, and next as to the extent in which, if present, it was differentiated from the muscular element.

Now, as stated in my former paper, the contractile tissues of *Sarsia* will endure very severe forms of section without suffering loss of their physiological continuity; but I find that, as a rule, their tolerance is not nearly so great as regards maintenance of their physiological harmony; for in general, though not invariably, the manubrium fails to respond to a stimulus applied to the bell if a cut of a millimetre or two in length intervenes between the base of the manubrium and the seat of irritation.

§ 2. *Character of the Excitable Tissues of* Aurelia.—Widely different is the case of *Aurelia*. As already stated, when a portion of the swimming-organ of this animal is very gently irritated, a contractile wave does not start from the point of irritation; but the passage of a stimulus-wave from that point is proved by the invariable discharge of a ganglion situated at a distance from the latter. It must now be added that the passage of the stimulus-wave admits, in many cases, of being actually seen; for it is a peculiarity of the innumerable tentacles which fringe the margin of *Aurelia*, that they are more irritable than the neuro-muscular sheet of the swimming-organ. Consequently a stimulus which is too gentle to cause the latter to respond even when applied directly to its own substance, nevertheless very often causes a response on the part of the former. When this happens, the tentacles all the way along the margin contract in succession, while the swimming-organ remains perfectly motionless. When this tentacular wave reaches a ganglion, it causes the latter to discharge (after $\frac{1}{2}$ sec. or more for the latent period), and so to give rise to a general contraction of the neuro-muscular sheet. This most beautiful expression of the passage of a wave of stimulation does not occur in all, or even in most, specimens of *Aurelia*; and even in those specimens where it does occur, it may be more readily started by stimulating some tracts of the neuro-muscular sheet than by stimulating other tracts, although there is no constancy as to the position of these tracts in different individuals.

Now the severity of section which is required to cause blocking of these

tentacular waves varies in different cases; but very often the tolerance of tentacular waves towards section is quite as remarkable as is that of contractile waves described in my former paper. Or, adopting our previous terminology, section proves that in *Aurelia aurita* physiological harmony is as difficult to destroy as is physiological continuity. And this fact I regard as the most noteworthy of all the facts to which the present investigation has led—if, indeed, it is not the most startling and significant that has yet been brought to light in the whole range of Invertebrate physiology.

That the fact before us cannot be explained by supposing, with Kleinenberg, that the functions of nerve and muscle are blended in the same cells, would seem to be conclusively shown by the additional facts, that in some cases section blocks the stimulus-wave before it blocks the contractile wave, while in other cases the reverse is true. The only other hypothesis open to us seems to be that of a rudimentary nervous plexus, the constituent elements of which must be supposed capable of vicarious action in almost any degree. This, it will be remembered, was the hypothesis to which I inclined last year as an explanation of all the facts relating to the passage of contractile waves; and the hypothesis is now greatly strengthened by the additional facts just stated with regard to the passage of stimulus-waves: for while the maintenance of physiological continuity is a function of muscle, the maintenance of physiological harmony is a function of nerve. It therefore seems to me that at the stage the inquiry has now reached there is no conceivable alternative between accepting this hypothesis and abandoning the whole subject as inexplicable.

Moreover this hypothesis is supported by the fact, that explorations of the swimming-organ of *Aurelia* with graduated stimuli reveals the presence of excitable tracts pervading the tissues in all directions; but I cannot here enter into particulars. Lastly, it must be stated that we are led up to this hypothesis by degrees. *Sarsia* is the most highly organized of the Medusæ which I have examined; and here I have found that the supposed plexus is so far differentiated, that vicarious action on the part of its constituent elements is usually possible in but a low degree. On the other hand, *Aurelia* is the least highly organized species which I have examined; and here the supposed plexus is so slightly differentiated, that vicarious action on the part of its constituent elements is possible in a very high degree. Lastly, the discophorous species of naked-eyed Medusæ stand midway between *Sarsia* and *Aurelia* in respect of the degree in which integration of their organs has proceeded; and in them the vicarious action of the supposed nerve-plexus also occupies an intermediate position, as we shall see while considering an interesting series of facts to the discussion of which I will now pass.

§ 3. *Character of the Excitable Tissues of* Tiaropsis indicans.—*Tiaropsis indicans* is a bowl-shaped species of naked-eyed Medusa, having a manu-

brium of unusual proportional size. I have given to this animal the specific name of "*indicans*," because of a highly interesting and important peculiarity of function that is manifested by its manubrium. This function consists in the organ localizing, with the utmost precision, any point of irritation which is situated in the bell. For instance, if any point in the irritable surface of the bell be pricked with a needle, the massive manubrium moves over towards that point, and applies its tapered extremity to the exact spot where the prick has been inflicted. Now this apparent reflex action is independent of the only ganglia that can be shown to occur in the organism,—*i. e.* the pointing action of the manubrium is not at all interfered with by removing the margin of the bell. Accordingly I removed the manubrium at its base, and found that by now irritating any part of its own substance, the apex endeavoured to curve down towards the seat of irritation. Similarly, if only a portion of the manubrium were removed, the pointing action of that portion resembled the pointing action of the entire organ, while the stump that remained *in situ* would continue to move over as far as it could towards any point of irritation situated in the bell. Hence there can be no doubt that every part of the manubrium is independently endowed with the capacity of localizing a seat of irritation either in its own substance or in that of the bell. And in this we have a very remarkable fact; for the localizing function which is so very efficiently performed by the manubrium of this Medusa, and which if any thing resembling it occurred in the higher animals would certainly have definite ganglionic centres for its structural correlative, is here shared equally by every part of the exceedingly tenuous contractile tissue that forms the outer surface of the organ. We have thus in this case a general diffusion of ganglionic function, which is coextensive with the contractile tissues of the organ.

The unerring precision with which the manubrium indicates a seat of irritation in the bell may be completely destroyed by introducing a short cut between the base of the manubrium and the seat of irritation in the bell. The afferent connexions, therefore, on which this localizing function depends are thus shown to be exclusively, or almost exclusively, radial. But although under these conditions the manubrium is no longer able to localize the seat of irritation, it nevertheless continues able to perceive, so to speak, that irritation is being applied somewhere; for every time the irritation is applied, the manubrium actively dodges about from one part of the bell to another, applying its extremity now at this place and now at that one, as if searching in vain for the offending body. This fact shows that after physiological harmony of a higher order has been destroyed, physiological harmony of a lower order nevertheless persists; or, to state the case in other words, the fact shows that after severance of the radial connexions between the bell and the manubrium by which the localizing function of the latter is rendered possible, other connexions between these organs remain which are in nowise

radial; or, perhaps still more correctly, that the stimulus escapes from the severed to the unsevered radial connexions through the vicarious action of the latter. I therefore next tested the degree in which these connexions might be cut without causing destruction of that physiological harmony of a lower order which it is their function to maintain. I found that this degree varied considerably in different specimens, but that in no case did the physiological harmony continue after a spiral section had been carried more than once round the circumference of the nectocalyx. In these experiments, moreover, I observed that the tracts occupied by the four radial tubes are tracts of comparatively high irritability as regards the manubrium; for the certainty and vigour with which the random motions of the manubrium occur in response to irritation of the part of a nutrient tube contained in a spiral strip, contrast strongly with the uncertainty and feebleness with which these movements occur in response to irritation of any other part of such a strip. Lastly, when a spiral section is carried only three fourths of the way round the nectocalyx, so as to leave one of the four radial tubes intact, I observed that, on irritating any part of the strip, the manubrium usually pointed to the single radial tube which still remained intact. Now all these facts together, as well as others which cannot be detailed in this abstract, tend strongly in favour of the plexus theory—the radial tubes being supposed here, as in the case of *Sarsia*, to coincide in their course with that of aggregations of nervous elements analogous to nerve-trunks *.

§ 4. *Character of the Excitable Tissues of* Staurophora laciniata.—In my former paper I described certain spasmodic movements which are performed by *Staurophora laciniata.* The remarkable points concerning these movements are, that they never occur except in response to stimulation, and that, in this particular species at any rate, they usually occur only when either the margin of the nectocalyx or one of the four radial tubes are stimulated—stimulation of the general contractile tissue being followed by an ordinary locomotor contraction. Nevertheless stimulation of the general contractile tissue a couple of millimetres from the margin is followed by a local spasmodic contraction; while if the stimulus be applied within a single millimetre of the margin, the effect is a general spasm. Cutting the whole nectocalyx of *Staurophora laciniata* into a spiral strip does not in any degree prevent this spasmodic action; for on irritating the marginal tissue at one end of the strip, a wave of spasmodic contraction passes along the entire strip. Such a spasmodic wave has a much greater power of penetration than has an ordinary contractile wave; for while the latter, in this species, may be very easily blocked by section, the former will continue to pass in spite of the severest forms of section which it is possible to make. Now it

* The plexus theory does not suppose any thing resembling nerve-fibres to be present, but merely tracts of functionally differentiated tissue.

is a remarkable thing that the contractile tissues, although themselves incapable of originating a spasm in response to irritation, are nevertheless so wonderfully capable of conducting a spasm when this has been originated by irritation of the slender tissue tracts above named. It is as though every fibre or cell of the general contractile tissues is able to liberate energy in either of two very different ways; and whenever one part of the general mass is made to liberate its energy in one of these two ways, all the other parts of the mass do the same, and this no matter how far through the mass the liberating process may have to extend. Or, to employ a somewhat far-fetched but convenient metaphor, we may compare the general contractile tissues of this Medusa to a mass of gun-cotton, which responds to ignition (direct stimulation) by burning with a quiet flame, but to detonation (marginal stimulation) with an explosion. Now to say that it is the ganglionic element of the margin or radial tubes which here acts as the detonator, is not to explain the facts. Doubtless it would be an interesting thing to know that a ganglion-cell may be able to originate two very different kinds of impulse according as it liberates its energy spontaneously or in answer to direct stimulation; but this knowledge would merely serve to transfer the questions which now apply to the marginal and radial tube tissues in general to the ganglionic tissues in particular. Again, the supposition of the ganglia acting as detonators when themselves directly irritated, would in nowise tend to explain why it is that the contractile tissues are capable of two such very different kinds of response.

In conclusion, I may state that when a tonic spasm is being slowly recovered from, one may often observe rhythmic locomotor contractions superimposed on the general spasmodic contraction. Again, anæsthetics block spasmodic waves; but not till after they have suspended spontaneity, and even destroyed muscular irritability as regards direct stimulation. Up to this stage the certainty and vigour of the spasm is not perceptibly impaired; but soon after this stage the intensity of the spasm begins to become less, and, later still, it assumes a local character. It is important also to notice that at this stage of anæsthesiation the effect of marginal irritation is very often that of producing a general locomotor contraction, and sometimes a series of two or three such. During recovery in normal sea-water all these phases occur in reverse order.

§ 5. *Rate of transmission of Stimuli in* Aurelia aurita. (A) *Contractile waves.*—The rate at which contractile waves traverse spiral strips of *Aurelia* is variable. It is largely determined by the length and width of the strip. In the unmutilated animal the rate is from 18 to 20 inches per second in water at 40° to 45°. Temperature exerts a wonderful influence on the rate, as the following example will show:—

Temperature of water.	Time occupied in passage of contractile waves through a spiral strip measuring 28 in. in length and $1\frac{1}{2}$ in. in width.
°	secs.
26	4
32	3
42	$2\frac{2}{5}$
65	2
75	$1\frac{3}{5}$
85	Blocked

Interdigitating cuts interposed in the path of the contractile waves slow the rate of the latter considerably; and submitting the uncut contractile tissue to slight strains has the same effect. So likewise with anæsthetics and a great variety of substances which have no anæsthetic property, such, for instance, as strychnine, fresh water, &c. Lastly, it is observable in a long contractile strip that, after a rest of a minute or more, the first wave that traverses it has a very slightly slower rate than its successor, provided the latter follows the former after not too great an interval of time. This fact is probably connected with the summation of stimuli before explained.

(B) *Stimulus-waves.*—The rate of transmission of tentacular waves is only one half that of contractile waves, if the stimulation which starts the former is so weak as not also to start the latter. But if the stimulus is strong enough to start both waves, the tentacular wave always keeps an inch or two in advance of the contractile wave.

V. Coordination.

§ 1. *Covered-eyed Medusæ.*—From the fact that in the covered-eyed Medusæ the passage of a stimulus-wave is not more rapid than that of a contractile wave, we may be prepared to expect that in these animals the action of the locomotor ganglia is not, in any proper sense of the term, a coordinated action; and this I find to be the case. For, as previously stated, it may usually be observed that one or more of the lithocysts are either temporarily or permanently prepotent over the others—*i. e.* that contractile waves emanate from the prepotent lithocysts, and then spread rapidly over the swimming-organ. Nevertheless in many cases such prepotency cannot, even with the greatest care, be observed; but upon every contraction all parts of the swimming-organ seem to contract at the same instant. I am inclined, however, to account for these cases of perfectly synchronous action by supposing that all, or most, of the ganglia require exactly the same time for their nutrition, and are of exactly equal potency in relation to the resistance (or excitability) of the surrounding contractile tissues, and that, therefore, the balance of forces being exactly equal in the case of all, or most, of the ganglia, their rhythm, though perfectly identical, is really independent.

§ 2. *Naked-eyed Medusæ.*—With the naked-eyed Medusæ the case is more definite; for the mere fact of *Sarsia* being able to follow a moving beam of light is in itself sufficient to prove coordination on the part of the locomotor centres *. From my previous observations on the physiological harmony subsisting between the tentacles of *Sarsia*, I was led to expect that the coordination of the locomotor ganglia is probably effected by means of the same tissue tracts through which the intertentacular harmony is effected—viz. those situated in the margin of the bell. Accordingly I introduced four short radial cuts, one midway between each pair of adjacent marginal bodies. The coordination, however, was not perceptibly impaired. I therefore continued the radial cuts, and found that when these reached to one half or two thirds of the way up the sides of the inner bell, or contractile sheet, the coordination became visibly affected, and this for the first time. These experiments, however, did not satisfy me that the coordination was not chiefly, or exclusively, due to the marginal nerves; since, even if the coordination were destroyed by the short radial cuts, it might still appear to remain intact for the following reasons. Supposing the four quadrants to have their physiological harmony destroyed, and one of the quadrants to be slightly prepotent over the others, every time this quadrant of the margin discharged, the other quadrants would immediately do the same †; and as the bell of *Sarsia* is so small, and the passage of contractile waves in it so rapid, the mere presence of physiological continuity might, in the case where only very short radial cuts were introduced, give rise to the false appearance of coordinated or harmonious action. Accordingly I tried the converse experiment of leaving the margin intact, and making four radial incisions from the apex towards the base of the cone. I found that these incisions might be carried quite down to the marginal canal without the synchronous action of the four quadrants being impaired. This proves that the marginal connexions are alone sufficient to maintain the coordinated action of the ganglia, and thus tends to substantiate the above view concerning the results of the converse experiment.

This view is still further confirmed by the results of the same experiment in the case of the Discophorous species of naked-eyed Medusæ. In these species the passage of contractile waves is not nearly so rapid as it is in the case of *Sarsia*; and so in them it may be quite easily observed that the four short radial cuts in the margin have the effect of destroying the physiological harmony of the marginal ganglia ‡. Great

* Removing the manubrium does not interfere with this steering action; but if any considerable portion of the margin be excised, the animal seems no longer able to find the beam of light.

† Allusion is here made to the fact that when a contractile wave reaches a ganglion it causes the latter to discharge. See "Croonian Lecture," Phil. Trans. 1876, p. 311.

‡ This operation, although so slight, has a very remarkable effect in enfeebling the animal—a vigorous specimen being usually reduced by it, after a short time, to absolute quiescence.

nervous shock (such as that caused by a sudden jar on an anvil or violent shaking in a bottle of sea-water) often has the effect of destroying coordination for some time after spontaneity returns.

VI. Poisons.

As this abstract is already too long, I will here only enumerate the poisons I have tried, without entering into the details of their action. I may say, however, in general terms, that in almost every minute particular the effects of the various poisons I have hitherto tried are precisely identical in the case of the Medusæ and in that of the higher animals. In my paper the effects of each of the following poisons are treated at length, viz.—chloroform, nitrite of amyl, caffein, strychnia, veratrium, digitalin, atropia, nicotin, alcohol, and cyanide of potassium. The details of this part of the inquiry are rendered particularly valuable from the fact that, in the case of *Sarsia*, we have the means of testing the comparative influence of any poison on the central, peripheral, and muscular systems respectively; but it is needless on the present occasion to occupy space with a description of the methods—it being enough to say that the effects of the various poisons on these respective systems are uniformly such as occur in the case of the higher animals. In one important particular, however, the actions of nearly all the above poisons on the Medusæ differ from their actions on the higher animals; for there is no poison in the above list which has the property, when applied to the Medusæ, of destroying life till long after it has destroyed all the signs of irritability. I think this anomaly is to be explained by two considerations. First, the Medusæ present to the action of the central nerve-poisons no nerve-centres which are of vital importance to the organism; and, consequently, such poisons are here at liberty, so to speak, to exert their full influence on all the excitable tissues, without having the course of their action interrupted by premature death of the organism. Second, the method of administering the above-mentioned poisons to the Medusæ was very different from that which we employ when administering them to other animals; for, in the case of the Medusæ, the neuro-muscular tissue is spread out in the form of an exceedingly tenuous sheet, so that when the animal is soaking in the poisoned water, every portion of the excitable tissue is equally exposed to its influence. And that the action of a poison is greatly modified by such a difference in the mode of its administration, has recently been proved by Professor Gamgee, who found that when a frog's muscle was allowed to soak in a solution of vanadium &c. it lost its irritability, while this was not the case when the poison was administered by means of the circulation.

Fresh water acts as a deadly poison to the Medusæ. The naked-eyed species usually cease their movements the instant they touch the fresh water, and are killed by it, not, indeed, instantaneously as Agassiz supposed, but in the course of a few minutes. The covered-eyed species

do not succumb quite so rapidly. The cause of this deadly influence exerted by fresh water has been found to depend on the absence of the mineral constituents of sea-water, and not, as Agassiz also supposed, on the difference of density between the former and the latter. Chloride of sodium *alone*, dissolved in appropriate amount in fresh water, deprives the latter, to a great extent, of its deleterious influence; but this is not the case with any other substance I have tried. Brine acts as an anæsthetic, and, in respect of depriving the tentacles and manubrium of their muscular tonus, exerts an influence the opposite of that which is exerted by fresh water.

II. "On some Phenomena connected with Vision." By B. THOMPSON LOWNE, F.R.C.S., Arris and Gale Lecturer on Anatomy and Physiology at the Royal College of Surgeons, Lecturer on Physiology at the Middlesex Hospital Medical School. Communicated by Prof. STOKES, Sec. R.S. Received September 25, 1876.

1. On the Physiological Effect of Ruled Surfaces.
2. On the Time required to Produce or Obliterate a Retinal Image.
3. On the Relation of the foregoing Observations to Fechner's law.

1. *On the Physiological Effect of Ruled Surfaces.*

Some months ago it occurred to me that an investigation of the relation of the shades produced by ruled surfaces (as in engravings) with those produced by variations in the intensity of illumination (shadows) would afford useful data in connexion with the physiological action of light upon the retina.

On examining a number of woodcuts and line-engravings I found that there are usually about nine different shades, which may be expressed by the following fractions, which represent the ratio of black to white upon the surface:—

$$\frac{0}{8}, \frac{1}{8}, \frac{2}{8}, \frac{3}{8}, \frac{4}{8}, \frac{5}{8}, \frac{6}{8}, \frac{7}{8}, \frac{8}{8}.$$

Such engravings, when seen at such a distance that every line makes a distinct picture and can be separately perceived, exhibits a sufficiently gradual series of tints or shades passing regularly from white to black. In this case I assume that the intensity of the sensations produced by a given surface is directly as the number of retinal elements stimulated, so long as the nature and intensity of the illumination remain constant.

I next endeavoured to determine the effect of variations in the intensity of the stimulus in the following manner:—

I repeated Lambert's well-known experiment, in which two candles are placed at distances D and D_1 from a screen, an opaque body being interposed so that each candle casts a shadow upon the screen, the light

being so arranged that each illuminates the shadow cast by the other. I, however, added a woodcut, in which I had previously determined the ratio of white and black in the shades by examining it with a microscope and low-power objective. This woodcut was so placed that it was illuminated by the light from both candles; by varying the distances of the candles I produced shadows of the same intensity as the shades in the print, determining the intensity of the shadow by making it fall close to the corresponding shade in the print without overlapping it. When the two appeared continuous, viewed at such a distance that the ruled surface was still a ruled surface to the eye if attentively observed, I found the proportion of the reserved white in the ruled surface varied as the square root of the intensity to which the illumination of a wholly white surface had to be reduced to match the other.

I found, for instance, in my first experiment, that when the candles were placed at the distances of two and four feet respectively, the faintest shadow was somewhat brighter than that portion of the woodcut in which $\frac{1}{8}$ was black and $\frac{7}{8}$ white, and the darker was somewhat darker than a half-black surface.

Considering the light thrown on the screen by the nearer candle to have the value of 100 units, that of the more removed gives 25 units. The total illumination of the screen was, then, 125 units—that of the darker shadow 25 units, and that of the brighter 100. The portions of the screen, therefore, have the following ratios of illumination:—

$$100 \quad : \quad 80 \quad : \quad 20.$$

But the sensations produced are nearly equal to those of a shaded surface having $\frac{10}{10}$ $\frac{9}{10}$ $\frac{4{\cdot}2^*}{10}$ white, or as

$$10 \quad : \quad 9 \quad : \quad 4{\cdot}2,$$

the square roots of the luminous intensities nearly.

In every case I found the apparent illumination, measured by a reference to a ruled surface, varied inversely as the distance of the source of light.

There is no difficulty in obtaining a very close approximation, as the eye easily detects a shadow differing by $\frac{1}{64}$ of the whole light, as has been shown by Lambert, Arago, Helmholtz, and others.

The experiments made by the method indicated are vitiated to a certain extent by the difficulty of guarding against diffused light; but when moderate precautions are taken a black surface differs in no perceptible degree from the shadow cast by making the two shadows overlap, so that the diffused light may, I think, be neglected.

†In order to test further the degree of accuracy of which the ex-

* The decimal is only estimated, not measured.

† The account of these experiments was received December 11.

periments are susceptible, on the afternoon of November 29 I made 24 consecutive experiments in two sets. In each case an assistant moved one of the candles until I told him to stop; he then marked the table with a piece of chalk. After each three experiments I took the measurements; these only differed by about 2 to 4 centimetres when the furthest candle was from 1 to 3 metres distant. I calculated the results after all the experiments were finished, and found, in the first case, the shadow had the value of

$$\frac{86}{100}, \frac{86}{100}, \frac{85}{100}, \frac{84}{100}, \frac{84}{100}, \frac{84}{100}.$$

I was disappointed at this result, as I had had the paper ruled very carefully to represent $\frac{75}{100}$. On examining it afterwards with the microscope I found the ruling had failed to give the proportion it should, as the lines were too narrow, and the engraver had removed the centre of each line with a diamond, so that, as accurately as I could measure it, it represented $\frac{85}{100}$, the mean of my measurements. This measurement is far the largest source of error.

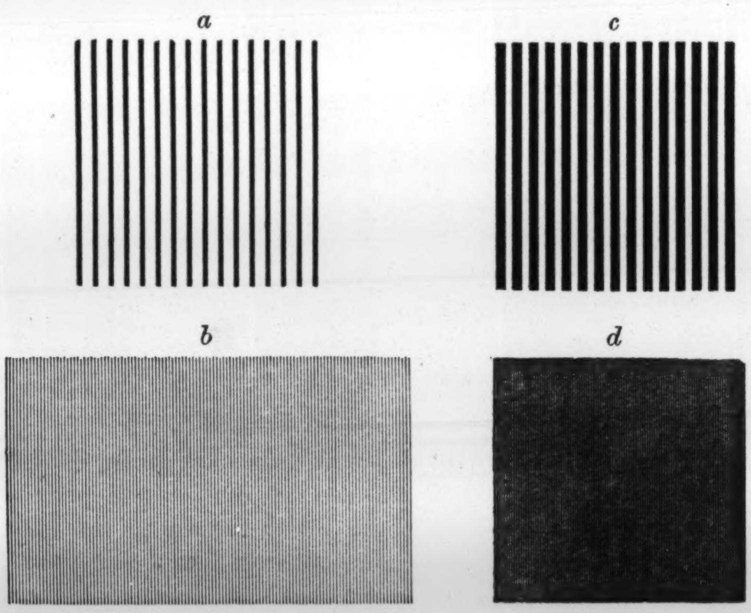
In the second set I obtained $\frac{65}{100}$ and $\frac{61}{100}$ as my results; and again I found the ruling, which should have been $\frac{1}{2}$ black, was only, as nearly as I could measure, $\frac{1}{3}$ black, or $\frac{66}{100}$, and in some parts still less. I think, therefore, I may say the error in different cases, eliminating the difficulty of estimating the ruling, is only about 5 per cent. or less, probably not more than 2 when a mean is taken. With regard to the ruling its estimation is more difficult, and I have hitherto found it impossible to get it done accurately. The ruling-machine does its work well enough, but the printing always spoils it. I think, however, the accuracy is sufficient to establish my law.

In my earlier experiments I assumed that the number of retinal elements stimulated by any given surface vary as the amount of the surface left uncovered by the black lines ruled upon it. Of course this is only the case when an accurate picture is made upon the retina of such a size that the lines and spaces fall on physiologically distinct elements; the lines need not be mentally distinguished, but are, I believe, always capable of being distinguished as lines by a mental act.

Professor Stokes first pointed out to me the necessity of proving that in my experiments such a picture is actually formed, and of investigating the effect when the lines no longer produce a perfect picture, but become diffused so as to give what is physiologically equivalent to a shadow. He pointed out that if I were right, such a surface should be fainter in shade—that is, it should appear brighter than a ruled surface.

I found this a by no means easy question to settle; but I easily convinced myself that a ruled surface seen slightly out of focus, or by an

astigmatic eye appears lighter than when accurately focused; and this appears to be the case in surfaces of considerable extent, so that it could not be due to the formation of a slightly larger picture. A distant newspaper scarcely differs in appearance from a corresponding sheet of white paper, and two similar prints observed at suitable distances give different tints; the further one, when it no longer produces a distinct picture of the individual line, appears lighter in tone. Still I did not feel quite satisfied until I succeeded in having the accompanying diagram ruled for me.



The squares *a* and *b*, *c* and *d* have respectively the same proportion of black upon the surface: *a* and *b* are $\frac{1}{4}$ black, *c* and *d* half black. At suitable distances the following sensations result:—So long as all the lines are distinct there are four distinct shades; *b* and *d* appear darker than *a* and *c* respectively. When the diagram is seen at a distance of from 15 to 20 feet, *c* and *d* become identical in shade and can no longer be separated, but *b* still appears much darker than *a*. At a still greater distance there are but two shades; and these remain distinct so long as the diagram can be distinguished: the illumination is really different; and no distance makes the sensation the same.

2. *On the Time required to Produce or Obliterate an Image on the Retina.*

Much difference of opinion exists on this point, and many contradictory statements have been made by the first authorities. Schafhaütl *

* Münch. Abh. vii. 465.

states that the time which elapses between intermittent luminous impressions without producing discontinuity of sensation varies as the square root of their luminous intensities; but Helmholtz seems to regard the statement as doubtful *.

Herman says that the time required to perceive an impression varies in arithmetical progression when the intensity of the stimulus increases in geometrical progression, but does not give his authority. M. Delbœuf †, making experiments with revolving disks, neglected the rate of rotation, and states that no difference occurs in the results whether it is rapid or comparatively slow.

I made a series of experiments with revolving disks similar to those made by MM. Delbœuf and Plateau. A white card disk, 6 inches in diameter, is set into rapid rotation by clockwork. A portion of a sector of the disk is blackened, so that a grey ring appears during rotation: by reducing the breadth of this sector until the ring was no longer visible, and making the experiment by artificial light, I found that the breadth of the sector at the time of disappearance varies as the distance of the source of light, and that by varying the rate of rotation in the inverse ratio of the distance of the light the ring remains just invisible.

I find that a disk with a portion of a sector, occupying $\frac{1}{200}$ of its circumference, blackened, gives no grey ring with a single candle to illuminate it 10 feet from it when it revolves from 5 to 6 times in a second, but by halving the distance of the candle it must revolve from 10 to 12 times in a second before the ring entirely disappears. A white sector on a black disk obeys the same law, but must occupy only $\frac{1}{1000}$ of the circumference of the disk with the same illumination.

I have concluded that when the grey ring ceases to appear the rotation is sufficiently rapid to cause the sector to occupy the same space for too short a time for it to be seen. With a dull light a white streak on a black surface must occupy the same position for about $\frac{1}{5000}$ of a second to be seen at all; but the time varies inversely as the square root of the illumination. A black spot upon a white ground must rotate much more slowly to be seen. In this case we have to deal with the duration of an exceedingly faint after image—that of the white surface—during the passage of the black spot. The rate of rotation necessary to obliterate the effect of the black spot varies also inversely as the distance of the illuminating source.

3. *On the Relation of the foregoing Observations to Fechner's law.*

A very simple modification of Fechner's convention with regard to sensations and their relation to stimuli will make the foregoing observations accord entirely with his law, and would further change the arbitrary measure of sensation in Fechner's formula into an equivalent measure of

* Helmholtz, Phys. Optique.

† Bulletin Belgique, 1872.

physical nerve change, so that the expression would become a physiological instead of a psychical one.

Fechner regards the liminal intensity of an increment of sensation as an invariable unit, whilst, as is clearly shown, the liminal increment of the stimulus varies as a function of the stimulus already existing. This is an entirely arbitrary convention. If we regard the value of the liminal increment of sensation as a variable depending for its value on the already existing sensation, we may take

$$2K\int\frac{dx}{\sqrt{x}}=K\sqrt{x}=S, \text{ or } \frac{\Delta x}{\sqrt{}}\propto\Delta S,$$

instead of Fechner's expression

$$K\int\frac{dx}{x}=K\log x=S,$$

where x represents the stimulus and S the sensation.

This relation is further borne out by the beautiful experiments of Mr. Dewar and Dr. McKendrick, which show that the electric variation in the natural current of the eye varies as the square root of the intensity of the stimulus; although those authors have attempted to make their results accord with Fechner's formula, they have only done so by the erroneous use of one of M. Delbœuf's constants, which gives a very wide range of arbitrary adjustment.

January 18, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Note on the Changes of the fixed Corpuscles of the Cornea in the Process of Inflammation." By G. F. Dowdeswell, B.A. Communicated by J. Burdon Sanderson, M.D., F.R.S.

[Plate 10.]

Since the discovery by Von Recklinghausen of the immigration of pus-corpuscles into the substance of the cornea in inflammation, several

observers have alleged that proliferation of the fixed corpuscles of that tissue also occurs; in other words, that the so-termed leucocytes are not entirely immigrant, but that some of them are formed in the inflamed tissues. It is stated by those who take this view of the process, that in a few hours after the establishment of inflammation the fixed corpuscles begin to alter, that their processes are partially retracted and thickened, their outline becoming more distinct, and that at a later period small spherical bodies appear in their substance by a process of endogenous cell-formation; that with the progress of inflammation these changes increase, the corpuscles losing their stellate form, and assuming the character of endogenous mother-cells which divide by fission.

These observations have all apparently been made upon corneas excised and examined in serum or other fluid, or upon laminæ of corneas prepared by the gold method.

Cohnheim and others have denied that the changes are objective, and attribute all the appearances to an active immigration of leucocytes. A principal objection to this conclusion has been founded on the grounds that the observations commenced too late, when the asserted changes had already occurred.

In the ordinary methods of preparation, the appearances presented during the second and third day of the inflammation are such as might readily be conceived to arise from proliferation of the elements of the tissue; and it is unquestionably matter of great difficulty to determine with certainty whether this does occur or not. The purpose of the present note is to describe a mode of investigating these appearances, by the employment of which satisfactory evidence may be obtained that the process essentially consists in the penetration of colourless corpuscles (migratory cells), in a state of active cell-division, into the cell-spaces of the cornea, where they overlie and obscure the cornea-cells in such a way that, in preparations made by the usual methods, they appear to be incorporated with them. If, however, a method is employed by which the ground-substance can be destroyed (as, *e.g.*, by potash) and the corpuscles separated by teasing, it is shown that they are perfectly unaltered, the migratory or wander cells only undergoing cell-division; so that it is by the presence of the latter alone that the difference between a normal and an inflamed cornea can be recognized.

The appearances presented by these corpuscles in corneas prepared by the ordinary methods, when supposed to be undergoing proliferation, have been so often described and figured that they need not be further referred to here.

Methods adopted in these experiments.—Inflammation was induced either by touching the surface with a fine point of nitrate of silver in the usual way (in which case the ensuing process arrives at its height in about 48 hours, and then gradually subsides, so that by the fourth or fifth day its effects have disappeared) or (when it was desired that the

process should be of longer duration) by a seton of silk thread. The animal having been killed at the proper period, and its cornea excised and immersed in half per cent. solution of gold chloride for 60 minutes, and exposed in a light warm place, when sufficiently coloured a small portion of the inflamed part is placed in a solution of potash till the ground-substance is completely dissolved. Care is requisite to hit off the exact point for this, a few minutes too much or too little rendering the preparation useless. If the time of exposure is too short, obviously the ground-substance is not wholly dissolved, and nothing is seen; if it is too long, the potash begins to act upon the protoplasm, and the corpuscles cannot be separated nor distinguished. It was found that, if the cornea was put into a 20 per cent. solution of pure potash cold, and then subjected to a temperature of 40° C., 50 minutes was the proper time required for the action of the potash.

When sufficiently acted upon, the cornea is transferred to slightly acidulated water, to stop any further action of the potash, then placed on a slide, if necessary teased out lightly, and preserved in glycerine.

In a preparation made by the above methods a number of migratory cells will be apparent under the microscope, some of which are entangled amongst the processes of the fixed corpuscles. All these are in a state of active cell-division and assuming diversified forms, but are very readily distinguished from the fixed corpuscles by their being devoid of processes, of somewhat darker colour, and by their presenting a more opaque and solid appearance*. The fixed corpuscles, with their processes, show no alteration whatever. As may be seen from the figures (Plate 10. group II.), the processes radiate from the bodies in a natural and symmetrical manner, and the ramifications are as perfect as ever. This would certainly not be so if segmentation had occurred; for in that case one side at least of each newly divided corpuscle would be devoid of processes. Nothing approaching to such a condition is ever seen. In inflamed preparations, as in normal ones, fixed corpuscles are occasionally met with containing two nuclei or a double nucleus; but this appearance is not often seen, and not more frequently in later than in earlier stages.

In conclusion it may be stated that in the whole number of successful preparations of corneas which have been examined (amounting to upwards of twenty), no single instance has occurred in which any distinct appearance of segmentation can be made out. The most careful scrutiny of the preparations fails to detect any difference whatever, as regards their forms or aspects, between the fixed corpuscles of inflamed corneas and those of normal corneas prepared in a similar way; nor would it be possible to distinguish preparations of the two classes from each other, were it not for the presence of migratory cells in the inflamed structure.

In the drawings representations are given both of normal preparations

* This is not adequately shown in the drawings.

(Plate 10. group I.) and of others at the most active period of inflammation (group II.). These have all been most carefully drawn under the camera, each fibre and line being actually copied as they appeared *in situ* in the field of view of the microscope, some processes and other bodies being omitted for the sake of clearness, and the peripheral members of the group sometimes brought nearer to the centre to save space. Numerous other preparations were made at all stages of inflammation, commencing with five hours, when little or no change was observable, up to five and seven days, when, in the case of inflammation induced by nitrate of silver, the effects had passed off, leaving no recognizable traces.

EXPLANATION OF PLATE 10.

Group I.

Corpuscles of the Normal Cornea.

Fig. 1. Two corpuscles, isolated, of a vacuolated appearance; nucleus indistinct, processes much anastomosing.

Fig. 2. Two similar corpuscles.

Fig. 3. A single corpuscle, showing reduplication of nucleus and nucleolus, and what might be taken for segmentation of its substance.

Fig. 4. Two typical normal forms.

Fig. 5. A single corpuscle, with very large and strongly defined nucleus.

Group II.

Corpuscles of a Cornea 48 hours after commencement of inflammation by application of Nitrate of Silver.

Fig. 6. A group of fixed corpuscles (*c*) and wander cells (*w*). The latter are seen to be of diversified form and in an active state of cell-division. The appearances which the fixed corpuscles present are all paralleled in the figures of normal preparations, and, notwithstanding the activity of the inflammation, as evidenced by the state of the wander cells, there is no appearance of proliferation nor of any thing abnormal in these.

Fig. 7. A corpuscle with conspicuous nucleus and two wander cells (*w*) clinging to its processes.

Fig. 8. Two corpuscles, the one vacuolated and with nucleus distinct, the other similar to fig. 3, group I.

II. "Residual Charge of the Leyden Jar.—II. Dielectric Properties of various Glasses." By J. Hopkinson, M.A., D.Sc. Communicated by Prof. Sir William Thomson, F.R.S. Received November 30, 1876.

(Abstract.)

I. The two following propositions are included under the law that the effects of simultaneous electromotive forces are superposable.

(*a*) If two jars be made of the same glass but of different thicknesses, if they be charged to the same potential for equal times, discharged for equal times, and then insulated, the residual charge will after equal times have the same potential in each.

(*b*) Residual charge is proportional to exciting charge.

These propositions are verified experimentally within the limits of errors of observation.

II. Electric displacement through a dielectric may be supposed to depend not only on the electromotive force at the instant, but also in part on the electromotive forces at all previous times. If we assume that the effect of the electromotive force at any previous time decreases according to some law as the time elapsed increases, and that these effects are superposable, we may write

$$y_t = x_t + \int_0^\infty x_{t-\omega} \psi(\omega) d\omega,$$

where x_t is the electromotive force at time t, and y_t is the surface integral of electric displacement divided by the instantaneous capacity of the jar.

If $\psi(\omega)$ is determined for all values of ω, the properties of the glass as regards conduction and residual charge are completely expressed.

$\psi(\infty)$ is equal to the reciprocal of the specific resistance of the material multiplied by 4π and divided by the specific inductive capacity. During insulation y_t is constant; hence

$$x_t = \mathrm{A} - \int_0^\infty x_{t-\omega} \psi(\omega) d\omega.$$

This is the fundamental equation of the following experiments.

Two methods of finding values of $\psi(\omega)$ present themselves.

1st. Let x_t be constant $= \mathrm{X}$ for a time T; insulate for time t.

$$x_t = \mathrm{A} - \int_0^{t+\mathrm{T}} x_{t-\omega} \psi(\omega) d\omega;$$

$$\frac{dx_t}{dt} = -\mathrm{X}\,\psi(t+\mathrm{T}) - \int_0^\infty \frac{dx_{t-\omega}}{dt} \psi\,\omega d\omega;$$

if t be small,

$$\frac{dx_t}{dt} = -\mathrm{X}\psi(\mathrm{T}),$$

and the value of $\frac{dx_t}{dt}$ may be observed with more or less accuracy.

2nd. Let x_t be constant $=$ X for a very long time T previous to time $t=0$; discharge and at time t insulate and observe $\frac{dx_t}{dt}$.

$$x_t = \mathrm{A} - \mathrm{X}\int_t^{\mathrm{T}+t} \psi(\omega) d\omega \,;$$

$$\frac{dx_t}{dt} = \mathrm{X}\left\{\psi(t) - \mathrm{B}\right\}.$$

There are also methods of verification; for example:—Charge during time T′, reverse the charge for time T″ and discharge; then after time t insulate and observe $\frac{dx_t}{dt}$; we shall find

$$\frac{dx_t}{dt} = \mathrm{X}\left\{\psi(t) - 2\psi(\mathrm{T}'' + t) + \psi(\mathrm{T}'' + \mathrm{T}' + t)\right\}.$$

III. Experiments were tried on ten glasses. The verifications were perhaps as close as could be expected, considering that no attempt was made to observe at a constant temperature. The glasses were:—

No. 1. A soda-lime glass containing much soda.
No. 2. A soda-lime glass coloured deep blue with oxide of cobalt.
No. 3. Window-glass.
No. 4. Optical hard crown.
No. 5. Soft crown.
No. 6. A very light flint glass.
No. 7. Light flint.
No. 8. Dense flint.
No. 9. Extra-dense flint.
No. 10. Opal glass.

Glasses 1, 2, and 3 agree in possessing high conductivity and also large values of $\psi t - \mathrm{B}$; whilst 7, 8, 9, 10 have a high resistance (thousands of times as great as 1, 2, or 3) and small residual charge.

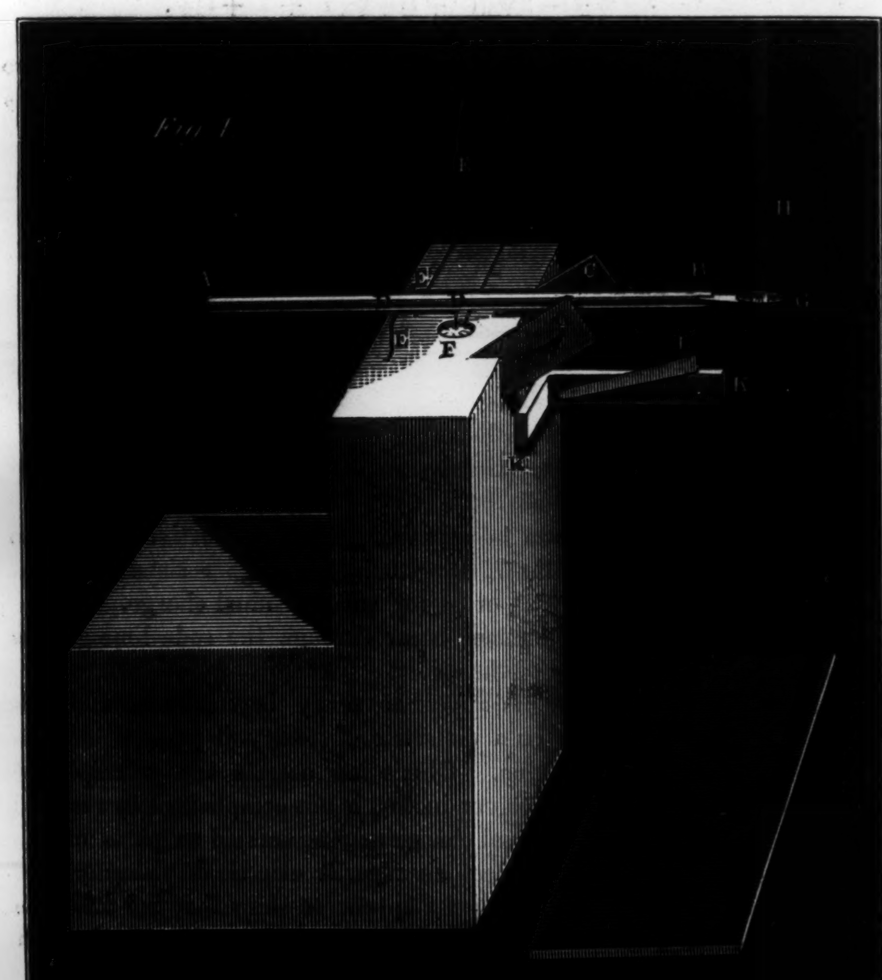
IV. Electrolytic conduction may occur through the soda-lime glasses at the ordinary temperature of the air.

Summary.—The experiments appear to verify the fundamental hypothesis, viz. that the effects on a dielectric of past and present electromotive forces are superposable. Ohm's law asserts the principle of superposition in bodies in which conduction is not complicated by residual charge. Conduction and residual charge may be treated as parts of the same phenomenon, an after effect as regards electric displacement, of electromotive force. The experiments appear to show that the principle of Ohm's law is true of the whole phenomenon of conduction through glass.

III. "A Second Paper on the Forms assumed by Drops of Liquids falling vertically on a Horizontal Plate." By A. M. WORTHINGTON. Communicated by B. STEWART, F.R.S. Received December 28, 1876.

In a previous paper* which I had the honour of submitting to the Society, a method was described by which the later stages of the oscillation had been seen. I wish now to supplement that paper with an account of the earlier stages. The experiments of the sequel were, by the kind permission of Professor Balfour Stewart, made this autumn in the Physical Laboratory at Owens College.

The arrangement that was found most successful for seeing the earlier



* See *ante*, p. 261.

stages was one by which the support was withdrawn from under a drop, so as to leave it free to fall, while at the same instant an electrical circuit was broken.

The apparatus is drawn in the accompanying fig. 1.

A B is a wooden rod about 6 inches long, turning about a horizontal axis C C. When the rod is horizontal, a platinum wire D D, bound to the underside, rests with one end on a platinum wire E E E, forming a bridge, while the other end dips into a mercury-trough F, so that, with the rod in this position, a current can pass from E to F, and so on through the coils of the electromagnet of the relay described in my previous paper.

The end B of the rod bears a little glass cup G made of the central part of a watch-glass, covered with an adherent coating of smoke, obtained by dipping the glass in paraffin-oil before smoking, on to which a drop of water or mercury can be lowered from the vertical tube H, so as to lie in the cup without adhering to it. A smart fillip with the thumb knocks up the end A, and sends down the end B with the cup, thus removing the support of the drop, and, of course, at the same time breaking the contact between D and E. K K is a wooden support bearing a slip of card L, which acts as a spring-catch, and prevents the rod from rebounding. The relay, described in the last paper, is then adjusted, so that the primary spark of the Ruhmkorff's coil is obtained, and the drop seen by it at any stage desired, whether in the air above the plate, at the moment of impact, or later.

A few precautions are necessary.

The cup G must be smoked even with mercury, as that liquid adheres slightly to glass, but not perceptibly to a smoked surface. With water or milk it requires frequent resmoking.

With a very large drop a cup of much deeper concavity must be used, otherwise the drop lies flattened out on the cup; and when the support is removed, efficiency is given to the curvature at the sides, by the tension of which the lower part of the drop is driven downwards into a column, which splits into two or more drops.

I was also able to see the early stages with mercury by fixing the two terminal wires of the electromagnet-current close under the end of a vertical tube, from which the drops were allowed to fall directly on to the plate. The convex end of the liquid, just before the drop fell, joined the two terminals and allowed the current to pass, and when the drop fell contact was broken.

But the time of the spark was not quite so constant as in the method just described, nor was the plan applicable to water, the conductivity of which, even when strongly acidulated, was too slight.

I noticed that, even when the spring of the relay was at its maximum tension, and the point of the relay-wire only just in contact with the mercury, the earliest spark that could be obtained showed that the drop

had already fallen about 20 millims. This explains the failure of the method described in my previous paper to show the earliest stages.

SET 1.

Fig. 1.

Fig. 2.

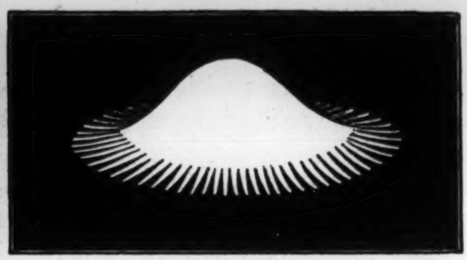


Fig. 3.

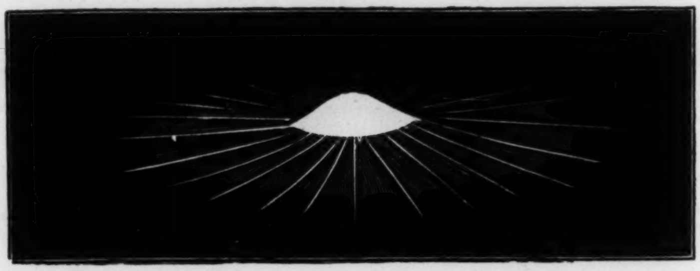


Fig. 4.

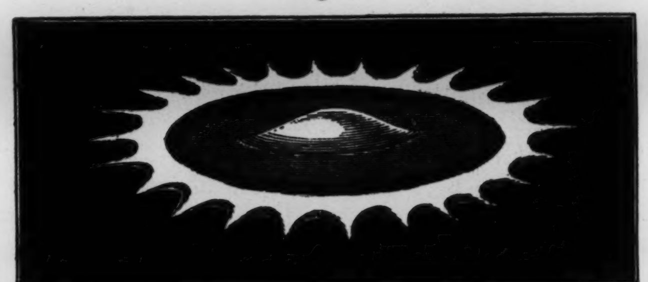


Fig. 5.

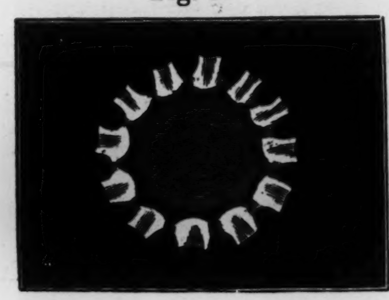


Fig. 6.

Fig. 7.

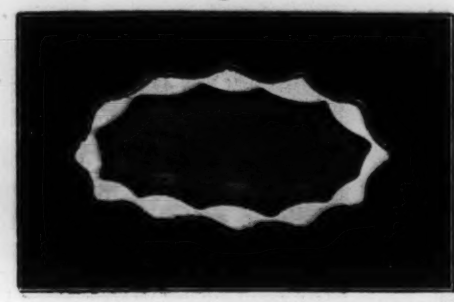
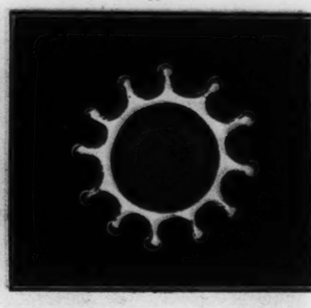


Fig. 8.



Set 1 of the drawings represents the forms most usually assumed by a drop of mercury 3·76 millims. in diameter falling on a clean glass plate from a height of 78 millims.

I will speak of variations afterwards.

Fig. 1. Rays, too numerous to allow of an estimate of their number, shoot out from the point of contact. The inner ends of these join, forming a continuous sheet of liquid, in which the upper part of the drop may be seen reflected.

Fig. 2. The liquid flows from above, over the rays, which shoot out still further.

Fig. 3. Main rays are seen, apparently symmetrically disposed about the centre, and connected by a thinner sheet of either continuous liquid or very fine rays. Very often drops split off from the ends of the main rays, and were left on the plate in a more or less complete circle. The number of these seemed to have been in most cases 24. Often there was no doubt of this, and sometimes there may have been 21 and 28. I have accordingly drawn the figure with 24 rays.

Fig. 4. The rays, having reached their maximum spread, are overtaken by the liquid, which rises in a convex ring and overflows them.

Fig. 5 shows the beginning of a transition from 24 rays to 12 arms. The liquid flows up and joins the rays in pairs.

Fig. 6 shows a later stage of the transition.

Fig. 7 shows the drop with 12 arms just beginning.

Fig. 8. The liquid begins to contract and feed the arms.

The stages later than this were described in my previous paper.

Set 2.

Fig. 1.

Fig. 2.

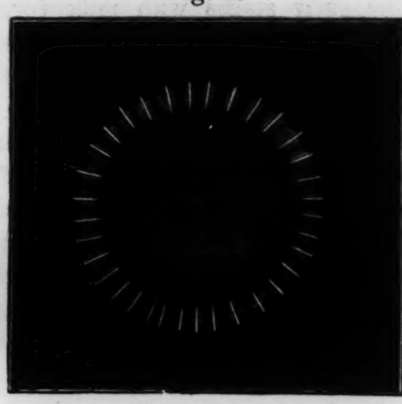


Fig. 3.

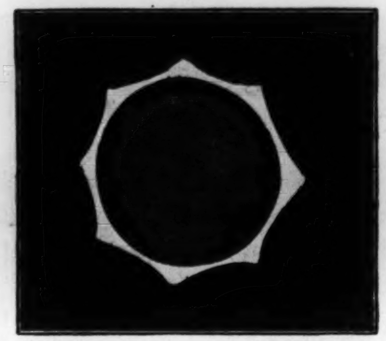
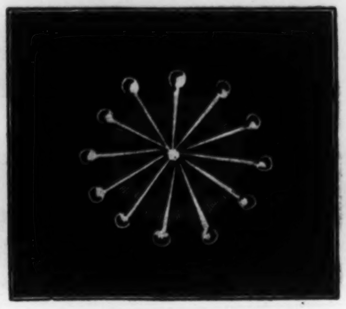


Fig. 4.



SET 2 exhibits variations.

Fig. 1 is a variation of fig. 2, Set 1, the rays being rather fewer and lobed at the ends. This was only seen once, and then no drops split off to be left on the plate.

Fig. 2. This is inserted here as a variation of fig. 4, Set 1, viewed from above, though it was really seen with a greater height of fall (205 millims.). There is a slight convexity at the edge, and the rays are only visible there.

Fig. 3 is a variation of fig. 7, Set 1, where the figure had, as near as could be estimated, 8 lobes.

Fig. 4 is a frequently seen variation of fig. 8, Set 1, the rays being visible to the centre, joined by a thinner film of liquid.

SET 3.

Fig. 1.

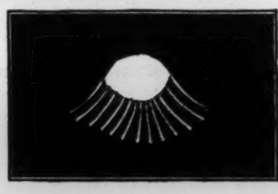
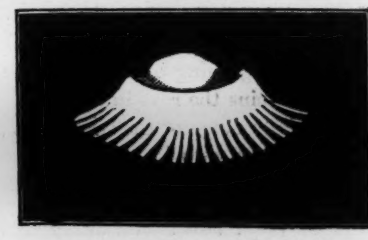


Fig. 2.



SET 3 shows two early stages seen with a drop of milk 4·22 millims. in diameter falling from a height of 30 millims. on to smoked glass.

The early stages with milk (or, what was rather less convenient, water coloured white with finely powdered flint) did not differ markedly from those of mercury.

By increasing the height of fall to 130 millims. the number of rays as indicated by the drops left on the plate was increased to 27, and with a height of fall of 270 millims. the number became 48, or sometimes somewhere about 60, the drops being smaller.

By increasing the size of the drop the number of rays was increased also. Thus with a drop of four times the cubical contents of those mentioned, and falling from a height of 78 millims., the number of drops left on the plate was often 36, and the drops were larger, and, indeed, the whole phenomenon was on a larger scale.

In counting the drops on the plate, judgment is required in distinguishing drops that have split off the rays from those that have split off later from the arms. Often the arrangement on the plate is so confused that no estimate can be made; at other times great regularity of the arrangement gives a probability almost amounting to certainty to the estimate.

The number of arms, as in fig. 8, Set 1, was estimated by judging of the angle between alternate arms. The estimate was sometimes confirmed by drops splitting off the arms and being left on the plate, as in the case of the rays. Thus I am pretty certain that, with a height of fall of 270 millims., the number of arms was often 18.

I may mention that I have obtained "patterns" on thinly smoked glass, made by drops of oil and mercury falling in an approximate vacuum of a pressure of 20 millims. of mercury. The marks thus obtained differ from those made in air, the central spot of lampblack being smaller in the case of air. For low heights of fall the difference is not perceptible, but it becomes very marked as the height is increased from 100 to 500 millims.

With a liquid which wets the surface on which it falls (as, for instance, milk on glass) I find that the earliest stages are very similar to the first two of Set 1; but no well-marked main rays are seen, as in fig. 3. The annular ridge of fig. 4 is seen to overflow the slightly protruding rays, and form a figure like number 7, with slight undulations which do not afterwards increase into arms.

It is, I think, worthy of remark that in the case of mercury on smoked glass, where the adhesion is least, the main ridges appear early before the drop is much spread out. On a clean glass, where the adhesion is greater, the main ridges first appear rather later, and are somewhat less strongly marked; while with milk on a clean glass, to which it adheres strongly, no ridges are seen at all.

IV. "Preliminary Note on the Development of Organisms in Organic Infusions." By JOHN TYNDALL, F.R.S. Received January 18, 1877.

I beg leave to submit to the Royal Society a brief preliminary note of the results obtained in the further prosecution of my researches "On the Optical Deportment of the Atmosphere, with reference to Putrefaction and Infection."

The very remarkable experiments of Dr. Roberts, of Manchester, which have been confirmed by Professor Cohn, of Breslau, have been both verified and contradicted by my recent researches. In some cases alkalized hay-infusions have been completely sterilized by boiling for five minutes, in other cases they have withstood the boiling temperature for a far longer period.

Pursuing with scrupulous exactness the method of experiment devised by Dr. Roberts, and in part followed by Professor Cohn, I have found in other infusions than hay an enormous resistance to sterilization. A single conspicuous example will serve as an illustration. Cucumber-infusion has been subjected, for intervals varying from five minutes to five hours and a half, to the boiling temperature without losing its power of developing life. Two days' exposure to a temperature of 90° Fahr., subsequent to this treatment, sufficed to develop in it swarms of *Bacteria*.

The infusion which thus withstood, in one of Dr. Roberts's "plugged bulbs," the temperature of boiling water for 330 minutes, was completely sterilized in three minutes by boiling it in a small flask with a narrow neck, and hermetically sealing the flask during ebullition. In the case of the "plugged bulbs" the observed resistance was due, not to the germs of the infusion, but to those diffused in the air above it.

I have also pursued my experiments with closed chambers, from which the floating matter was removed by self-subsidence. With certain new infusions introduced into this inquiry failure after failure occurred, two or three days generally sufficing to fill the boiled and protected liquids with Bacterial life. The vegetable infusions usually became turbid throughout; but a characteristic feature of the life developed in all infusions during the last three months was the formation upon their surfaces of a thick and deeply pitted fatty scum. Precautions far greater than those found successful a year ago failed to protect these infusions from contamination.

I resorted to the mode of calcination by an incandescent platinum wire, applied with such uniform success in my last inquiry. The wire was brought close to its point of fusion, the period of incandescence was doubled, and extraordinary care was taken to ward off infection by a ring of cotton-wool. The care proved nugatory; for, in despite of it, swarming life appeared in the infusions afterwards.

I tried to reproduce the results with animal infusions obtained with such ease and certainty a year ago. Some of these old infusions, highly concentrated by evaporation, remain with me to the present hour; they are as clear as distilled water. But in my recent experiments, where the care bestowed far exceeded that found necessary in my last inquiry, the animal infusions, like the vegetable ones, fell, for the most part, into putrefaction.

With hermetically sealed flasks, properly boiled and sealed with due care (I would emphasize this condition), there was no difficulty in sterilizing any of the animal infusions.

By the scrupulous removal of every possible source of contamination I was able finally to maintain some of the most refractory of the liquids operated on perfectly pellucid, in closed chambers from which the floating dust had disappeared by self-subsidence.

It is to be noted that the earliest experiments of this inquiry were

quite in harmony with all the results of the former one, and that it was only as time advanced that the singular discordance between recent and former results showed itself in any marked degree. What was the cause of this discordance?

The question is to be answered by reference to the experiments with hay-infusions, which were begun early and were multiplied and varied later on. By practice such a mastery over these infusions was at length attained that, though the same method of experiment was undeviatingly pursued, I could contradict or corroborate, at will, the observations of Dr. Roberts and Professor Cohn.

On analyzing these apparently irreconcilable results, it was found that, in almost every case where five minutes' boiling sufficed to sterilize alkalized hay-infusion, the hay employed was mown in 1876, while in almost every case where the greater resistance to sterilization was shown, the hay was mown either in 1875 or some previous year. The hay found most difficult to sterilize was from Colchester, and it was five years old.

To the drying and hardening of the germs of the old hay by time I ascribe this singular result.

An experiment on artificially dried peas, as compared with the same peas undried, is not without instruction. After boiling for an hour or so, the undried peas became tasteless, while the dried ones retained a considerable amount of flavour. After a couple of hours' boiling the undried peas rendered the water in which they were immersed thickly turbid, the liquid surrounding the dried peas remaining at the same time perfectly clear. The dried peas were rendered soft, but many of the green peas were reduced by two hours' boiling to a mere pulp, the mixture of which with the water rendered it muddy.

The comparative tastelessness of the undried peas proved that their juices, which are an essential factor of their individuality, and probably also of their power of germination, had diffused into the surrounding water. On the other hand, the clearness of the water which embraced the dried peas indicated a restriction of the exchange of matter between the peas and the medium in which they were immersed. The experiment threw light upon the fact that even with four or five hours' digesting, it was impossible to make the specific gravity of the samples of my oldest hay sensibly greater than that of water. The dryness and induration of the old hay thus indicated being shared by the germs attached to the hay, endowed them, I doubt not, with their greater power of resistance.

Experiments have also been made with new hay dried artificially at temperatures varying from 140° to 185° Fahr., an account of which shall be communicated in due time to the Royal Society.

The different samples of hay employed in this investigation were introduced in succession into the laboratory of the Royal Institution, and they ended by rendering the atmosphere of the place so virulently infective that precautions which, under ordinary circumstances, were more than

sufficient to secure perfect immunity from external contamination, were found utterly ineffectual.

Thanks to the friendly action of the President of the Royal Society, I was enabled to escape from this atmosphere to a purer air. I had a series of tin chambers constructed, which were not permitted to enter the Royal Institution at all, but were taken straight from the tinman to Kew Gardens. They were mounted in the excellent laboratory recently erected there by the munificence of Mr. Jodrell. In this new position the insuperable difficulties encountered in London disappeared, and the experiments followed the course of those described in my last investigation. Two of the chambers gave way; but on being scrutinized they were found leaky. Five sound chambers, on the contrary, remained perfectly intact, and they embraced the particular substances which had given me so much trouble in London. Infusions exposed to the common air at Kew became rapidly rotten.

A fuller account of these researches shall soon be submitted to the Royal Society. In prosecuting them thus far I have been very ably assisted by Mr. Cottrell and his junior colleague Mr. Frank Valter.

January 25, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Description of the Living and Extinct Races of Gigantic Land-Tortoises.—Parts III. and IV. The Races of the Aldabra Group and Mascarene Islands." (Conclusion.) By Dr. ALBERT GÜNTHER, F.R.S. Received November 30, 1876.

(Abstract.)

In continuation of, and concluding, the researches into the history of the Gigantic Land-Tortoises, read before the Royal Society on June 20, 1874, and published in the 165th volume of the Philosophical Transactions, the author treats in Parts III. and IV. of the Tortoises of the Aldabra Group and Mascarenes.

By the addition of the valuable materials obtained by one of the naturalists of the "Transit-of-Venus" Expedition to Rodriguez, and by the Hon. Edward Newton in Mauritius, as well as by the aid of supplementary information received from other sources, the author has been enabled to show in the present parts of his paper that the round-headed

division of Tortoises is confined to Aldabra and never extended to the Mascarenes proper, and that the Tortoises from the latter islands can be externally, though not osteologically, distinguished *as a whole* from the Galapagos Tortoises, as will be seen from the following synopsis:—

I. Nuchal plate absent. Frontal portion of the skull flat. Fourth cervical vertebra biconvex. Pelvis with broad symphysial bridge.

A. Gular plate double; sternum of moderate extent .... GALAPAGOS TORTOISES.

B. Gular plate single; sternum short ...... MASCARENE TORTOISES.

a. Carapace thin, thickened towards the margins; centre of the last vertebral plate raised into a hump, which is separated from the penultimate vertebral by a transverse depression: *Tortoises of Mauritius* (*T. triserrata*, *T. inepta*, *T. indica*, *T. leptocnemis*).

b. The entire carapace extremely thin and fragile, all the bones very slender: *Tortoise of Rodriguez* (*T. Vosmœri*).

II. Nuchal plate present. Frontal portion of the skull convex. Third cervical vertebra biconvex. Pelvis with narrow symphysial bridge. Gular plate double. Carapace thick. ALDABRA TORTOISES (*T. elephantina*, *T. Daudinii*, *T. ponderosa*, *T. hololissa*).

II. "On certain Definite Integrals." By W. H. L. RUSSELL, F.R.S. Received December 5, 1876.

The following paper is a continuation of one recently inserted in the 'Proceedings of the Royal Society.'

$$(13.)\quad \int_0^{\frac{\pi}{2}} d\theta \log_\epsilon (1+2a\cos^2\theta\cos 2\theta + a^2\cos^4\theta) = \pi\log_\epsilon \frac{a+4}{4}.$$

$$(14.)\quad \int^\infty \frac{d\theta\sqrt{1+a\cos\theta+\sqrt{1+2a\cos\theta+a^2}}}{1+\theta^2} = \frac{\pi}{\sqrt{2}}\cdot\frac{\sqrt{\epsilon - a}}{\sqrt{\epsilon}}.$$

Similarly we may find

$$(15.)\quad \int_0^\infty d\theta\sqrt{\cos 2\theta+\mu\cos 3\theta+\sqrt{1+\ 2\mu\cos\theta+\mu}}\log_\epsilon\frac{\theta^2+\mathfrak{c}^2}{\theta^2+a^2}.$$

$$(16.)\quad \int_0^{\frac{\pi}{2}} \frac{\log_\epsilon \cos\theta\cos 2\theta}{1-2x^2\cos 4\theta+x^4} = \frac{\pi}{8x(1-x^2)}\log_\epsilon\frac{1+x}{1-x}.$$

$$(17.)\quad \int_0^\pi \theta d\theta\cdot\frac{\sin\theta+x^2\sin 3\theta}{1-2x^2\cos 4\theta+x^4} = \frac{\pi}{\sqrt{x}}\left\{\log_\epsilon\left(\frac{1+\sqrt{x}}{1-\sqrt{x}}\right)^{\frac{1}{4}}+\frac{1}{2}\tan^{-1}\sqrt{x}\right\}.$$

$$(18.)\ \int_0^{\frac{\pi}{2}} \theta d\theta \frac{\sin\theta\cos\theta}{1+(2a-a^2)\cos^2\theta+a^2(a^2+2a+4)\cos^4\theta}=\frac{\pi}{(a^2+2a)\sqrt{3}}\tan^{-1}\frac{a\sqrt{\ }}{a+}$$

$$(19.)\ \int_0^{\pi} \frac{\epsilon^{x\cos\theta}\big(\epsilon^{x\cos\theta}\sin\theta+\sin(\theta+x\sin\theta)\big)\theta d\theta}{\epsilon^{2x\cos\theta}+2\epsilon^{x\cos\theta}\cos(x\sin\theta)+1}=\frac{\pi}{x}\log_\epsilon\frac{2\epsilon^x}{\epsilon^x+1}.$$

$$(20.)\ \int_0^{\pi} \frac{\epsilon^{x\cos\theta}\big(\epsilon^{x\cos\theta}\sin\theta+\sin(\theta+x\sin\theta)\big)d\theta}{\epsilon^{2x\cos\theta}+2\epsilon^{x\cos\theta}\cos(x\sin\theta)+1}=1.$$

$$(21.)\ \int_0^{\frac{\pi}{2}} \cos\theta d\theta\frac{\cos(\theta+\tan\theta)+a\cos\theta}{1+2a\cos\theta+a^2}=\frac{\pi}{4(\epsilon+a)}.$$

$$(22.)\ \int_0^{\frac{\pi}{2}} \cos\theta d\theta\frac{\cos(\theta+c\tan\theta)+a\cos\theta\cos c\tan\theta}{\sin^2\theta+(1+2a+a^2)\cos^2\theta}=\frac{\pi}{2(a+2)\epsilon^c}.$$

$$(23.)\ \int_0^{\frac{\pi}{2}} \frac{d\theta}{\cos\theta}\cdot\frac{\cos(\theta-\tan\theta)+a\cos\theta\cos\tan\theta}{\sin^2\theta+(1+2a+a^2)\cos^2\theta}=\frac{\pi}{\epsilon^{a+1}}.$$

$$(24.)\ \int_0^{\infty} \frac{dx(\epsilon^{x\cos a}\cos a\sin a+\mu)}{\epsilon^{2x\cos a}+2\mu\epsilon^{x\cos a}\cos a\sin a+\mu^2}=\frac{\cos a}{\mu}\log_\epsilon(1+\mu).$$

$$(25.)\ \int_0^{\infty} \frac{dx\epsilon^{x\cos a}\sin(x\sin a)}{\epsilon^{2x\cos a}+2\mu\epsilon^{x\cos a}\cos a\sin a+\mu^2}=\frac{\sin a}{\mu}\log_\epsilon(1+\mu).$$

$$(26.)\ \int_0^{\infty} \frac{(\epsilon^{2x\cos a}+\epsilon^{x\cos a})\cos(x\sin a)dx}{\epsilon^{4x\cos a}+2\epsilon^{2x\cos a}\cos(2x\sin a)+1}=\frac{\pi}{4}\cos a.$$

$$(27.)\ \int_0^{\infty} \frac{(\epsilon^{3x\cos a}+\epsilon^{x\cos a})\sin(x\sin a)dx}{\epsilon^{4x\cos a}+2\epsilon^{2x\cos a}\cos(2x\sin a)+1}=\frac{\pi}{4}\sin a.$$

$$(28.)\ \int_0^{\pi} \theta d\theta\sin\theta\cos^{n-m-1}\theta\frac{1-\cos^{2n}\theta}{1-\cos^{2n}\theta}=\frac{\pi^2}{2n}\tan\frac{m\pi}{2n}.$$

$$(29.)\ \int_0^{\pi} \theta d\theta\sin\theta\cos^{n-m-1}\theta\frac{1-\cos^{2m}\theta}{1-\cos^{n}\theta}=\frac{\pi}{m}-\frac{\pi^2}{n}\operatorname{cotan}\frac{m\pi}{n}.$$

$$(30.)\ \int_0^{\frac{\pi}{2}} \frac{d\theta\cos^n\theta(\cos n\theta+\mu\cos^n\theta)}{(1+2\mu\cos^n\theta\cos n\theta+\mu^2\cos^{2n}\theta)(a^2\sin^2\theta+b^2\cos^2\theta)}=\frac{\pi}{2ab}\cdot\frac{a^n}{(a+b)^n+\mu}$$

(31.) $$\int_0^\pi d\theta\epsilon^{\sin 2\theta\cos 2\theta}\cos(\sin^2\theta\sin 2\theta)=\frac{\pi}{\sqrt[4]{\epsilon}}.$$

(32.) $$\int_0^\pi d\theta\sin\theta\epsilon^{\sin 2\theta\cos 2\theta}\sin(\sin^2\theta\sin 2\theta+\theta)=\frac{\pi}{2\sqrt[4]{\epsilon}}.$$

(33.) $$\int_0^{\frac{\pi}{2}} d\theta\theta\{\epsilon^{a\sin 2\theta}\sin 2(\theta-a\cos^2\theta)+\epsilon^{-a\sin 2\theta}\sin 2(\theta+a\cos^2\theta)\}=\frac{\pi\sin a}{2a}.$$

(34.) $$\int_0^\omega\frac{(x-a^2x)dx}{\big((1+a+a^2)\sin ax-a\sin 3ax\big)(1+x^2)}=\pi\frac{\epsilon^{2a}+a}{\epsilon^{2a}-a}\cdot\frac{\epsilon^a}{\epsilon^{2a}-1}.$$

(35.) $$\int_0^\omega\frac{(x-a^2x)dx}{\big((1+a+a^2)\cos ax+a\cos 3ax\big)(1+w^2)}=\pi\frac{\epsilon^{2a}-a}{\epsilon^{2a}+a}\cdot\frac{\epsilon^a}{\epsilon^{2a}+1}.$$

(36.) $$\int_0^\pi d\theta\epsilon^{x\cos\theta}\cos(x\sin\theta+\theta)\log_\epsilon(1+\cos\theta)=\frac{\pi(\epsilon^x-1)}{\epsilon^x x}.$$

(37.) $$\int_0^{\frac{\pi}{2}}\frac{d\theta\log(1+2a\cos^n\theta\cos n\theta+a^2\cos^{2n}\theta)}{a^2\sin^2\theta+b^2\cos^2\theta}=\frac{\pi}{ab}\log_\epsilon\frac{(a+b)^n+aa^n}{(a+b)^n}.$$

I observe that in (28) n and m must be odd and even or *vice versâ*, and that in (29) n must be even and m odd.

The integrals here are not solitary examples: each of them may be considered as the representative of a very numerous class. I have selected as examples those which seemed most suitable to be brought before the Royal Society.

P.S.—The following were added 13th February, 1877 :—

(38.) $$\int_0^\pi\theta d\theta\,\epsilon^{x\cos\theta}\sin\left(x\sin\theta+\frac{3\theta}{2}\right)\cos\frac{\theta}{2}=\frac{\pi}{2}\left(\frac{x-1}{x^2}+\frac{1}{\epsilon^x x^2}\right).$$

(39.) $$\int_0^\pi\theta d\theta\frac{\sin 2\theta+(1-x)\sin\theta}{1-2x\cos\theta+x^2}=\pi\left\{\frac{(x+1)\log_\epsilon(1+x)-x}{x^3}\right\}.$$

Presents received, January 11, 1877.

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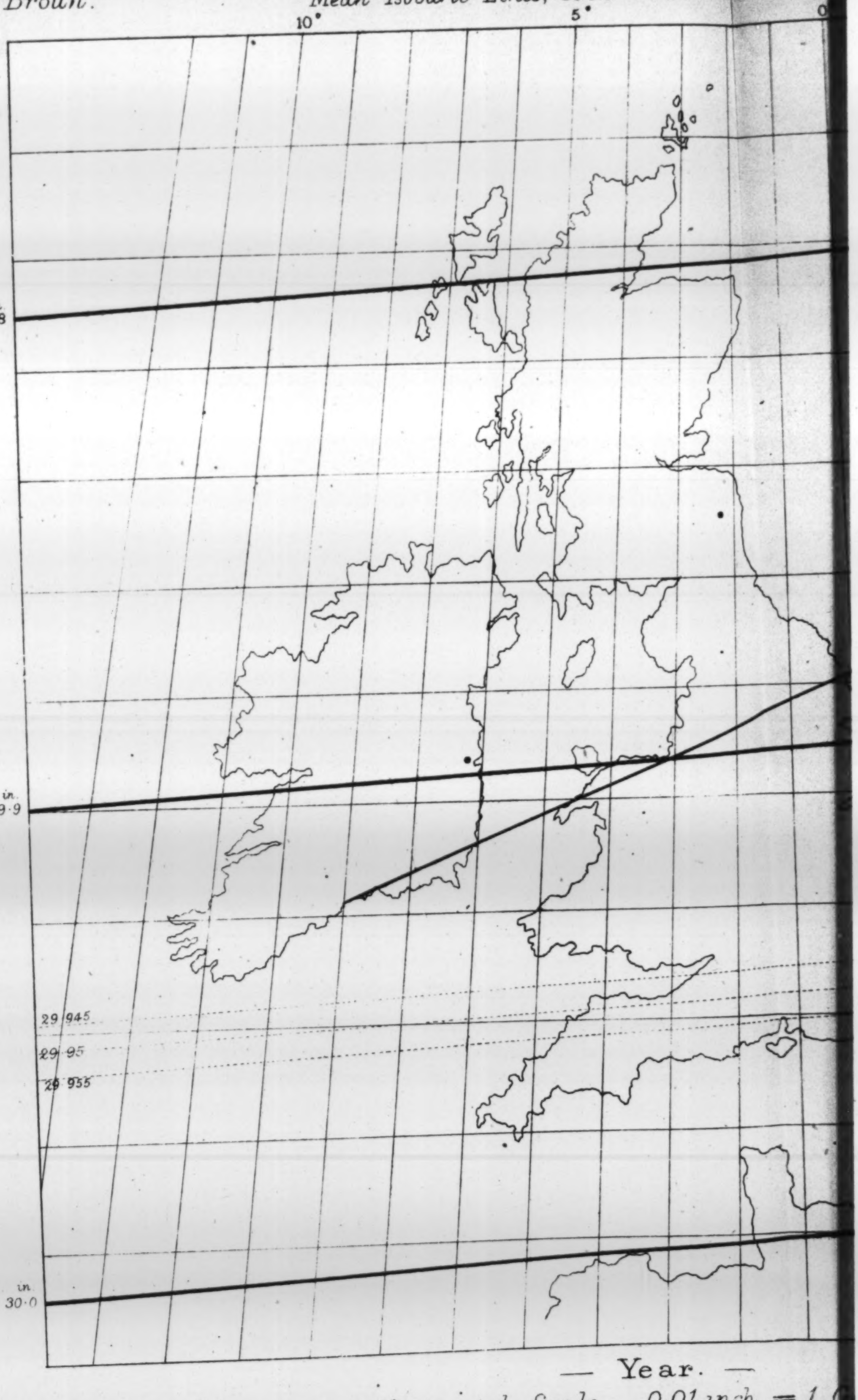
Mean Isobaric Lines, 1842-1849.



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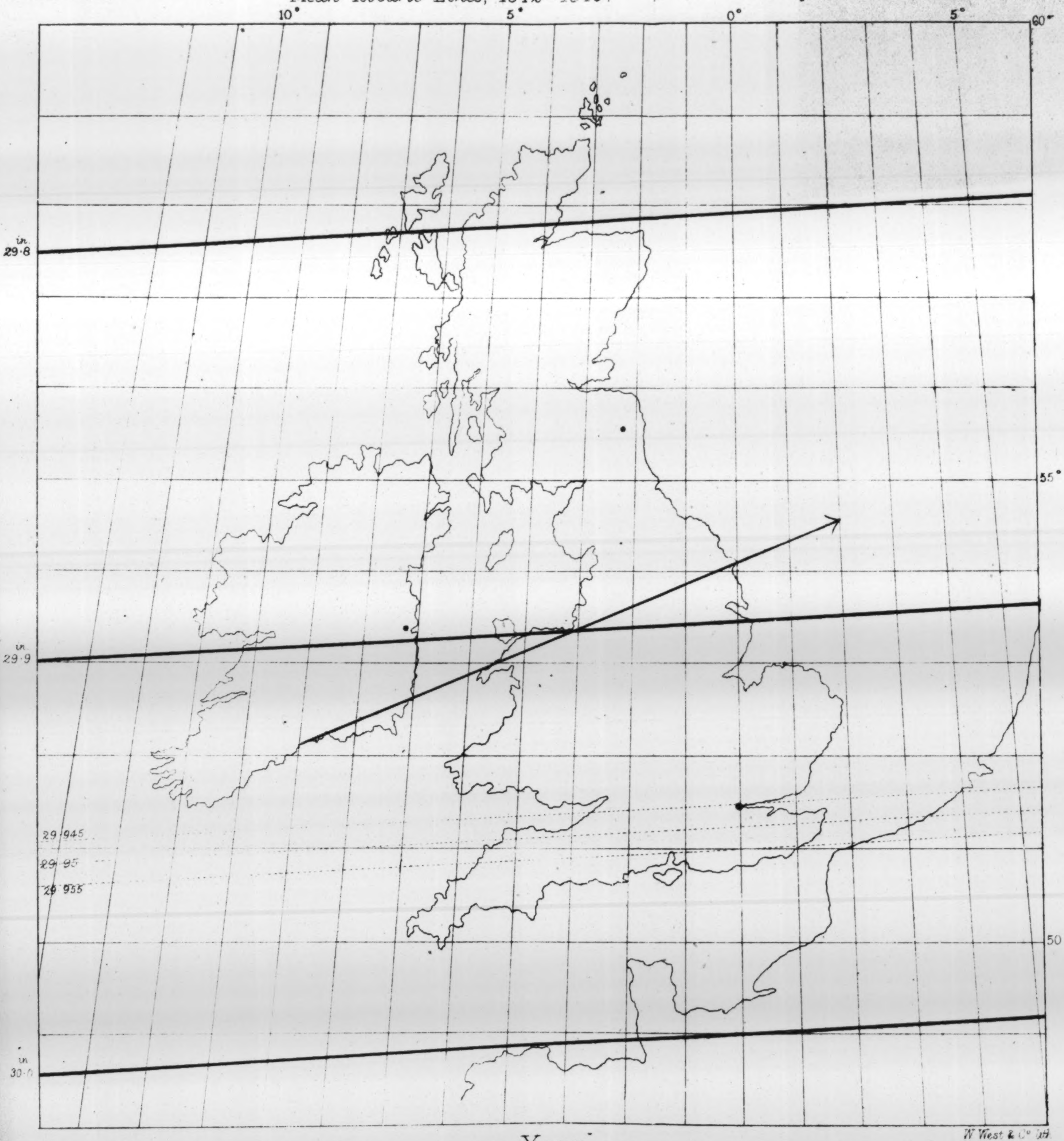
Vertical and Horizontal Scale 0·01 inch = 1 G

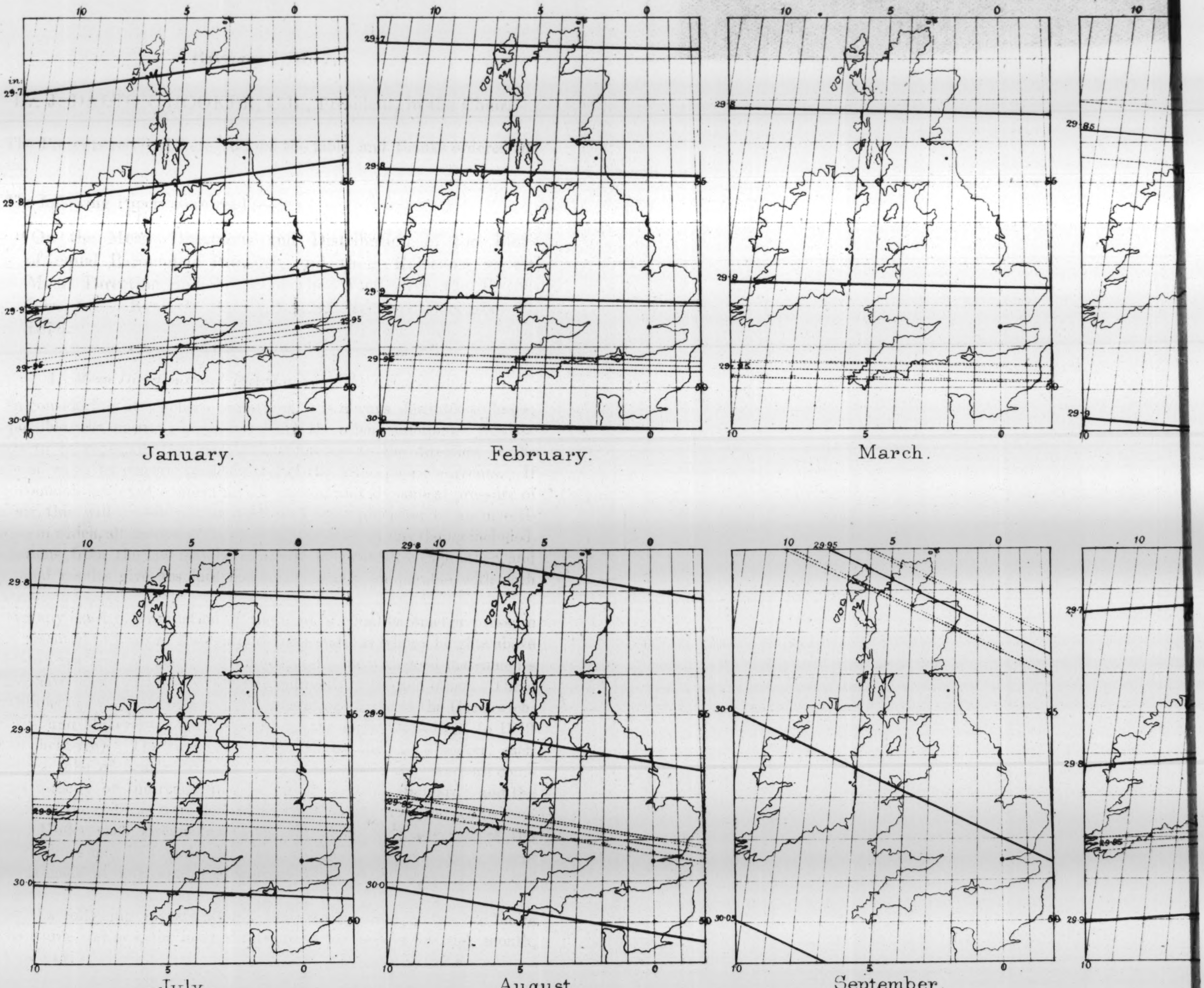
Mean Isobaric Lines, 1842–1849.



— Year. —

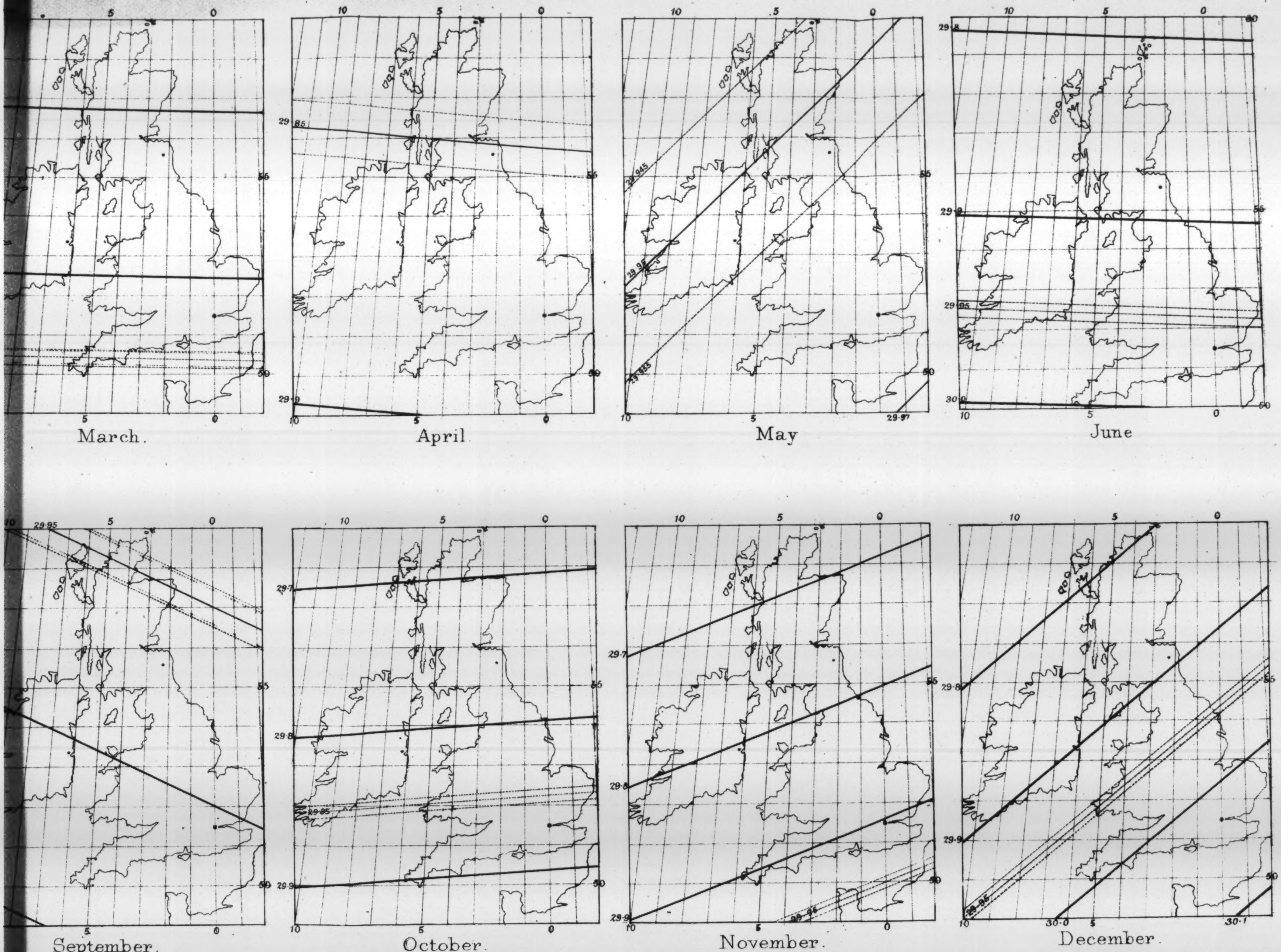
Vertical and Horizontal Scale. 0·01 inch = 1 Geographical mile

Broun. Monthly Mean Isobaric Lines



Vertical and Horizontal Scale: 0·005 inch

Monthly Mean Isobaric Lines, 1842 – 1849.



...cal and Horizontal Scale: 0·005 inch = 1 Geographical mile.

February 1, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

1. "On the Mean Directions and Distribution of the Lines of equal Barometric Pressure, and their Relations to the Mean Direction and Force of the Wind over the British Isles, &c." By J. A. BROUN, F.R.S. Received December 30, 1876.

[PLATES 11 & 12.]

1. *Mean Directions and Distribution of the Isobaric Lines.*

In considering atmospheric variations, it is always desirable to know, if possible, the mean values about which the others fluctuate: this appears to be especially the case with reference to the direction of the lines of mean barometric pressure and of the atmospheric currents. If any common law exist connecting the statical and dynamical pressure of the air, this will probably show itself with some precision by an investigation in which, all the cases (the observations of every day) being included, deviations from the law may be expected to neutralize each other, and the final results give absolute measures directly comparable with each other.

For any exact determination of the lines of equal barometric pressure it is essential to possess observations from stations whose heights above the mean sea-level are accurately known, and made with good instruments which have been compared directly or indirectly with each other. These conditions are well satisfied by the observations made at the Greenwich, Dublin, and Makerstoun Observatories in the eight years 1842 to 1849 (both inclusive). The barometers were all by the same maker, each having a tube of nearly 0·6 inch internal diameter; they were all compared directly or indirectly with the Royal Society's standard; and the heights of the cisterns were determined by levelling from the sea in each case. Under such circumstances the directions and intervals of the isobaric lines may be found with much more precision than from observations made at any number of stations where these conditions are not fulfilled.

In the following investigation I shall assume that, within the limits of the three stations, the mean directions of the *isobars* for each month,

as deduced from the eight years' observations, may be represented approximately by parallel straight lines drawn on a plane chart.

The positions of the three barometers are as follow :—

	Lat.	Long.	Height above *mean* sea-level.
	° ′	° ′	ft.
Greenwich	51 29	0 0	159
Dublin	53 21	6 16 W.	18*
Makerstoun........	55 35	2 16 W.	213

Taking the meridian of Greenwich for the axis of y, and the circle of latitude of Greenwich for that of x, represented by a straight line on the chart perpendicular to y (both on the same plane), we obtain the following equations for the isobars passing through Makerstoun (M) and Dublin (D):—

$$\text{M} \quad . \quad . \quad . \quad y=ax+b_1,$$
$$\text{D} \quad . \quad . \quad . \quad . \quad y=ax+b_2;$$

and if y, x, b_1, and b_2 be expressed in geographical miles, then from the preceding coordinates we have for the points M and D,

$$\text{M} \quad . \quad . \quad . \quad . \quad 246=-\ 77a+b_1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$
$$\text{D} \quad . \quad . \quad . \quad . \quad 112=-224a+b_2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where the usual trigonometrical directions are employed.

If β_1 be the difference of barometric pressures (always for the mean sea-level) for Greenwich *minus* Makerstoun, and β_2 for Greenwich *minus* Dublin, these will be proportional to the perpendiculars from G to the isobaric lines through M and D; hence

$$\frac{\beta_1}{\beta_2}=\frac{b_1}{b_2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

From these three equations we find

$$a=\tan\theta=\frac{112\beta_1-246\beta_2}{77\beta_2-224\beta_1}=\tfrac{1}{2}\cdot\frac{\beta_1-2{\cdot}2\beta_2}{0{\cdot}344\beta_2-\beta_1}; \quad . \quad . \quad . \quad . \quad (4)$$

so that, if θ be counted from the north through the east, south, and west (as for the directions of the wind), we find for the isobar passing through G and M,

$$\beta_1=0, \quad \tan\theta=\frac{\pm 2{\cdot}20}{\mp 0{\cdot}69}, \quad \theta=343° \text{ or } 163°;$$

for the isobar passing through G and D,

* The height given by Dr. Lloyd is 24·4 feet above low water of spring-tides, from which I have subtracted 6 feet to reduce to mean sea-level, as at the other stations (Dublin Observations, vol. i. p 386)

$$\beta_2=0, \quad \tan\theta=\frac{\pm 1}{\mp 2}, \quad \theta=300° \text{ or } 120°;$$

for the isobar passing through M and D,

$$\beta_1=\beta_2, \quad \tan\theta=\frac{\mp 1{\cdot}20}{+1{\cdot}31}; \quad \theta=228° \text{ or } 48°.$$

The following Tables contain the monthly mean barometric pressure at Greenwich for the eight years, and the corresponding values of β_1 and β_2.

TABLE I.—Monthly mean Barometric Pressure at Greenwich reduced to mean Sea-level, 1842–1849. (Add 29 inches.)

Month.	1842.	1843.	1844.	1845.	1846.	1847.	1848.	1849.
	in.	in.	in.	in.	in.	in.	in.	in.
January	1·082	0·851	1·069	0·882	0·847	0·918	0·996	0·949
February	1·057	0·651	0·676	1·021	1·025	0·963	0·692	1·285
March	0·923	0·935	0·886	0·975	0·831	1·066	0·679	1·093
April	1·091	0·861	1·174	0·870	0·763	0·828	0·763	0·692
May	0·955	0·836	1·119	0·885	0·951	0·936	1·097	0·938
June	1·070	0·871	0·984	0·945	1·034	0·976	0·812	1·039
July	0·990	0·996	0·922	0·939	0·925	1·092	1·005	0·958
August	1·037	0·988	0·847	0·899	0·946	1·046	0·892	1·010
September	0·885	1·188	1·052	0·974	0·994	0·997	1·003	0·937
October	1·025	0·778	0·734	1·021	0·688	0·976	0·818	0·918
November	0·775	0·893	0·864	0·750	0·997	1·080	0·961	0·919
December	1·184	1·424	1·066	0·834	0·877	0·868	0·983	0·973
Year	1·006	0·939	0·950	0·916	0·906	0·979	0·892	0·976

TABLE II.—Differences of the Monthly Mean Barometric Pressures at Greenwich and at Makerstoun in thousandths of an inch.

β_1.

Month.	1842.	1843.	1844.	1845.	1846.	1847.	1848.	1849.
January	+256	+256	+135	+129	+219	+ 72	+ 30	+201
February	+206	− 90	+115	+ 73	+170	+ 96	+262	+226
March	+201	+ 34	+118	− 8	+188	+ 51	+ 88	+108
April	− 93	+138	+132	− 9	− 9	+136	− 70	+ 5
May	+ 95	− 20	− 99	− 55	+ 70	+103	+ 95	− 93
June	+ 75	+ 19	+125	+117	+100	+ 73	+106	+ 71
July	+ 94	+130	+ 66	+ 85	+140	+ 68	+156	+145
August	+ 84	+ 99	+127	+ 90	+ 26	+ 63	+135	+121
September	+ 1	+ 20	+ 1	+ 95	+ 30	+159	+ 52	− 89
October	+106	+141	+103	+184	+144	+ 95	+ 32	+ 76
November	+ 90	+184	+ 65	+192	+105	+200	+121	+152
December	+299	+223	− 71	+227	+ 35	+139	+203	+ 8
Year	+118	+ 94	+ 68	+ 93	+101	+105	+101	+ 78

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TABLE III.—Differences of the Monthly Mean Barometric Pressures at Greenwich and Dublin in thousandths of an inch.

β_2.

Month.	1842.	1843.	1844.	1845.	1846.	1847.	1848.	1849.
January......	+ 93	+ 97	− 5	+114	+160	+163	+ 36	+ 79
February ...	+165	− 80	+ 2	+ 33	+ 66	+ 7	+136	+102
March	+ 97	+ 95	+ 22	− 68	+119	+ 53	+ 8	+ 13
April	− 56	+121	+ 74	+ 3	− 55	− 5	+ 32	− 28
May	+ 89	+ 12	−110	−102	+ 38	+119	+ 44	+ 26
June	+ 30	+ 4	+ 70	+ 60	+ 67	+ 1	+ 73	− 15
July	+ 51	+ 24	+ 8	+ 56	+ 84	+ 14	+ 71	+ 61
August	+ 38	+ 70	+ 19	− 15	− 5	− 10	+ 84	+ 52
September...	− 27	− 20	− 13	+ 53	+ 18	+ 26	+ 26	− 57
October	− 41	+ 21	+ 81	+ 96	+ 89	+ 88	+ 27	+ 80
November ...	+119	+100	+ 74	+137	+137	+119	+ 18	+ 92
December ...	+166	+139	+ 40	+ 35	+ 52	+240	+290	+ 60
Year	+ 60	+ 48	+ 22	+ 34	+ 64	+ 68	+ 71	+ 39

Although we may assume, when observations for a considerable period of time are considered, that the isobars over the small space about the three stations may be represented by parallel straight lines, yet this assumption becomes less probable when the observations for single months are employed; since in these occasional cyclones may produce considerable irregularities. I have thought it desirable, however, to calculate the values of θ for each month by equation (4), in order to obtain some approximation to the probable errors of the directions deduced from the whole eight years' observations. The values of θ are given in the following Table.

TABLE IV.—Directions of the Isobars (θ) for each Month, counted from North through East, South, and West.

Month.	1842.	1843.	1844.	1845.	1846.	1847.	1848.	1849.
January	277°	276°	298°	236°	248°	186°	216°	275°
February	242	55	296	270	275	293	265	270
March	268	180	288	348	256	228	292	292
April	78	236	262	129	340	302	131	336
May	232	134	41	14	263	218	269	126
June	274	286	262	265	253	296	251	304
July	263	287	290	254	259	286	270	273
August	270	250	290	287	303	303	257	272
September..............	242	320	342	262	258	289	267	75
October	307	290	243	261	257	233	239	224
November	210	263	219	245	210	259	289	258
December	262	257	133	289	203	196	205	269
Year	268	266	280	278	256	255	251	266

Variations of Yearly Means.—The yearly mean barometric pressure does not seem to vary according to any law (see last line of Table I.). The greatest pressure at all the stations occurred in 1842, and the next

highest in 1847 and 1849, the least pressure being in 1848. There is, however, an appearance of systematic change in the yearly mean values of β_1, β_2, and of θ, which may be noticed on account of its partial coincidence with the decennial period of sun-spots and magnetic variations.

The differences of pressure shown by the isobars passing through the three stations was least in 1844, and greatest in 1847–48*. It does not follow, however, that the tenth-inch isobars were really further separate in 1844 than in 1847–48, since this depends also on the perpendicular distance between the isobars, or on the angle θ which they make with the meridian; thus we find the perpendicular distances of the tenth-inch isobars in geographical miles in the different years to be as follows:—

1842.	1843.	1844.	1845.	1846.	1847.	1848.	1849.
218	273	317	242	273	266	281	330

Thus the isobars were furthest separate in 1844 and 1849, and nearest in 1842 and 1845. The regular variation of β_1 and β_2 was thus chiefly due to the inclination of the isobars to the meridian. From the last line of Table IV. we see that the isobars were from 10° north of west (W. by N. nearly) in 1844, and from 19° south of west (W.S.W. nearly) in 1848. Though there is a coincidence in these epochs with those of minimum and maximum sun-spot frequency, it is a coincidence which requires confirmation by a longer series of observations, the more especially as the change of value of θ from 1848 to 1849 is very considerable.

Annual Variations.—The following Table contains the monthly mean values for the whole eight years, those of θ being derived from the mean values of β_1 and β_2 by equation (4) (they are therefore not the means of the quantities in Table IV.). β_1 and β_2, with their probable errors, are in thousandths of an inch.

TABLE V.—Monthly Means from eight years' Observations.

Month.	Mean. Greenwich, +29.	β_1.	Probable error of β_1.	β_2.	Probable error of β_2.	θ.	Probable error of θ.	$\beta=0{\cdot}1$ in.
	in.					°	°	miles.
January	0·949	+163	±20	92	±14	261	± 9	157
February	0·921	+133	26	54	19	271	13	186
March	0·923	+ 96	15	42	15	271	12	254
April	0·880	+ 28	22	11	15	275	25	839
May	0·964	+ 12	21	14	20	216	25	1710
June	0·966	+ 85	8	36	8	272	5	285
July	0·978	+110	9	46	7	273	3	220
August	0·958	+ 93	9	29	9	280	5	220
September	1·004	+ 33	17	1	9	295	15	546
October	0·870	+110	11	55	12	267	7	238
November	0·905	+138	12	100	9	249	7	186
December	1·026	+133	31	128	23	230	13	178
Year	0·945	+ 95	4	+50	4	264	2	267

* This result was obtained for β_1 from the Greenwich and Makerstoun Observations in 1850 (Trans. Roy. Soc. Edinb. xix. part 2, p. xci).

Annual Variation of Barometric Pressure in England.—In the discussion of the Makerstoun observations of barometric height for the years 1842–49 I sought to determine this law, and found that the greatest pressures occurred in the months from May to September (both inclusive), while the least occurred in October and November; the probable errors of the monthly means were, however, found to be too considerable to give much value to the secondary variations which were shown in the monthly mean values. The mean for the six months April to September was one tenth of an inch greater than that for the six winter months*. Very similar conclusions were arrived at by Dr. Lloyd, in his discussion of the Dublin observations for 1840 to 1850, both as regards the epochs of maximum and minimum pressure, as well as of the probable error (or divergence) of the monthly means†. Somewhat similar conclusions may be deduced for Greenwich from the second column of Table V., the maximum in December being slightly more marked than at Makerstoun and Dublin.

If the isobars made the same angle with the meridian in all the months of the year, and kept at the same interval, the annual variations would be the same at all the stations. This, however, it will be seen, is not the case, and the annual variation of barometric pressure for any place is a compound result.

I have sought to determine the annual law for the centre of gravity of the three stations (a point a little to the east of Liverpool) as derived from the monthly means, reduced to the sea-level, for the three places; these are given below.

		Probable error.			Probable error.
	in.	in.		in.	in.
January ..	29·864	0·027	July	29·926	0·015
February ..	·859	·049	August	·917	·019
March	·877	·035	September ..	·993	·022
April......	·867	·039	October	·815	·033
May	·956	·027	November ..	·826	·027
June	·926	·022	December ..	·939	·049

The probable errors are least for the four months of June to September, and greatest in the months of December and February. The probable errors are much diminished if we determine the means for groups of months in each year. Thus we find:—

	in.
October and November	29·820±·016
December to April	·881±·017
May to August	·931±·010
September	·993±·022

* Trans. Roy. Soc. Edinb. xix. part 2. p. xci, 1850.

† Observations made at the Magnetical and Meteorological Observatory at Trinity College, Dublin, under the direction of Humphrey Lloyd, D.D., D.C.L., vol. ii. p. 347, 1869.

The probable errors are here sufficiently small, when compared with the variations from group to group, to allow us to conclude that we have obtained a considerable approximation to the law of change for the centre of England. It is not a little remarkable that the greatest monthly mean pressure (that for September) is followed immediately by the least, the probable errors leaving no doubt as to this fact*, the difference of pressure from September to October–November being 0·173 inch. It is not improbable that the pressure in December is greater than in the months immediately following; but a longer series of observations is required to determine that question, since the probable error for the month is very large.

The mean barometric pressure at the mean sea-level for a point in 53° 24′ N. and 2° 48′ W. of Greenwich is

$$29{\cdot}897 \text{ inch} \pm {\cdot}010,$$

as deduced from the eight years' observations (1842–49) at the three stations.

Annual Variation of the quantities β_1, β_2, *and* θ.—It will be seen from Table V. that the magnitude of β_1 and β_2 is a maximum in December and January, a minimum in April or May, a secondary maximum in July, and a minimum again in September. This result was obtained by me for β_1 in 1850. The perpendicular distances between the isobars calculated with the aid of the values of θ are given in the last column of Table V.: the distances are least about January and about July; they are greatest in April, May, and September. The following are the differences of barometric pressure corresponding to 100 geographical miles perpendicular to the isobar.

* The probable errors given above and in Table V. have been obtained in the same way as for the observations of a constant quantity. There can be no doubt, however, that the conditions are not strictly the same in the two cases. In the case of deviations of observations of magnetic declination from the mean, I have shown, in the Makerstoun Observations for 1844 (Trans. Roy. Soc. Edinb. vol. xviii. p. 351), that they did not satisfy the hypothesis of positive and negative distribution employed in the calculus of probabilities for the probable error. This is also true for various meteorological phenomena. A consideration of the differences of the monthly mean barometric height has induced me to believe that the formula for the probable error gives such an approximation to the probable deviation of any determination that it may be accepted as a relative measure of the exactness of the result. It will be obvious also that these differences cannot be called errors if they are due to a law. In the present instance it has been remarked that β_1, β_2, and θ seem all to have a somewhat regular variation depending on the year. It will be seen, however, from the last line of Table V., that if we assume this variation to be accidental, the probable difference due to it is so small that it can only slightly increase the probable differences or errors for the different months.

	in.		in.
January........	0·064	July	0·045
February	0·054	August	0·045
March	0·039	September	0·018
April..........	0·012	October........	0·042
May	0·006	November......	0·054
June	0·035	December	0·056

The year .... 0·038 inch.

It thus appears that the difference of pressure for 100 miles is greatest in the coldest months, is a secondary maximum in the warmest months, and is a minimum in April, May, and September*. The only laws resembling this are those for magnetic disturbance, frequency of the aurora borealis, and for the horizontal force of the earth's magnetism.

Annual Variation of the direction of the Isobars.—The values of θ given in Table IV. can be considered as only rough approximations in some months. The probable errors of the mean result (Table V.) have been calculated from Table IV., giving each determination equal weight. I have found that the probable errors thus obtained differ little from the mean deviation of θ, deduced from equation (4) with the limiting values of β_1 ($\pm$ p. e.) and of β_2 ($\pm$ p. e.), excepting for the months of May and April, for which the mean deviations are much greater. This is due to the very small *mean* values of β_1 and β_2, and their comparatively large probable errors for these months.

From the seventh column of Table V. it appears that the isobars run most from south of west in December (nearly from S.W.) and in May (S.W. by S.), and most from north of west in September. Beginning with October, the isobars run from a little south of west; they are from further south in November, and from furthest south of west in December. They again approach the west point in January, remain nearly west and east (within a few degrees) from February to July (excepting May, to which I shall refer presently); move further north in August, to the maximum north of west (25°) in September.

The lines from May run from 54° south of west (to 54° north of east); but this result cannot receive much value, as the probable errors are greater than the mean values of β_1 and β_2. This remark applies also, to a great extent, to the result for April, which, however, agrees nearly with

* I think there is some reason to believe that this law, as regards the months, with some slight modification, is general. Mr. Buchan, in his valuable "first approximation' to charts of isobars for the whole earth, has stated that April is the month in which "pressure is more equally distributed over the globe than in any other month" (Edinb. Trans. vol. xxv. p. 578, 1869). This conclusion depends on the value of the approximation to the isobars for the southern hemisphere, for which the data are not only least numerous, but probably least exact. Mr. Buchan's charts show May to be the month of widest distribution for the northern hemisphere, as has been found here for England.

the directions for February and March. I shall return to the results for April and May in the next part of this paper.

The directions here obtained from eight years' observations, made under the most unexceptionable circumstances in three observatories, will probably not be improved for some time*. On this account I have thought it desirable to project the isobaric lines on small charts, so as to show at a glance their general direction and distribution (see Plate 12). The results for the whole year have so small probable errors that I have projected them on a larger scale† (see Plate 11).

It should be here remarked that there is no ground as yet for affirming that the mean direction and distribution of the isobars for any series of years will be the same as for any following series; and the best determination that can be obtained for any given time may be of the greatest importance for comparison with another of equal value at a later period for the answer to this question.

2. *Relation of the Direction and Interval of the Isobars to the Direction and Force of the Wind.*

These relations have been hitherto studied chiefly with reference to cases of strong winds or storms. The conclusions have been obtained directly from the projections on charts of the isobaric lines and wind-directions on particular days. In the following part of this paper, as in the preceding, the results are sought from the observations made on every day throughout the series of years employed; they may therefore serve as a base for the study of particular cases.

* I have sought to obtain some confirmation for the directions of the isobars in Great Britain from the numerous observations made in 1857 to 1867, employed by Mr. Buchan in the formation of his charts. Mr. Buchan's object in the construction of these charts appears to have been to obtain the best possible approximation to the broad features of the distribution of the isobars for the whole earth; and he has exercised his judgment in employing the mass of observations which he was able to collect (Edinb. Trans. vol. xxv. pp. 575, 576). My examination of these for the months of April and May, in which the mean change of pressure for a distance of 100 miles is only a few thousandths of an inch of mercury, has shown the difficulty of deducing from them any accurate determination, such as is here attempted, for a small space like England.

† These charts are not projections, and were devised by me for another investigation, for which the true areas on the sphere were required; they are, however, probably not new. The following will give the most simple idea of their construction. If we suppose the surface of a globe to be built up of series of rings of cylindrical wires laid on the parallels of latitude, and cut out the wires covering any part of the surface, then if these be laid out touching each other on a plane surface, in straight lines perpendicular to a central or principal meridian, the external meridians will be curves of sines (straight lines for 20° squares on the scale of the chart for the year). The distances of any point perpendicularly to a circle of latitude and to the principal meridian are the true lengths of the arcs in latitude and longitude from these circles. For small spaces the distortion is inconsiderable. In the charts here given the principal meridian, that of Greenwich, is not in the middle, the direction of the isobaric lines having been referred to that meridian.

Directions and Force of the Wind.—In most meteorological investigations, and especially in those connected with climate, it is essential to obtain the force and frequency of the winds for each point of the compass. It has been usual, however, in many researches to determine the *resultant* direction and force for given periods by the known law of the composition of forces. It is quite possible that this resultant may be in a direction from which no wind ever blows, but it does not the less represent what all the other winds would have been equivalent to, both in direction and force. It may also be affirmed that in fact the resultant direction thus obtained is generally that of the prevailing wind.

In questions such as the present, if any general relation subsists between the direction of the isobars and that of the winds, the mean direction of the one will probably be affected in like manner with that of the other *. It occurred to me then to compare the *resultant* directions of the wind with those of the isobars for each month of the year. Possessing these resultants for Makerstoun for each month derived from four years' observations (1843–1846) †, it was necessary to compute the corresponding values for Greenwich and Dublin, and to calculate the mean directions and intervals of the isobars for the same years.

The observations at Greenwich were made with an Osler's anemometer, and only pressures which on the mean for an hour exceeded one fourth of a pound were noted. At Makerstoun the instrument employed was of a wholly different description and not self-registering; observations were made of the maximum pressures (if not less than one tenth of a pound) within ten minutes at each observation hour (see the Introductions to the Makerstoun Observations). Observations of the direction and force of the wind were not begun at the observatory of Trinity College, Dublin, till 1845; but I have obtained observations for the four years in question from the volume of observations made at the Ordnance Observatory, Phœnix Park, in a locality stated by Dr. Lloyd to be wholly unexceptionable‡. The instrument employed at the Phœnix Park observatory was a Whewell's anemometer, which always worked imperfectly, so that the observed runs of the pencil differed from time to time for the same wind-velocities §. The monthly resultants calculated from the

* It is to be observed that the mode in which the resultant wind-direction is obtained is not the same as that employed for the direction of the lines of equal pressure; but this difference can only affect results derived from periods during which the wind has blown frequently in very different or opposite directions; and even in such cases the isobars should retain their relation to the predominating wind, if the resultant force for that direction is considerable. A special investigation will show, however, the amount of error which may be due to this cause.

† Trans. Roy. Soc. Edinb. vol. xix. pt. 2, p. 99.

‡ Observations made at the Observatory, Trinity College, Dublin, vol. i. p. 384.

§ Introduction to Observations made at the Phœnix Park Observatory.

numbers of runs have been combined by me in the same way as for the forces to obtain the means for the four years.

The resultant forces cannot therefore be considered absolute measures at any of the three stations; I have, however, endeavoured to reduce them to a common unit*.

In order to determine the factor required to give approximately the true wind-pressures, I have assumed that the mean velocities of the wind at Greenwich and Makerstoun do not differ much from those shown by Robinson's anemometers at Kew and Oxford. Mr. Johnson found that the mean velocities at Kew and Oxford were nearly the same †; the mean velocity at Oxford, 110 feet above the ground, from four years' observations (1857–61) was 10·2 miles an hour. This velocity, employing the generally accepted formula $P = \frac{V^2}{200}$, gives a constant mean pressure of 0·52 lb. on the square foot of surface ‡; the mean of the observed pressures during the four years 1843 to 1846 (without reference to direction) was 0·46 lb. at Makerstoun and 0·48 lb. at Greenwich, an agreement which, considering all the circumstances, the difference of instruments, modes of observation, and distances from the ground of the air stratum whose force was measured, must be quite accidental.

The resultant forces for Makerstoun have then been multiplied by 1·13 and those for Greenwich by 1·08 to reduce to the mean, 0·52 lb. at Oxford. In the case of the pencil-runs on Whewell's anemometer at Dublin they have been divided by 50, which gives an approximation to the mean pressures at the other stations. The means of the three resultant pressures found for each month from the four years' observations may then be taken as approximations to the resultant mean pressure for the mean resultant direction over the space included by the three stations.

The following Table contains the mean barometric pressure at Greenwich, with the differences (β_1 and β_2) from the mean pressures at Makerstoun and Dublin (at mean sea-level), and the resultant directions of the wind at the three stations.

* At Dublin and Makerstoun the vanes were nearly 20 feet above the ground (at Greenwich I believe the height was greater). I do not know any objection to the position of the vanes which could vitiate the observed *directions* of the wind at any of the stations. From my knowledge of the instrument used at Makerstoun and of the care with which the observations were made, I believe the means to be very good *relative* measures of the forces.

† Radcliffe Observatory Observations, vol. xviii., introduction to Meteorological Observations in 1857, p. xxiv.

‡ This formula is, I believe, very far from being accurate; the true relation between the pressure and velocity of the wind, as shown by different anemometers, requires an integrating pressure instrument, in which the sums of pressures may be shown by work done.

TABLE VI.—Directions of the Isobars and of the Wind, 1843–1846.

Month.	Greenwich barom. +29 in.	β_1.	β_2.	θ.	Resultant wind-direction.				$\theta-\phi$.
					Mak.	Dubl.	Green.	Mean ϕ.	
	in.	in.	in.	°	°	°	°	°	°
January ...	0·912	+·185	+·091	267	241	255	246	247	+20
February...	0·843	+·068	+·005	293	287	298	256	280	+13
March	0·907	+·084	+·042	267	244	228	245	239	+28
April	0·918	+·064	+·037	260	274	266	241	260	0
May	0·948	−·026	−·040	22	24	−27	3	0	+22
June.........	0·959	+·091	+·051	262	233	255	230	240	+22
July	0·946	+·106	+·043	274	230	276	241	249	+25
August......	0·920	+·086	+·017	287	247	300	246	264	+23
September	1·052	+·037	+·009	284	244	261	224	243	+41
October ...	0·805	+·143	+·071	267	237	267	232	245	+22
November	0·876	+·136	+·112	240	211	251	216	226	+14
December	1·051	+·104	+·067	255	239	270	262	257	− 2
Year	0·928	+·090	+·042	269	243	261	243	249	+20

Relation of the Direction of the Isobars to that of the Wind.—The resultant direction of the wind at Greenwich is the same as that at Makerstoun on the mean of four years, but the monthly directions differ on the average 12° (without reference to sign). The resultant direction at Dublin for the four years differs considerably (18°) from that at the other two stations. The mean of the three resultant directions (ϕ) is given in Table VI., together with the differences ($\theta-\phi$).

It appears that the direction of the isobars was *positive* of that of the wind in ten months of the year, the directions being nearly the same in two months, April and December; the difference was greatest in September. From the mean directions for the four years we find

$$\theta-\phi=+20^\circ\ *.$$

I believe that the only really exceptional result is that for December, since the determination of θ for April and September depends on very small values of β_1 and β_2; the mean of the results for these two months is, however, exactly the mean for the year. The most remarkable of the

* "The wind in storms neither blows round the centre of least pressure in circles, or as tangents to the concentric isobarics, nor does it blow directly towards that centre. It takes a direction intermediate, approaching, however, more nearly to the direction and course of the circular curves than of the radii to the centre" ("On the Mean Pressure of the Atmosphere," by Mr. A. Buchan, Edinb. Trans. vol. xxv. p. 581); that is, the angle with the isobar is less than 45°. Mr. Buchan adds, "or, according to Dr. Buys-Ballot, the angle is not a right angle, but from 60° to 80°;" that is, from 30° to 10° with the isobar. The mean of these limits is 20°, exactly what has been found above from the mean direction of the isobars and resultant direction of the wind in all cases. I have not been able to find any memoir by the distinguished Utrecht meteorologist containing the grounds on which his limits are founded. The tendency of the winds inwards (towards the centre) in cyclones is noticed by several early writers on the subject, among others by Redfield (Silliman's Journal, vol. i. p. 14, 1846).

differences is that for May, which also depends on small negative values of β_1 and β_2, yet gives a result differing but little from the mean, though the direction of the wind (a little to west of north) is upwards of 100° from the mean direction *.

Relation of the difference of barometric pressures for 100 *miles to the resultant force of the wind.*—The differences of barometric pressure for 100 geographical miles perpendicular to the isobars (100 mile *gradients* †) have been calculated from the values of β_1, β_2, and θ; these, with the resultant mean pressures for the wind obtained as indicated p. 525, are given in Table VII. The 6th column contains the mean pressure (P) of the wind in pounds on the square foot of surface, derived from the results for the three stations. The last column contains the differences of barometric pressure for 64 miles of interval, for which, in the mean, one thousandth of an inch of barometric pressure is equivalent to one hundredth of a pound of pressure of the wind.

TABLE VII.—Resultant forces of the Wind and difference of Barometric Pressure, 1843–1846.

Month.	β for 100 miles	Resultant force of wind.				β, for 64 miles.
		Green.	Mak.	Dubl.	Mean P.	
	in.	lb.	lb.	lb.	lb.	in.
January	0·074	0·66	0·49	0·71	0·62	0·047
February	·035	·22	·20	·14	·19	·022
March	·035	·21	·27	·28	·25	·022
April	·028	·34	·11	·20	·22	·018
May	·016	·23	·07	·09	·13	·010
June	·036	·27	·25	·17	·23	·023
July	·046	·29	·23	·20	·24	·028
August	·040	·25	·12	·14	·17	·026
September	·017	·08	·08	·04	·07	·011
October	·057	·41	·26	·32	·33	·036
November	·054	·40	·29	·23	·31	·035
December	·040	·16	·37	·26	·26	·026
Years	0·036	0·25	0·20	0·23	0·23	0·023

* This agreement is probably to some extent accidental: it will be seen that the direction of the wind at Dublin was $-27°$ (27° west of north), while it was near north at Greenwich, and 24° east of north at Makerstoun. When the resultant direction for May at Dublin is determined by the frequency of the wind from different points (by Lambert's formula), it is found to be $+20°$, nearly as at Makerstoun; the difference, $\theta-\phi$, for May would then become $+6°$. I may remark that the resultant directions for the other months at Dublin obtained by the same formula agree better with the mean of those at Greenwich and Makerstoun than when the "runs" of the pencil are employed. The resultant direction for the four years is 256° by the frequency, or 5° further south of west than when determined by the pencil-runs. The resultant direction at Makerstoun for the four years is exactly the same by the two methods.

† I have felt some difficulty in employing the word gradient, due to Mr. Thomas Stevenson, C.E., in this relation, as it is associated to some extent with a hypothesis of

It has already been stated that the resultant pressures of the wind are only rough approximations, yet these indicate very distinctly the same annual law of variation as the *diabars*; not only so, the results for each month show no great divergences from the ratio stated above derived from the mean for the four years.

When we remember that these resultant forces are obtained in some cases from winds proceeding from opposite directions, which destroy each other to a great extent, and that the mean differences of barometric pressure are obtained similarly from pressures which increase in opposite directions, it seems probable that the relation found always holds, though the ratio may vary somewhat with winds of different forces and from different directions. The variations including the greater divergences from the mean must also be the object of special investigations, in which winds from the same direction, and with nearly the same mean forces, must be considered alone.

Ratio of the mean pressure of the wind (surface current), independent of direction, to the resultant pressure.—It has been stated that the pressure of the wind was always noted at Makerstoun whenever the maximum pressure within an interval of 10 minutes at the hour of observation was at least one tenth of a pound on a square foot of surface, and that although the absolute mean pressures cannot be derived directly from these observations, yet that their *relative* values are probably determined with considerable accuracy. We may then obtain a measure of the variability of the wind, that is of the degree of opposedness of the masses of air in motion at the earth's surface, from the ratios of the mean (M) to the resultant (R) pressures. Those at Makerstoun for the four years, 1843 to 1846, were as follow*:—

	M. lb.	$\frac{M}{R}$.	I. miles.		M. lb.	$\frac{M}{R}$.	I. miles.
January	0·72	1·5	135	July	0·40	1·7	227
February	0·63	3·1	286	August	0·32	2·5	250
March	0·63	2·3	286	September	0·27	3·4	588
April	0·52	4·6	357	October	0·58	2·2	175
May	0·51	7·5	625	November	0·59	2·0	185
June	0·51	2·0	278	December	0·60	1·6	250

We see that the greatest proportion of the surface current proceeded from one quadrant in the winter months of December and January, and in the summer months of June and July; that the wind was most variable in May, April, and September. We have already found (p. 522)

atmospheric surfaces with hills and valleys, into the latter of which the former are supposed to flow. On this account I should much prefer some other word (such as *diabar* or *barode*), which would serve to indicate the relation of the difference of pressure to the unit of distance.

* "Results of Makerstoun Observations," Trans. Roy. Soc. Edinb. vol. xix. pt. 2, art. 209, p. xcvi (multiplied by 1·13, as before).

that the directions of the isobars varied most in the same months, May being the month of greatest variability*.

I have added the perpendicular distances (I) between the tenth-inch isobars in geographical miles (deduced from the values of β, second column of Table VII.). These intervals show a remarkable constancy of ratio to the variability of the wind. In the mean for the four years at Makerstoun,

$$\frac{M}{R}=\frac{0{\cdot}52}{0{\cdot}20}=2{\cdot}60;\ I=278 \text{ miles}.$$

The intervals between the tenth-inch isobars increase with the variability of the wind. These, it will be remembered, are mean results. The mean pressure of the wind has its mean value in the two months of April and May, when the direction is most variable; but in September, the next epoch of maximum variability, the mean pressure is a minimum.

It appears also from the fourth column of Table VIII. that the direction of the cirrus current was most variable in April and May; the next epoch of maximum variability was, however, in August instead of September, as for the surface current.

3. *Atmospheric Currents.*

In the preceding investigations we have employed the observations of the direction of the surface current only. We require greatly to know to what extent the direction would vary in ascending from the earth. The late Mr. Johnson, Director of the Oxford Observatory, found that an anemometer at a height of 110 feet from the ground showed a wind-velocity two and a quarter times that indicated by a similar instrument 22 feet from the soil; part of this difference was probably due to buildings and trees in the neighbourhood; but there can be little doubt that proximity to the soil is a cause of diminished velocity. I am acquainted with no observations, made at points free from all obstructions, which can answer the question, whether the *direction* of the wind varies considerably within a few hundred feet of the ground. The only way, then, of investigating this subject must be by comparisons of the direction of the wind near the surface, in well-exposed localities, with that of the motions of the clouds.

Such observations require in general much time; to be of any use to science they should be made frequently and systematically. This can be

* I may remark here that the variability of the direction of the wind and of the intervals and direction of the isobars have no relation whatever to the diurnal variation of temperature, nor to the variation of temperature from day to day. The diurnal range of temperature was greatest in the mean of the four years in June and August. The difference of the daily mean temperature from the monthly mean was least in August, May, and July; less in these months than half the mean difference for January (5°). ("Results of Makerstoun Observations," Trans. Roy. Soc. Edinb. vol. xix. pt. 2, p. lxxxv.)

done well only at observatories where assistants are continually on the watch, during the day at least; the employment of self-registering instruments has diminished the number of observers required in permanent institutions, and observations of cloud-motions can now scarcely be expected with the requisite completeness. On board ship these observations are made with even more difficulty than on shore; rapidly moving upper currents, especially those in which clouds are seen moving from very different or nearly opposite directions, catch the eye at once, and are most frequently recorded. No serious investigations can be founded on observations when the most frequent motions are really not observed.

Believing that any just conclusions as to the atmospheric currents could be obtained only from long and careful observations of cloud-motion, I began a series of observations in 1842 at Makerstoun, which I continued in the following years with my assistants, the late Mr. John Welsh, F.R.S., and Mr. A. Hogg*. Four currents were distinguished at different heights. 1st, the surface current observed by the wind-vane; 2nd, the current of loose cumulus (*scud*) and cumulus; 3rd, the current of cirro-stratus; 4th, the current of cirro-cumulus and cirrus†. The observations of these currents have been only partially discussed; I shall give here some of the results which bear upon the subject of the present investigation, derived from the observations made in the four years 1843 to 1846, for which the directions of the surface-wind and of the isobaric lines have been already obtained.

Direction of the Cirrus current.—Having found the *mean* direction in which the cirri and cirro-cumuli moved for each day on which their motion was observed during the four years, I have combined these by Lambert's formula (on the assumption that the velocity was the same on each day) in order to obtain the resultant direction (ψ) for each month; these are given in Table VIII.

* The motions of the clouds were determined by seeking till a marked portion was found which seemed to run up or down one of the four corners of the observatory, the points of the compass relatively to which were marked on a surrounding paling; when no portion of cloud could be found that would pass or had passed through the zenith, the vanishing-point of the motions of different portions of cloud in the same stratum could generally be ascertained very nearly. It is believed that in a great majority of cases the directions were found to within half a point of the compass, though the nearest point was always noted. In the year 1844 alone 4370 observations were made of the directions of motion of the cloud-currents.

† The same classification of cloud-motions was proposed by M. Poey to the Paris Academy of Science in 1864 (Comptes Rendus, t. lviii. p. 669), excepting that he separated the loose cumuli from the cumuli: this separation I had also made, especially in the north-east quadrant, in which the loose cumuli have a much lower position than in the other quadrants; but in taking the mean directions the two strata were included in one. M. Poey has, I believe, recommended this system of observation of the atmospheric currents to the Meteorological Committee of the Royal Society, a recommendation in which I entirely concur.

TABLE VIII.—Resultant Direction of Cirri and Surface-wind, 1843–1846.

Month and Year.	No. of days.	Cirri resultant.		Wind res. direc. ϕ.	$\psi-\phi$.
		Dir. ψ.	No. p. 100.		
January	57	279°	62·6	241°	+38°
February	41	303	62·7	287	+16
March	42	301	62·7	244	+57
April	50	242	39·4	274	−32
May	44	291	39·9	24	−94
June	51	263	56·9	233	+30
July	42	269	58·0	230	+39
August	56	267	48·0	247	+20
September	34	274	60·4	244	+30
October	35	276	56·0	237	+39
November	38	236	56·9	211	+25
December	44	308	69·3	249	+59
1843	119	276	48·0	249	+27
1844	155	288	56·4	249	+39
1845	150	280	50·9	247	+33
1846	110	259	59·1	229	+30
4 years	534	277	52·0	243	+34

I have entered for comparison the resultant directions (ϕ) of the surface current at Makerstoun. It will be seen that in ten months of the year the values of $\psi-\phi$ are positive; in two months, April and May, they are negative*: the greatest positive difference is that for December.

When we determine the resultant directions from all the observations in each of the four years (see Table VIII.), we find that the difference of directions of the cirrus and surface currents may be given as $34^\circ \pm 2^\circ$.

It has to be pointed out, however, that these directions are not strictly comparable; the direction of the wind is observed at all hours when the wind blows; those of the currents carrying clouds can be observed only when clouds exist in them. The motion of the cirri was observed at Makerstoun only in two days out of five, whereas the direction of the wind was observed on most days in each year. This fact may affect the exactness of the differences for limited periods; but it will be shown that it affects little the final results. There is, however, a correction required that should not be neglected. The observations of cloud-motions were generally during the day, that is, on the mean of the year, from

* This difference is, I believe, chiefly due to the north-east winds which blow so frequently in these months. The scud carried by this current is the lowest cloud observed. This and the cirrus stratum appear generally to be set in motion by different causes; the cirri move rarely from an easterly point, and the maximum of frequency shown for north-east winds in Great Britain does not appear in the results for the cirrrus stratum (Comptes Rendus, t. lxxxi. p. 34, 1875).

6 A.M. to 6 P.M. Now at Makerstoun during the four years 1843–1846 the mean of the resultant directions for the hours 6 A.M. to 6 P.M. was 248°. Hence we have

$$\psi-\phi=277°-248°=+29°.$$

So many causes may affect the direction of the surface current in certain places that it cannot fail to appear possible, if not probable, that we have here a result depending on local conditions, contour of the country, or accidental causes. This possibility it is essential to consider. In the first place it will have been remarked that the mean direction of the wind from the four years' observations, 27° south of west, is the same as has been deduced from the Greenwich observations. In the next place, the mean direction of the wind for Scotland, deduced from eleven years' observations (1857–1867) at 9 A.M. and 9 P.M., made at *fifty-five* stations, was 32° south of west*, while at Makerstoun, from observations at the same hours in the four years, 1843 to 1846, it was 29° south of west†—a difference of only 3°, which would go to increase that of $\psi-\phi$.

Fortunately I have been able to confirm the result for the difference of directions of the cloud and surface currents by observations made in a very different locality. M. Quetelet observed the directions of the cloud-motions (without distinction of species) at the Brussels Observatory during the years 1833 to 1846, and he found the resultant directions by Lambert's formula as follows ‡:—

In the 7 years 1833 to 1839....$(\psi)=258°\ 53'$
Do. 1840 to 1846.... „ $=255\ 21$
In the 14 years 1833 to 1846.... „ $=257\ 50$;

whereas by the wind-vane of Osler's anemometer (which was erected in 1842) the direction was found

In the 5 years 1842 to 1846....$\phi=225°\ 49'$

A comparison of the numbers of times the clouds and surface-wind had moved from each point of the compass in the five years 1842 to 1846 induced Quetelet to think that the clouds, as well as the resultant surface-wind, had really proceeded from a more southerly point than in the preceding years. Had the eminent director of the Brussels Observatory sought the resultant directions for each year, he would have found that marked differences of the direction of motion existed in each of the five years. I have calculated these directions from Quetelet's numbers; they are as follows:—

* This result I have deduced from Table II. of Mr. Buchan's paper already cited, Trans. Roy. Soc. Edinb. vol. xxv. p. 616.

† "Results of Makerstoun Observations," Trans. Roy. Soc. Edinb. vol. xix. pt. 2, p. c.

‡ Sur le Climat de la Belgique, 2de partie, p. 8.

Year.	(ψ).	ϕ.	$(\psi)-\phi$.
1842..........	230°	221°	$+\ 9^\circ$*
1843..........	257	232	+25
1844..........	283	251	+32
1845..........	251	219	+32
1846..........	252	216	+36
5 years........	255	226	+29

If we admit that a correction should be applied, as at Makerstoun, on account of the difference of the direction of the wind during the day hours when the cloud-motions are observed, we find for Brussels

$$(\psi)-\phi=24^\circ.$$

The agreement of the results for the last four years, which are the same as in the preceding discussion, is very remarkable; thus the direction of the surface-wind at Brussels became 32° more southerly in 1845 than in 1844, and the direction of the cloud-motion changed exactly by the same number of degrees and in the same direction. A similar though less marked result will be seen at Makerstoun (Table VIII.), the cirri moving from a point 21° more southerly in 1846 than in 1845, while the surface current changed 18° in the same direction.

It has been remarked (p. 531) that the wind and cloud directions are not exactly comparable, as the latter cannot be observed so frequently as the former; this fact has less weight in the results for Brussels than in those for Makerstoun, since the former include clouds of all kinds, which are observed much oftener than the cirri alone. There is, however, another method of comparing the directions of motion which was employed by me at first in the discussion of the Makerstoun observations for 1843. The *differences* of the directions of the surface and cloud currents when observed simultaneously were noted: when several such comparisons made at successive hours gave nearly the same difference, the mean was termed a "result;" when the differences varied considerably two or three results might be obtained in the same day. From these results for the four years 1843 to 1846 the following mean differences were obtained:—

1. *Cirrus current minus surface current.*

Number of results.				Mean difference.
Total.	Per 1000. +	Per 1000. −	Per 1000. =	$\psi-\phi$.
359	769	181	50	$+29^\circ{\cdot}6$

The total number of results was 359; as the motions of tne cirri were observed on 534 days in the four years, there were several days on which no surface current was blowing when the motions of the cirri were

* The small value of $(\psi)-\phi$ for 1842, compared with those for the following years, may perhaps be due to some instrumental cause, as this was the first year in which the anemometer was employed.

determined. The numbers of results in 1000 for which $\psi-\phi$ was positive (+), negative (−), or the same (=) are given. The final value of $\psi-\phi$ is the same *to a fraction of a degree* as that found, p. 532. This confirmation is of importance, as it shows that the method of calculating the resultant directions does not affect the accuracy of the final values.

It is probable that this difference between the directions of the cirrus and surface currents will not take place *per saltem*, and that the observations of cloud-motions at different heights should show some variation in the value of $\psi-\phi$. If we should find that the lowest stratum of clouds, that of scud and cumuli, which may be considered generally at from 2000 to 5000 feet above the ground, has its direction intermediate between that of the cirrus and that of the surface, we shall have a most conclusive proof, were other proof needed, that the differences of motion found are not due to local causes.

In the four years 1843 to 1846, 339 results were obtained from simultaneous observations of the cumulus and cirrus motions; from these and comparisons of the other currents the following quantities are derived, where ψ, ψ', ψ'', and ϕ are the mean directions of the cirrus, cirro-stratus, cumulus, and surface currents respectively:—

2. *Cirrus current minus scud and cumulus current.*

Number of results.				Mean difference.
	Per 1000.			
Total.	+	−	=	$\psi-\psi''$.
339	666	251	83	$+13°\cdot7$

The difference is somewhat less than half of that between the cirrus and surface currents. As the currents approach each other the differences become smaller, and the relative number of negative results increases. Thus when we examine the 683 results derived from simultaneous observations of the cirro-stratus and cumulus currents which pass into each other, we find:—

3. *Cirro-stratus current minus cumulus current.*

Number of results.				Mean difference.
	Per 1000.			
Total.	+	−	=	$\psi'-\psi''$.
683	568	297	135	$+6°\cdot9$

which is again nearly half the difference between the cirrus and cumulus current.

Having found, I think conclusively, that the differences of motion observed between the surface and cirrus currents are independent of circumstances of locality, we may with confidence examine the results derived from the more numerous comparisons of the surface current with the cirro-stratus and cumulus currents. Thus we find:—

4. *Cirro-stratus current minus surface current.*

Number of results.				Mean difference.
Total.	Per 1000. +	−	=	$\psi'-\phi$.
754	788	183	29	$+22^\circ\cdot8$

5. *Cumulus current minus surface current.*

Number of results.				Mean difference.
Total.	Per 1000. +	−	=	$\psi''-\phi$.
1434	791	171	38	$+14^\circ\cdot5$

If we subtract the 5th mean difference from the 1st and from the 4th, we find

$$\psi-\psi''=+15^\circ\cdot1,$$
$$\psi'-\psi''=+\ 8^\circ\cdot3.$$

These two mean differences are slightly greater than were found by the direct comparisons 2 and 3.

If we give the 1st, 4th, and 5th mean differences weights corresponding to the number of results from which they are obtained, then we find

$$(\psi)-\phi=+19^\circ\cdot1\ ;$$

and if the three mean differences receive equal weights,

$$(\psi)-\phi=+22^\circ\cdot3\ ;$$

where (ψ) indicates, as before, the mean direction from the north (through east) of *all* the species of clouds. The former of these results may represent approximately the *observed* difference when no attention has been paid to the species of cloud: the latter may be considered an approximation to the mean difference for the whole mass of air in motion. Each of these results is somewhat less than that found for Brussels (24°).

We have no knowledge of the mean law according to which the velocity of the air in motion varies with height above the ground. From previous observations, while observing the aerial currents at Makerstoun, I had concluded that the maximum velocity occurred rarely at a greater height than 6000 feet, and that the *mean* stratum of maximum velocity was probably not above 5000 feet. From this height, then, the velocity diminishes as we ascend and descend, but according to different laws. At the *surface* of level ground the velocity is, on an average, probably not one third of that at the maximum; the upper limit, where the motion ceases, is in all probability little above the average height of the cirri. I had frequently observed at Makerstoun that the angular movement of cirri passing through the zenith was generally very much smaller compared with that of the lower clouds than could be explained by the difference of heights had the velocities been the same.

If we remember the increased dimensions of a unit of mass of the atmosphere as we ascend and take the equation

$$mv_h=\frac{1}{n}\Sigma(mv),$$

where m is the unit of mass, v_h the velocity at a height h, n the number of units in the vertical column in motion, and $\Sigma(mv)$ the sum of the units of mass in the column into their velocities, then it seems probable that the height h will not be higher than the stratum of the cirro-stratus, nor lower than that of the scud and cumulus. In all probability, then, we shall have $(\psi)-\phi$ between 23° and 15°; and there cannot be any considerable error in concluding that for the height h

$$(\psi)-\phi=+20 \text{ approximately*}.$$

It will be remembered that we have already found (p. 526) that

$$\theta-\phi=+20°;$$

and since ϕ, the direction of the surface current, appears similarly in this and in the preceding equation, we have approximately †

$$\theta-(\psi)=0.$$

Or, *That the average direction of the isobaric lines and the mean direction of the mass of air in motion are approximately the same.*

Cause of the difference between the direction of the motion of the upper and lower currents.—In the preceding investigation we have been occupied simply with numerical relations following directly from observations made without reference to any hypothesis. It will be easy, however, to show that the results obtained relatively to the different directions of motion of the atmospheric currents are in strict conformity with the reasoning of writers on this subject, from Hadley to Herschel.

Putting aside altogether the hypothetical ascending and descending currents of air, which were first suggested by Hooke, and for which we have no evidence whatever, we owe to Hadley the first indication of the

* If we consider the mean value of $(\psi)-\phi$ for each stratum to be half that at the upper and lower limits (it should probably be more than half), and assume that for the stratum above the cirri to be 32° ($\psi-\phi$ for the cirri being 29°), we have—

Soil to scud and cumulus stratum	$=+$	7°·25
Scud and cumulus to cirro-stratus	$=$	18·65
Cirro-stratus to cirrus	$=$	26·20
Cirrus to upper limit of motion	$=$	32·00

and give to each equal weight, we find the mean,

$$(\psi)-\phi=+21°.$$

There is no hypothesis that can be proposed, founded on the observed differences, which could affect the final results to any considerable extent, nor the conclusion which is deduced from it.

† The difference $(\psi)-\phi$ deduced from the observations at Makerstoun, confirmed approximately by those at Brussels, being supposed to hold at Greenwich and Dublin.

fact, that if a mass of air changes its latitude, from whatever cause, it will not proceed in the direction in which it was originally propelled or drawn; but if it passes to a higher latitude the motion will be towards a point more easterly, and if to a lower latitude towards a point more westerly than the original direction; that is, in our hemisphere the ultimate direction of motion will be *positive* of the original direction.

The friction of the moving mass of air against the earth's surface is always recognized as a cause which diminishes the amount of this displacement. Thus Hadley, on the lower currents from the tropics towards the equator:—"Before the air from the tropics can arrive at the equator, it must have gained some motion eastwards from the surface of the earth or sea whereby its relative motion will be diminished" *. The upper current moving towards the north is supposed by him to preserve its excess of velocity till it descends. So Sir John Herschel on the same hypothetical upper current:—"In flowing over to regain its level, it commences its course relatively in a meridional direction, but really with the full amount of easterly velocity which the earth's equator has; and since this, as it proceeds north or southwards, is in excess of what would suffice to keep it on the same meridian, it continually deviates to the westward [*i. e.* the direction from which it appears to proceed]; and when it again returns to the earth in its circulation, which it does on both sides beyond the tropics, it does so with a powerful westward tendency, *and the more, as in its course it has been less under the influence of surface friction owing to the elevated region in which it has travelled*" †. I have put in italics the explanation of the difference of directions of the currents found by me.

The theory of the trade-winds is so mixed up with unknown currents, ascending and descending, and presents so many other difficulties as usually stated, that it is not easy to determine what may be due to differences of latitude velocity, and what to other causes in the production of winds with definite directions in different parts of the globe. It is not a little remarkable, however, that no writer, as far as I am aware, had perceived that if the difference of latitude velocity affected the direction of motion of a vertical mass of air, this fact should be observed at once, especially in middle latitudes, in the movements of the wind and of the clouds.

If, as Sir John Herschel shows, the *lower* stratum of a mass of air moving northwards has its tendency to move eastwards most diminished, this diminution will be communicated gradually by the viscosity of the air from layer to layer till the upper stratum of the mass in motion is attained where "the influence of the surface-friction" is least‡. We

* Phil. Trans. 1735, p. 61.

† Meteorology, by Sir J. Herschel, 1861, p. 59.

‡ It is always understood that friction diminishes the velocity in all directions; but the cause which propels or draws a mass of air from one latitude to another is probably continuous for some time at least.

have here at once an explanation of the difference of directions found by me, and a proof of the conservation of the excess (or defect if proceeding towards the equator) of the initial latitude velocity; that is to say, another proof of the earth's rotation.

It is obvious that this explanation refers only to the mass of air set in motion by the same cause; it is probable, however, that the lower and upper strata are frequently propelled in different directions. This is evident in the case already noticed of the north-east winds and cirrus currents (footnote, p. 531); and is shown without doubt in the cases for which the upper currents proceed from points *negative* of the lower currents. I may also remark that the difference of direction of these currents will depend on the nature of the surface and on the difference of latitudes of the place from which the air originally started and the place over which it is moving. It is probably due to this cause that the difference $(\psi)-\phi$ is found greatest for winds from the southern quadrants; but this difference and its relation to θ will be considered in a special investigation.

It is not my intention at present to enter into the many consequences which follow from the facts developed in this paper. It is evident that they indicate a wholly new theory of the circulation of the atmosphere. It was believed that the air was continually flowing into atmospheric basins; and as the barometer gave no indication of the fact, it became a logical necessity, as the air could not flow out below, that it must rush up above and flow off in still more rapid currents in directions for which there was neither law nor reason. It appears that the air does not pour into these basins, but that it moves as a whole in the directions of equal pressure.

Postscriptum.—I have remarked (see footnote, p. 526), on Mr. Buchan's authority, that Dr. Buys-Ballot had found the direction of the wind relatively to that of the isobars to be between 10° and 30°, giving an average of 20°. Mr. Buchan informs me (in a letter dated Dec. 27, 1876) that this result was communicated to him by letter. I owe also, at the same time, to the eminent Scottish meteorologist the communication of a similar investigation by the Rev. W. Clement Ley, some of whose results I am glad to be able to add to this paper.

Mr. Clement Ley has projected the isobarics obtained from the observations for one hour made at 15 stations, telegraphed daily: by means of a graduated circle he obtained approximately the directions of the isobars at each place (for three months nearly). These directions were then compared with the telegraphed wind directions, which were given to two points of the compass only; he found the average inclination of the wind to the isobar $(\theta-\phi)$ to vary at the different places from $+5°$ to $+41°$, and that the mean of the whole gave

$$\theta-\psi=+21° \text{ nearly.}$$

Considering the many sources of error in the telegraphed observations,

especially in those for the direction of the wind at a single hour, the agreement of the final result with Dr. Buys-Ballot's mean (obtained probably in the same way), and that found here by a wholly different method, is very satisfactory.

One of the most interesting parts of Mr. Clement Ley's paper is the confirmation given, by so zealous and accurate an observer, of the results relating to the different directions of the aerial currents discovered by me upwards of thirty years ago. The author remarks,—"From many thousands of observations made at well-exposed situations in the Midland Counties of England, I have found that in about seven cases out of eight, with a light air or moderate wind, if the observer *exactly face* the wind on the earth's surface, he finds the current in clouds of no great altitude overhead to be a little *from* his *right*. This difference, it may be remarked, is commonly greatest and most uniformly noticed in dull or rainy weather with S.W. or S.E. winds"*.

That is to say,

$$\psi - \phi \text{ is } \textit{positive},$$

and most so in the south quadrants. These results are in exact conformity with those obtained from the Makerstoun observations†.

Mr. Clement Ley also finds the difference of direction between the isobars and wind to be greatest in the south quadrants (a result which I had proposed to examine specially, p. 538); but he does not seem to have remarked the important fact of the coincidence in the increase of $\theta-\phi$ and $\psi-\phi$. I recommend Mr. Clement Ley's instructive paper to the attention of meteorologists.

II. "The Meteorology of the Bombay Presidency." By Charles Chambers, F.R.S., Superintendent of the Colába Observatory. Received January 2, 1877.

(Abstract.)

This work consists of four parts—the first dealing with registrations of meteorological phenomena at the Colába Observatory during a period of twenty-seven years; the second with moderately full observations at five military stations in the Bombay Presidency during a period of nineteen years; and the third with large numbers of observations from civil hospitals and revenue stations, being those of selected registers extending over various periods from not less than a fortnight up to a number of years: in this part the phenomena treated are temperature of the air, winds, and rainfall only; and the extent of territory to which the observations refer includes the whole of the Presidency, Sind, and the western

* Journal of the Scottish Meteorological Society, July 1873, p. 70.

† "Results of Makerstoun Observations," Trans. Roy. Soc. Edinb. 1843 to 1846, pp. ciii, civ.

half of Rajputána. In the fourth part are discussed the general distribution (as regards both space and season) of temperature and rainfall, and the variations of the wind; first with respect simply to the physical geography of the country, and then in combination with certain theoretical views, the elucidation of which, by means of the dynamical theory of heat and the kinetic theory of gases, occupies much space.

Nearly half the memoir is devoted to the work of the Colába Observatory, of the history of which a short sketch is given. The design of this part is to give a compendious account of the results of a long and continuous maintenance of the Observatory, both in the shape of numerical determinations of meteorological elements and of their periodical and other variations, and in throwing light by means of these upon the physical conditions and actions which give rise to the observed relations between different phenomena, and to the variation of these relations with time.

In the course of the work the author introduces several new modes of picturing clearly to the mind, and of representing graphically, the general results of the various phenomena observed: he also develops a theory of aerial circulation, including a dynamical theory of convection-currents, which is original, and, so far as he knows, put forth now for the first time.

February 8, 1877.

Dr. GUY, Vice-President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Hindoo Division of the Octave, with some additions to the Theory of the Higher Orders." By R. H. M. Bosanquet, Fellow of St. John's College, Oxford. Communicated by Prof. Henry J. S. Smith, Savilian Professor of Geometry in the University of Oxford. Received January 5, 1877.

(Abstract.)

Attention has been recently directed to the remarkable division of the octave into 22 intervals, employed by the Hindoos. The paper commences with a slight account of the Hindoo scales as thus derived. It is

then remarked that our best way to a real analysis of this music would be to study the system of 22 and compare the results with those actually obtained by Hindoo musicians. The methods which have been employed in the writer's former paper on the subject* are then extended to the higher orders, which have not been before thoroughly discussed. The system of 22 is a system of the second order; and the nature and peculiarities of such systems, and of the system of 22 in particular, are discussed.

A classification of systems of the higher orders according to their mode of forming thirds is advanced. If the system be arranged in successive series of fifths, differing by one unit in pitch, then the system is said to be of class x, if the third of any note is in the series x units below that which contains the note itself.

The system of 22 is shown to be of the second order and first class.

A system of 34, also of the second order and first class, is pointed out as being of considerable excellence, even from a modern practical point of view.

It is shown that in systems of the second order and first class, modulation through a third cannot be regarded as equivalent to modulation through any number of fifths.

The notation is extended to systems of the rth order.

The subject of the transformations of the generalized key-board is then entered upon. It is remarked in the first instance that any form of arrangement whatever can be constructed by rearranging a supply of keys of the ordinary patterns.

The problem of inversion is then solved, and it is shown under what circumstances, by simply inverting the succession from end to end, a key-board can be obtained in which rise corresponds to fall of pitch, and *vice versâ*.

The general transformation of the rth order is then investigated, and a rule is given by which the key-board of the rth order can be arranged with the ordinary keys.

This rule is then applied to the construction of the key-board of the second order, and a diagram is given of a portion of a key-board so arranged. Systems of the second order and first class, such as the systems of 22 and 34 above mentioned, can be controlled with facility by means of this arrangement.

* Proc. Roy. Soc. 1875, vol. xxiii. p. 390, and 'An Elementary Treatise on Musical Intervals and Temperament' (Macmillan, 1876).

II. "On the Transport of Solid and Liquid Particles in Sewer Gases." By E. Frankland, F.R.S. Received January 6, 1877.

The suspension of vast aggregate quantities of solid and liquid particles in our atmosphere is the subject of daily remark. Cloud, fog, and smoke consist of such particles, whilst the observations made by the Astronomer Royal for Scotland, on the Peak of Teneriffe, afford evidence of the occasional existence of abundance of dust in the air even at great altitudes. I have already mentioned, in connexion with some winter observations in the Alps*, that, by placing the eye in shadow and then looking into the sunshine, I repeatedly saw at a distance of a few feet abundance of snow-crystals floating in the air, when the atmosphere was apparently perfectly clear and cloudless.

A very large proportion of the suspended particles in the London atmosphere consists of water and other volatile liquid or solid matter, as was, I conceive, proved by Professor Tyndall's observation, that the heat of boiling water is sufficient to dissipate them. That this is the true explanation of the disappearance of such particles by the application of a moderate degree of heat, and that it is not caused by the rarefied air from the heated body ascending and leaving behind the suspended matter, as suggested by Tyndall†, is, I think, conclusively proved by the following experiments.

Two large glass flasks were filled, the one with atmospheric air, the other with hydrogen. Two pieces of cotton-wool moistened, the one with five drops of strong solution of ammonia and the other with eight drops of strong hydrochloric acid, were plunged into each flask and allowed to remain there for a definite period. The time required for the settlement of the suspended particles of ammonic chloride was then noted and was found to be as follows :—

1. When the pieces of cotton-wool remained in the flasks for two minutes, the ammonic chloride settled down in that filled with air in eighteen minutes, and in that filled with hydrogen in ten minutes.

2. When the pieces of cotton-wool remained in the flasks to the end of the experiment, the settlement in the air-flask required thirty minutes for its completion, whilst that in the hydrogen flask was finished in seventeen minutes.

It is evident from these results that an atmosphere fourteen times as

* Proceedings of the Royal Society, vol. xxii. p. 317.

† Proceedings of the Royal Institution, vol. vi. p. 4. "What is the explanation? Simply this. The hot wire rarefied the air in contact with it, but it did not equally lighten the floating matter. The convection-current of pure air, therefore, passed upwards *among the inert particles*, dragging them after it right and left, but forming between them an impassable black partition. * * * Even when its temperature does not exceed that of boiling water, the wire produces a dark ascending current."

rare as that of London still offers sufficient resistance to the subsidence of minute suspended particles to prevent them from falling more rapidly than one inch per minute, the globular flasks in which the experiments were made being only about 8 inches in diameter. Such particles could not therefore be left behind by an ascending current of the slightly rarefied air produced by an increase of temperature to 100° C.*

In addition to these aqueous and other volatile particles which disappear by a gentle heat, there are also others which consist partly of organic and partly of mineral matters. But the organic seem greatly to preponderate in the air of towns, because such air becomes *apparently* perfectly clear after it has been ignited.

The processes of fermentation, putrefaction, and decay afford abundant evidence that zymotic and other living germs are present amongst the organic portion of the suspended matters; whilst many analyses of rain-water, made by myself and others, show that the salts of sea-water are amongst the mineral constituents floating in the atmosphere.

Of the zymotic matters, those which produce disease in man are obviously of the greatest importance; and it was chiefly with the object of ascertaining the conditions under which these poisons become suspended in the air that I undertook the experiments, the results of which I have now the honour to communicate to the Royal Society.

The outbreak of Asiatic cholera in Southampton in the year 1866 was traced by the late Professor Parkes, F.R.S., to the dispersion of infected sewage through the air. The sewage became infected by the intestinal discharges from some cholera patients who landed from the Peninsular and Oriental Company's steamship 'Poonah.'

In this case the dispersion was produced by the pumping of the infected sewage and its discharge, in a frothy condition, down an open channel 8 or 9 feet long. The effluvium disengaged from this seething stream was described as overpowering, and was bitterly complained of by the inhabitants of the adjacent clean and airy houses, amongst whom a virulent epidemic of Asiatic cholera broke out a few days after the sewage received the infected dejections. Nevertheless the discharge of the frothy liquid was kept up day and night for about a fortnight, and 107 persons perished. At length a closed iron pipe was substituted for the open conduit: from that day the number of cholera cases diminished, and within a week of the protection of the conduit the epidemic was virtually over.

* When this paper was read, Professor Stokes called my attention to the fact that the time of subsidence of solid particles in a gas depends upon the viscosity, and not upon the specific gravity, of the gas. The viscosity of gases is directly as their times of transpiration, and is increased when they are expanded by heat. The time of transpiration of hydrogen is nearly half that of air, and hence suspended matters ought to subside twice as quickly in hydrogen as in air. The slight excess of viscosity of the hydrogen used in these experiments was doubtless due to the almost unavoidable admixture of traces of air, for Graham found the transpiration time of hydrogen to be greatly prolonged by admixture with oxygen.—Feb. 17, 1877.

In this example a potent cause of the suspension of the zymotic poison in the air was obvious; but in the many alleged instances of the propagation of typhoid fever by sewer gases, the condition of dispersion is not so evident. Does the flow of sewage in a properly constructed sewer produce sufficient agitation to disperse liquid particles through the air-space of the sewer? I endeavoured to answer this question by violently agitating a solution of lithic chloride in a glass cylinder 3 inches in diameter and 30 inches high, with a wooden rod, and ascertaining whether the atmosphere at the mouth of the cylinder became impregnated with the liquid, by testing it with the flame of a Bunsen burner; but no trace of lithium could be detected at the mouth of the jar, even after an agitation much in excess of what would ordinarily occur in a sewer. Before making this and the subsequent experiments, it was ascertained that no lithic chloride is carried off by aqueous vapour from a saturated solution of this salt at ordinary temperatures, first, by placing a shallow porcelain basin containing the solution under a bell-jar, and then spectroscopically examining induction-sparks passed through the atmosphere of the bell-jar; secondly, by burning a mixture of coal-gas and air under a ventilating tube beneath the bell-jar in such a way as to cause a circulation of air through the jar and then testing the effluent air for lithium as before; and thirdly, by passing air from a gas-holder over a saturated solution of lithic chloride contained in a Woulfe's bottle, and testing the air as it issued from the bottle.

The results of the experiment in the glass cylinder render it exceedingly improbable that the mere flow of foul liquid through sewers can impregnate the circumambient air with suspended particles.

There is, however, another kind of agitation to which sewage is subject that may produce a very different result: I allude to the development of gases during the processes of fermentation and putrefaction. It is well known that the bursting of minute bubbles of gas at the surface of an effervescing liquid causes the projection of visible liquid particles into the air to the height of several inches. Such visible particles are seen to fall back again immediately into the liquid; but it appeared to me not unlikely that other particles, too minute to be seen, might be simultaneously projected, and, by reason of the smallness of their masses in relation to their sectional areas, might continue suspended in the air for a long time. To ascertain the truth or fallacy of this supposition I made the following experiments.

A quantity of a strong solution of lithic chloride was placed in a shallow basin and acidulated with hydrochloric acid; fragments of white marble were then added, and a paper tube 5 inches in diameter and 5 feet high was placed vertically above the basin. So long as the effervescence continued, abundance of particles of lithium were visible in a Bunsen flame held at the upper end of the tube. A tinplate tube 3 inches in diameter and 12 feet long was now placed in such a position as to bring one of its

open ends over the top of the paper tube. The tin tube was nearly horizontal but slightly inclined upwards from the paper tube, so as to cause a gentle draught of air to pass through it when it was slightly heated externally near its lower extremity. A Bunsen flame placed at the end of this tube furthest away from the effervescing liquid showed that the suspended particles of solution of lithic chloride were not perceptibly less numerous than at the mouth of the paper tube; neither were they much diminished at the further end of the tin tube when the height of the paper tube was increased to $9\frac{1}{2}$ feet. There can, I think, be little doubt that these particles, which had thus been carried along by a gentle current of air for a distance of 21 feet, would be similarly conveyed to very much greater distances.

In some of my earlier experiments I had noticed that the suspended particles in a current of air were diminished in number, or sometimes altogether removed, when the current had to pass a right-angled bend in a tube; and it therefore appeared to be not unlikely that a stratum of small fragments of charcoal would arrest them. This surmise, however, did not prove to be correct; for the particles of lithic chloride solution suspended in air, when the latter was moving very slowly, passed easily through a stratum 2 inches thick, composed of fragments of charcoal varying in size from $\frac{1}{4}$ to 1 cubic inch; and even when the thickness of the stratum was increased to 5 inches, the particles still came through although in greatly diminished numbers.

The following conclusions as to the behaviour of flowing sewage may be drawn from these experiments:—

1. The moderate agitation of a liquid does not cause the suspension of liquid particles capable of transport by the circumambient air; and therefore the flow of fresh sewage through a properly constructed sewer is not likely to be attended by the suspension of zymotic matters in the air of the sewer.

2. The breaking of minute gas-bubbles on the surface of a liquid consequent upon the generation of gas within the body of the liquid is a potent cause of the suspension of transportable liquid particles in the surrounding air; and therefore when, through the stagnation of sewage or constructive defects which allow of the retention of excrementitious matters for several days in the sewer, putrefaction sets in and causes the generation of gases, the suspension of zymotic matters in the air of the sewer is extremely likely to occur.

3. It is therefore of the greatest importance to the health of towns, villages, and even isolated houses, that foul liquids should pass freely and quickly through sewers and drain-pipes, so as to secure their discharge from the sewerage system before putrefaction sets in.

III. "Researches in Spectrum-Analysis in connexion with the Spectrum of the Sun.—No. V." By J. NORMAN LOCKYER, F.R.S. Received January 10, 1877.

(Abstract.)

The author submits to the Royal Society the first portion of a new map of the solar spectrum, w.l. 39–40 ten millionths, constructed after the manner described in a previous "Preliminary Note."

February 15, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The President read the following letter:—

To the President of the Royal Society, London.

We have the honour to inform you of the establishment of a Scientific Club, under the presidency of His Excellency Dr. A. Ritter von Schmerling, and to request you to do us the favour of communicating the fact to the Fellows of the Royal Society.

We beg to invite the Fellows of the Royal Society to make use of this Club during their occasional stay in Vienna, either as guests or as foreign members.

We have the honour to subscribe ourselves, on behalf of the Club,

Yours obediently,

The Vice-Presidents { HOFRATH VON HAUER, *Director of the Imperial Geological Institute.* HOFRATH BRUNNER VON WATTENWYL.

DÖBLHOFF, 1. *Secretary.*

Vienna, Feb. 1877.

Club and Office:
1. Eschenbach-Gasse, No. 9, 1st floor.

The following Papers were read:—

1. "On Stratified Discharges.—III. On a Rapid Contact-Breaker, and the Phenomena of the Flow." By WILLIAM SPOTTISWOODE, M.A., F.R.S. Received January 9, 1877.

In a paper published in the Proceedings of the Royal Society, vol. xxiii. p. 455, I have described a form of contact-breaker designed for great rapidity and steadiness of action. It consisted of a steel rod which vibrated under the action of an electromagnet. As regards sharpness of break and steadiness of definition in the striæ, this instrument left little or nothing to be desired. But, as explained in the paper above quoted, an alteration in the current not only affected the steadiness directly, but also reacted on the break itself. The effects due to an alteration of the current alone thereby became masked, and the study of the laws relating to such changes was rendered more difficult, or altogether impracticable. In order to obviate this inconvenience I devised another form of contact-breaker, in which the vibrating rod and electromagnet were replaced by an arrangement purely mechanical in its action, and therefore entirely under control.

This instrument consists essentially of a wheel platinized at the edge, on which a platinum spring rests. In the circumference of the wheel a number (40 in the first instance) of slots were cut, and filled with ebonite plugs so as to interrupt the current. The breadth of the slots was about ·04 inch, and that of the teeth about ·5 inch. The wheel was connected with suitable driving gear, so as to give from 250 to 2000 currents from the coil in each direction per second. A 4-inch coil was found sufficient to produce the effects; but the 18-inch coil by Apps, mentioned in former communications, was preferable. With the wheel, as with the electromagnetic break, a very slight strength of current was required; but, on the other hand, high tension in the primary was found necessary. In many of the experiments accordingly from 10 to 20 of the smallest Leclanché cells usually made were employed with the small, and from 20 to 50 with the large coil. But these were afterwards replaced by a double fluid battery suggested by my assistant, Mr. P. Ward, and described at the end of this communication.

For some time the experiments were conducted with the platinum spring resting on the wheel; and the effects were varied by altering either the pressure of the spring or the velocity of the wheel; but the gradual abrasion of the platinum through friction proved to be a fruitful source of irregular results. This irregularity of action, at all times difficult to compensate, and sometimes insuperable, was fortunately removed by a simple although delicate adjustment. It was, in fact, found that actual metallic contact between the spring and wheel was not necessary, provided that a layer or cushion of conducting material were interposed. Such a layer was formed by a thin film of liquid drawn out by a thread leading from a reservoir and resting on the wheel. Various fluids were

tried; but the simplest, and on the whole the best, proved to be dilute sulphuric acid, in the proportion of 1 drop of acid to 6 drams of water. Generally speaking the better conductor the fluid is, the better are the results obtained; but, owing to the insulating slots being very narrow in this instance, a comparatively weak mixture of acid and water was necessary. In one wheel, where the insulating slots are $\frac{1}{4}$ in. wide, a mixture 36 times as strong may as advantageously be used. The spring, which under these circumstances became unnecessary, was replaced by a point, the adjustment of whose distance from the wheel was simpler and more accurate. This arrangement gave excellent results, even when the number of currents per second was reduced in some cases to 250; added to which the unpleasant and disturbing noise of the friction was entirely avoided.

Wheels having different numbers of teeth were also constructed, and (what was perhaps of more importance) having teeth of different breadths, so as to give with the same velocity of rotation contacts of different duration. The breadth of the ebonite plugs, or length of interruption of the current, was immaterial, so long as the current was efficiently broken. It did not appear, however, that with the same tube more could be obtained with wheels having different numbers of teeth, than with the same wheel at different speeds. But it was found that for different tubes different wheels occasionally gave better results.

With the contact-breaker here described effects similar to those produced by the rapidly vibrating break were obtained. The striæ were formed in a regular manner, and advanced or receded, or remained at rest, in a column usually unbroken, so long as the velocity of the wheel was maintained without change; and even in the longer tubes, where the striæ, of the double discharge, advanced or receded towards both ends at the same time, and appeared sometimes compressed and at others dilated, the phenomena always maintained their characteristic features.

The condition of the striæ here described, whether flowing or stationary, may be comprised under the general term "steady;" and when there is no motion in either direction, they may be specifically denominated as "stationary."

Two questions here presented themselves:—First, what are the conditions necessary for the production of steady striæ? Secondly, what are the conditions and circumstances of the advance and retreat, in other words, of the flow of steady striæ?

With a view of ascertaining the nature of the distinction between the ordinary and the steady striæ, careful observations were made with the revolving mirror. It having been noticed that when the wheel break moved slowly ordinary or irregular striæ were produced, and that when it moved rapidly steady striæ resulted, it seemed probable that the latter effect might be due to the short time of contact, and to the consequent absence of many of the features described in Part II. of these researches.

This is, in fact, identical with the suggestion there made, that the fluttering appearance was due to the unequal duration of the striæ themselves, and to the irregular positions of the points at which they were renewed at successive discharges of the coil. And such, in fact, proved to be the case; for as the speed of the wheel was increased the duration of the discharges diminished; the image as seen in the mirror became narrower and simpler in its configuration, until, when the steady effect was produced, each discharge showed only a single column of striæ of a width proportional to the apparent width of the slit. The proper motion, implied by the inclination of the individual striæ to the vertical, was still perceptible, and was directed, as usual, towards the negative pole.

From a comparison of the number of striæ as seen by the eye with those seen in the revolving mirror, it was found that the striæ so formed were of the kind called "simple" in former communications. And the phenomena of the flow may therefore be considered to be due to the different positions taken up by the striæ in successive discharges. If in each discharge the striæ occupy positions in advance of those occupied in a previous discharge, the column will appear to advance; if the reverse be the case, they will appear to recede. If the positions remain unchanged, the column will appear stationary.

The following consequence of this explanation of the flow will readily occur to the reader, viz. that the rapidity of the flow will increase with the extent of advance made by the striæ in each successive discharge, until that advance amounts to half the distance between two contiguous striæ. Before this is attained the flow will have become too rapid to be followed by the unassisted eye, and can only be observed by the aid of the mirror. When this rate of advance has been exceeded, the flow will appear to be reversed. If the rate of advance still continues to increase, the rapidity in the reverse direction will appear to decrease until the advance amounts to the entire distance between two contiguous striæ, when it will apparently be reduced to zero; the striæ will then again appear stationary. Experiments appear to confirm this view of the case.

Experiments were next instituted with a view of ascertaining the connexion between the flow and resistance. Starting from a condition of current and break for which the striæ were stationary, it was found that an increase of resistance, introduced generally in the primary circuit, produced a forward flow, *i. e.* from the positive towards the negative terminal, while under similar circumstances a decrease of resistance produced a backward flow. Furthermore, if after producing a forward flow the resistance be continually increased, the flow after increasing in rapidity so as to become indistinguishable by the unassisted eye, gradually appears to become slower, and ultimately to reverse itself, in accordance with the law suggested above.

Another form of contact-breaker was also occasionally used. The principle upon which it was based was the sudden disruption of a thin

film of conducting liquid by a discharge between the electrodes of a circuit. The mode of effecting this was to make one electrode terminate in a platinum plate fixed in a horizontal position, and supplied with a uniform film of dilute sulphuric acid; the other in a platinum point, the distance of which from the plate is capable of delicate adjustment by means of a screw. Electromotive force required for this break is not less than that of 5 cells of Grove.

As soon as the current passes, the fluid between the plate and point will be decomposed and electrical continuity broken. This done, the fluid flows back again, and continuity is restored. By a proper adjustment of the supply of fluid and of the distance of the electrodes (the latter varying from ·05 inch to ·001 inch), the number of disruptions may be made to attain 1000 per second.

The currents delivered by this form of break are exceedingly uniform, and the effects produced are quite equal in delicacy to those produced by the electromagnetic or by the wheel break.

The elements used in the battery to which allusion was made in the early part of this paper are zinc and carbon. The zinc is immersed in dilute sulphuric acid in the proportions of 1 volume of acid to 7 of water; and the carbon in a saturated solution of bichromate of potash with 1 volume of sulphuric acid to 7 of the solution. The carbon and bichromate solution are held in a porous cell.

The absence of nitric acid permits this battery to be used in a room; while the fact that the zinc is attacked only when the circuit is complete, renders it unnecessary to lift the plates out of the fluid when not in use, as in the bichromate battery. The only limit to the time during which this battery may be left untouched, appears to be the period when the bichromate salt finds its way into the outer cell, so as to attack the zinc independently of electrical action. But this does not take place to an extent materially to affect the action for some months.

II. "Lymphatics and their Origin in Muscular Tissues." By George Hoggan, M.B., and Frances Elizabeth Hoggan, M.D. Communicated by Dr. Billing, F.R.S. Received January 18, 1877.

The authors announce that they have discovered the long-looked-for lymphatics of striated muscle, and describe them as radicles, valveless reservoirs, and valved efferent vessels. While describing their structure and relations, they point out that the reservoirs are found on one plane or side of a muscle; the valved efferents are found on the other side, as, for example, in the case of the diaphragm, transversalis abdominis, and triangularis sterni muscles. In connexion with this, they have discovered

a dense plexus of valved vessels on the anterior surface of the abdominal wall, corresponding to that on the pleural surface of the diaphragm.

Upon the lymphatics of muscle they find the peculiar serous cells first described by Ludwig and Schweigger Seidel, whose views they fully confirm, in opposition to those expressed by Ranvier. They deny the existence of stomata in the Mammalia, but admit it in the case of frogs; and as the peritoneum of the latter is lined by crenated lymphatic endothelium, they admit its connexion with the lymphatic system; but, on account of the absence of the latter endothelium as well as stomata from the serous cavities of mammals, they deny any connexion between these and the lymphatics. While describing the structure of basement membrane, they discuss the facts adduced by Klein and Debove as bearing on the question of absorption, and give their own views on this question. They hold that the lower surface of the diaphragm is an exuding one, and only an absorbent one when all the natural conditions are reversed.

They describe the minute anatomy of the lymphatics of the intestine, and show that it is the glandular structures, and not the muscles of the wall, that regulate the amount of these vessels. They also trace complete identity between these and the lymphatics of striated muscle. In either case they figure the connective-tissue cavities as forming the radicles of the lymphatics, but hold that these are not the only lymphatic afferents, nor that that is their only function. To prove this, they discuss the nature of these cavities, as they have discovered them in tendon and other gelatinous structures in different classes of animals to be of the same structure as in the cornea. Unlike man, the small mammals have no special vascular or lymphatic vessels in the peritoneal tissue, being dependent on the muscles below for those structures.

The authors finish by entering upon a minute description of the *technique* employed by them, and offer a series of about 60 camera-lucida drawings of preparations in their possession in illustration of their researches.

III. "Remarks chiefly on $487^2 \equiv 486$." By William Shanks. Communicated by the Rev. G. Salmon, D.D., F.R.S. Received November 29, 1876.

In the cases of $3, 3^2 \equiv 1$, also of $487, 487^2 \equiv 486$, we are unable to show why the Period itself is, in each case, divisible by the Prime. But we can show, with little labour, that the period arising from $\frac{1}{487}$ is itself divisible by 487, and therefore that $\frac{1}{487^2} \equiv 486$.

The number composed of 486 9s is divisible by 487. Now this number

is made up of the two factors 243 9s and $\overset{242\ 0s}{100000..0001}$. The latter only is divisible by 487. The latter, moreover, admits of the factor $\overset{80\ 0s}{1000..001}$ being thrown out, as 487 is not exactly contained in it. We thus have the number $\overset{81\ 9s}{999..9}\overset{80\ 0s}{9000..0001}$, which may be shown to be divisible by 487^2, as follows :—

$$\text{Since } \frac{10^{81}}{487} \equiv 233, \therefore \frac{10^{81}-1}{487} \equiv 232; \text{ hence } \frac{(10^{81})(10^{81}-1)+1}{487} \equiv 0.$$

Therefore $\frac{1}{487} \equiv 486$.

$$\text{Again, } \frac{10^{10}}{487^2} \equiv 6284, \frac{10^{20}}{487^2} \equiv 118602, \frac{10^{40}}{487^2} \equiv -58986, \frac{10^{81}}{487^2} \equiv 78153;$$

$$\therefore \frac{10^{81}-1}{487^2} \equiv 78152. \text{ Hence } \frac{(10^{81})(10^{81}-1)+1}{487^2} \equiv 0, \text{ that is } \frac{1}{487^2} \equiv 486.$$

We may show that $\frac{1}{487^2} \equiv 486$ in another way, thus :—

Taking, as before, the numbers $\overset{81\ 9s}{99..9}\overset{80\ 0s}{9000..001}$, and dividing the nines by 487, we have remainder 232.

Hence, *after* 232, by dividing by 487, the quotient is 232 times the first half $+111 \left(\frac{(232 \times 233)+1}{487} = 111\right)$.

Divide first half of quotient by 487, and we have remainder 160.

Hence $\frac{160 \times 233}{487} \equiv 268$.

Remainder from *after* 160 is got from $\frac{160 \times 232 + 111}{487} \equiv 219$; hence $\frac{268+219}{487} \equiv 0$. In other words, the quotient obtained from dividing the large number (given above), consisting of 162 digits, by 487 is itself divisible by 487.

It is observable that 486 is an aliquot part of 487^2-1; generally, that P^2-1 is divisible by $P+1$.

The Prime $69499 \equiv 486$; but, as is usual, $69499^2 \equiv 486 . 69499$. Hence the number $\overset{81\ 9s}{999..9}\overset{80\ 0s}{000..01}$ is divisible by 69499. We append the results of these divisions, as being somewhat curious, the last result being prime or otherwise.

487. 20533 88090 34907 59753 59342 91581 10882 95687 88501 02669
40451 74537 98767 96714 57905 54414 76386 03696 09856 26283
36755 64681 72484 59958 93223 81930 18480 49281 31416 83778
23408 62423.

69499. 42 16402 64958 74250 00737 87046 36777 99375 12912 73311
43614 89064 75972 82950 13260 58633 29520 29987 05564 38657
66605 24773 47376 76509 15591 83535 79093 38910 22857 11876
34134 30929.

60 66853 69514 29876 69949 02151 63927 52953 46570
07023 75019 62711 34797 37766 20182 42900 32259 88844 52386
92150 48569 40061 68976 19403 38122 61378 99960 27151 79868
94597 53571.

February 22, 1877.

Dr. J. DALTON HOOKER, C.B., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

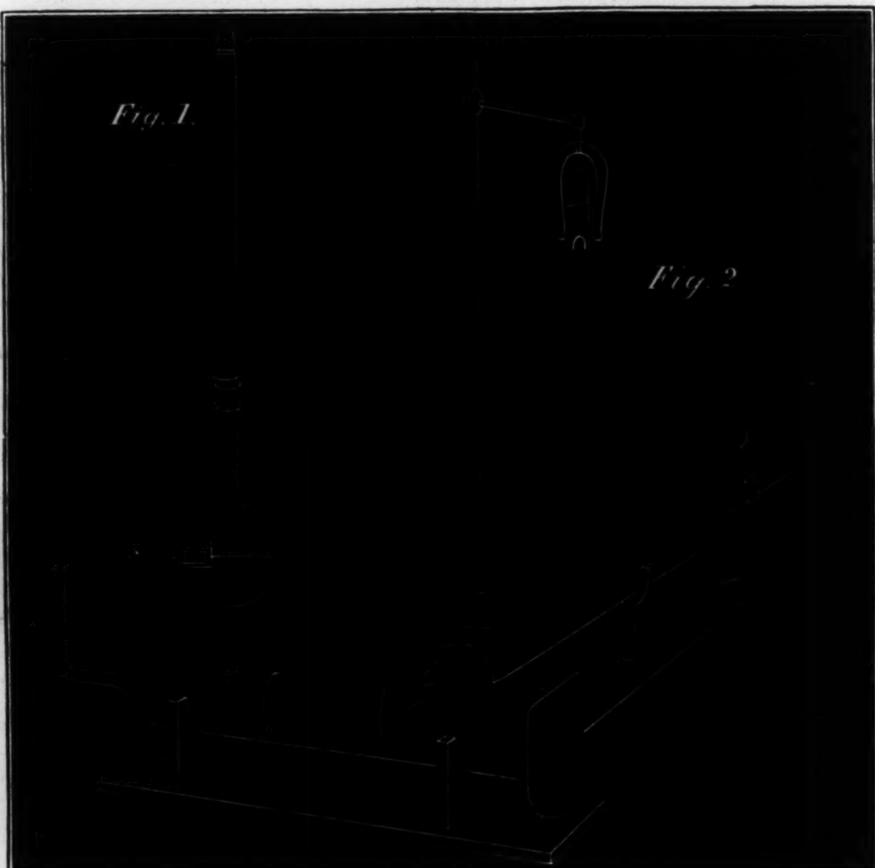
The following Papers were read:—

I. "On Crookes's Force." By G. JOHNSTONE STONEY, M.A., F.R.S., and RICHARD J. MOSS, F.C.S. Received January 12, 1877.

In two papers by one of the authors of the present communication, which appeared in the Philosophical Magazine for March and April 1876, it has been shown that the motion of the blackened disks of a Crookes's radiometer can be explained by the known dynamical properties of the trace of gas which is present, and the term "Crookes's force" is proposed to designate the reaction which comes into play between the blackened disks and the walls of the exhausted chamber when a difference of temperature exists between them. Shortly after the first of these papers appeared we commenced an experimental investigation of the subject with the view of learning, if possible, the laws to which the force conforms. The investigation is still in progress, and, being exceedingly tedious, it will require a great expenditure of time before it is completed; we propose, however, in this preliminary paper to describe the apparatus and methods of observation employed, and to give some of the results already obtained.

If the pressure which is exerted on the blackened pith surfaces reacts on the sides of the glass envelope, it follows that a transparent disk delicately suspended close to a stationary disk of blackened pith ought to move away from the pith, and therefore towards the light, when the

pith is illuminated. This inference was submitted to the test of experiment by means of an apparatus represented in fig. 1 and constructed as follows:—A piece of elder-pith 2·5 centims. in length and 1·2 centim. in breadth, blackened on one side, was fastened by one end to the interior surface of the bulb of an ordinary boiling-flask (of about 200 cub. centims. capacity) in such a manner that the free end of the pith extended towards the middle of the bulb. A light glass rod with a small magnet on one end, and a disk of thin microscope-glass on the other end, was so sus-



pended in the bulb that the glass disk could be readily balanced in a position nearly parallel with the surface of the blackened pith, and a few millims. distant from it. The silk fibre from which the glass rod was suspended hung from a fixed arm at the upper end of a tube, the lower end of which was hermetically fastened into the neck of the flask. An elongation of this tube (not shown in the figure) with a contraction for sealing, served to connect the apparatus with the exhaust-tube of a Sprengel pump. The pump was set in action, and occasionally the flame of an ordinary gas-burner was held at a distance of about 10 centims. from the blackened pith, while the microscope-glass was closely watched.

When the gauge of the pump showed a tension of 7 millims., as compared with the mercurial column of a barometer standing in the same vessel of mercury, the glass disk was distinctly repelled from the pith and towards the source of light. As the exhaustion was continued the repulsion between the pith and the glass increased. The apparatus was sealed off from the pump when the mercury falling in the exhaust tube had for some days produced a metallic sound. Feeble illumination now caused the glass disk to be forcibly driven away from the pith*.

We now endeavoured to determine quantitatively the influence of variations in the tension of the residual gas, and also the influence of variations in distance between the reacting surfaces. For this purpose we constructed the apparatus represented in fig. 2.

On a wooden stand supported by three levelling-screws rests a glass tube 20 centims. in length and 3·8 centims. in diameter, having a tubular opening at one side, into which is cemented horizontally a smaller tube 1·5 centim. in diameter. In the larger tube there is a circular disk of elder-pith 2·3 centims. in diameter, having one side blackened with lampblack; it is supported in a vertical position on a movable stand of iron wire. By means of a magnet the pith disk can be moved up and down the tube, and thus placed at any required distance (within 12 centims.) from a delicately suspended circular disk of thin microscope-glass, 3 centims. in diameter and 0·3 millim. in thickness. The glass disk is attached to the end of a glass arm, which is suspended in the smaller tube by means of a silk cocoon fibre contained in a vertical limb 38 centims. in length and 9 millims. in diameter. In order that the torsion of the silk fibre may be conveniently regulated, there is a small ∩-shaped piece of iron wire attached to it a few centimetres below the end from which it hangs. A horseshoe magnet is suspended outside the tube with the piece of wire between its poles. By turning the magnet round torsion may be imparted to the silk fibre. The balance of the glass arm is adjusted by means of a small iron ring which it carries; the position of the ring can be altered at will by an external magnet. There is a small silvered mirror attached to the arm at the point of suspension; this reflects the image of a narrow illuminated slit on to a scale divided into degrees 2·5 millims. each. An alteration in the position of the index amounting to 0·5 millim. is readily observed; this corresponds with a change in the position of the outer edge of the glass disk amounting to 0·033 millim. One end of the large tube is ground perfectly flat and closed by cementing to it a plate of glass 4 millims. in thickness; through this light is admitted to the pith disk by an arrangement to be presently referred to. The other end of the large tube is contracted and terminates in a narrow tube bent upwards, partly packed with gold leaf (to intercept mercury vapour), and attached to the exhaust-tube of a

* The apparatus was sealed off on the 14th of April, 1876. The experiments described above were made in March.

Sprengel pump. The smaller tube terminates in a contraction bearing a stopcock which serves for admitting the gases to be experimented upon.

We found it necessary to avoid the irregular actions which arose when the incident light was allowed to shine on the inside of the glass tube. This was accomplished by projecting on the disk the image of a uniformly illuminated circular aperture in a screen of copper foil placed outside the glass chimney of an Argand gas-burner. The lens employed for this purpose is permanently attached to a stand on which the lamp is secured. When the position of the pith disk is altered, the position of the stand carrying the lamp and lens is altered to the same extent, so that the pith disk is always in focus. The burner is automatically supplied with coal-gas at the uniform rate of 3·2 cubic feet per hour, this being the quantity that gives a flame of the required size.

We found that the torsion of a cocoon fibre furnishes a force which is too variable to admit of its being delicately controlled by the method just referred to; but a very accurate adjustment was secured by a supplementary arrangement. It has already been mentioned that the arm which bears the thin glass disk carries a small iron weight by which its balance is regulated. This weight was made to serve for balancing the torsion of the silk fibre. For this purpose a small bar magnet sliding in a groove is so placed that one pole acts on the weight. With a little care the distance of the magnet from the weight can be adjusted so as to bring the index to zero, and thus exactly counterbalance the torsion of the silk, the index remaining practically stationary. In this condition the apparatus is sensitive to an extreme degree.

It will be observed that in this apparatus the cooler of the heat-engine consists of the swinging disk along with that part of the containing tube which lies between the swinging disk and the disk of blackened pith. By thus making a portion of the cooler freely movable, we hoped to be able to ascertain the thickness of the layer of gas within which Crookes's force exists. It would not have answered for this part of our investigation to have made the heater the part freely movable, as in all apparatus of the kind that had been previously constructed, because the heater cannot be placed far from the cooler in apparatus that is not inconveniently large for the Sprengel pump, since when the containing tube is of any moderate size its sides become the principal part of the cooler* when the glass disk is at a distance.

* It is obvious, from the dynamical theory, that if the molecules tending in one direction within a stationary gas are at one temperature, while the rest of the molecules of the gas and the surface of a solid with which they come in contact are at another temperature, then the Crookes's force which arises may be either normal to that surface like the pressure of a gas, or tangential to it like friction, or in any way compounded of these two, being in each case in the direction spoken of above.

Accordingly the forces that act upon the containing vessel and the vanes of radiometers are in general partly tangential and partly normal; so that in estimating the

With the improved apparatus repulsion at first appeared to exist at all tensions in hydrogen, the distance between the disks being 1 millim. This repulsion was not perceptibly increased by alterations in tension, until the latter was reduced to about 200 millims., when a slight increase of repulsion took place, and further reductions of tensions were in each case followed by an increase of repulsion.

It was observed that when repulsion was detected at ordinary tensions the glass disk occupied the upper portion of the containing tube, and that when the disk was made to swing in the lower portion of the tube, instead of being repelled it was attracted towards the pith when illuminated. These results are obviously due to convection-currents. By carefully balancing the glass disk as nearly as possible in the centre of the tube the effects are greatly reduced; but it would be very difficult, if not impossible, to balance the disk in a perfectly neutral position.

In an atmosphere of hydrogen at ordinary atmospheric tensions there is no indication of attraction or repulsion when the distance between the pith and glass disks is 100 millims. and the time of illumination 15 seconds, a period which experience has led us to adopt. The first unmistakable indications of pressure on the swinging disk at this distance occur when the tension is about 50 millims., at which tension there is a very feeble repulsion. As the tension is reduced the repulsion increases. An extensive series of experiments have been made for the purpose of determining the ratio in which the repulsion increases for given reductions in the tension of the residual gas. The accompanying Table exhibits the results of one set of experiments:—

T = tension of the residual gas.

D = distance between the disks in millims.

P = pressure on the swinging disk, as represented by the space, in degrees of the scale, through which the index passes in 15 seconds.

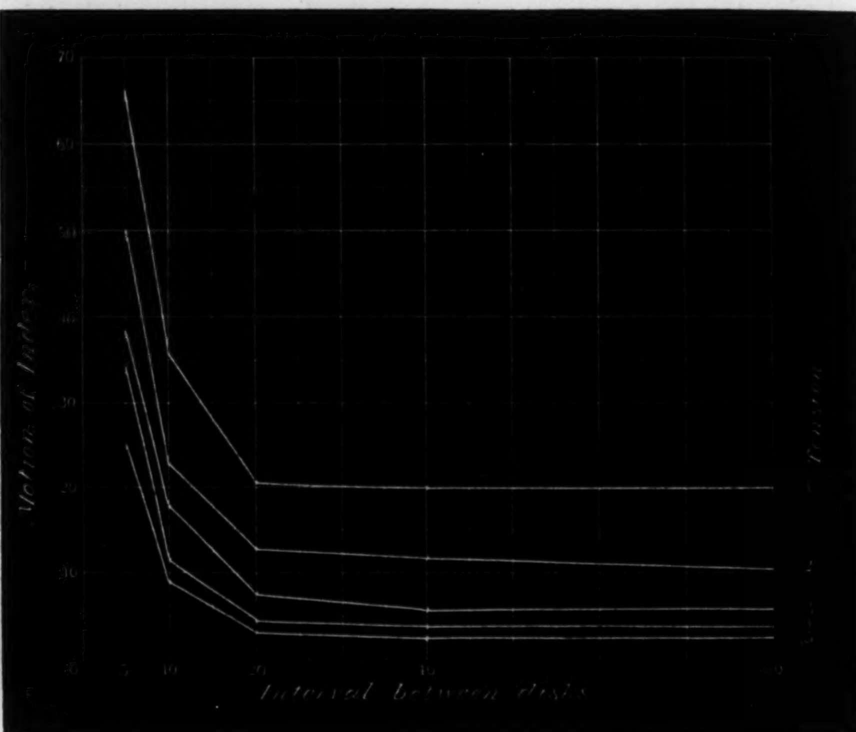
T.	D.	P.
50	100	$\frac{1}{4}$
20	,,	$\frac{1}{2}$
10	,,	$1\frac{1}{8}$
5	,,	$2\frac{1}{2}$
4	,,	3
3	,,	4
2	,,	5
1	,,	$8\frac{1}{2}$

---

intensity of these forces the matters to be considered along with the directions of the motions in the intervening gas, are the proximity and extent of the opposed surfaces, and not the degree of their parallelism. It is probable that all the phenomena of radiometers with crumpled or inclined vanes, which Mr. Crookes has lately investigated, admit of explanation by these considerations.

It is manifest that in the apparatus described in the text the chief part of the reaction on the containing vessel, when the disks are at a considerable distance asunder, consists of tangential forces acting on the inside of the tube.

In the above experiments it will be observed that the distance between the pith disk and the swinging glass disk was constant, viz. 100 millims. We have made several series of experiments with the disks at various distances in atmospheres of various tensions. The means of several of these series are graphically represented in the following diagram. The ordinates represent the distance in millimetres through which the index moved in a period of 15 seconds; the abscissas represent the distance in millimetres between the disks.



Our expectation of being able to ascertain the thickness of the Crookes's layer has not been entirely fulfilled with the apparatus as at present constructed, owing to the presence of the exceedingly feeble force that is represented by the horizontal portions of the diagram. We have found this feeble force nearly constant at each tension when the two disks are at any distances asunder exceeding 20 millims. So far as we have been able to ascertain, it seems to arise partly from the sudden expansion of the gas throughout the extent of the Crookes's layer as soon as the light is turned on, an expansion which acts on the swinging disk as if a feeble explosion had taken place in front of it; and partly from a Crookes's force acting between the upperside of the tube and the swinging disk, caused by the inside of the tube becoming sensibly heated by the convection-current that commences as soon as the Crookes's layer is established. We do not think that any appreciable part of it is due to the *direct* action of the convection-current.

The rest of the diagram represents the forces which we found when the disks were at distances of 10 and 5 millims. asunder. The forces which presented themselves at these distances are to be attributed mainly to a true Crookes's reaction between the disks; and they seem to warrant the conclusion that Crookes's reaction was manifested at a distance of at least 10 millims. in a hydrogen vacuum, when the outstanding tension was as much as 5 millims. of mercury.

At distances of from 20 to 80 millims. the very feeble force acting on the glass disk in our apparatus seemed to vary about inversely as the tension. As already mentioned, it appeared to be nearly independent of the distance when the distance exceeded 20 millims.

At distances of 5, 10, and 20 millims. the force on the swinging disk made some approach to varying at each tension inversely as the distance. But, so far as may be judged from our measures of such exceedingly feeble forces, there is a sensible deviation from this law at most of the tensions.

Moreover the diagram, taken as a whole, seems to suggest, in conformity with the dynamical theory, that the law changes with variations of density. For if the law were the same at all the observed tensions, the converging lines in the diagram should converge to points in the axis of abscissas, whereas they converge towards points lower down.

We will postpone the further discussion of the observations already made with this apparatus until we can supplement them by others.

II. "On the Structure of *Magelona*." By W. C. M'Intosh, M.D., F.R.S.E., F.L.S. Communicated by G. Busk, F.R.S., V.P.L.S. and Z.S. Received January 23, 1877.

(Abstract.)

This annelid was first discriminated * by Dr. George Johnston, of Berwick; but as his description (under the name *Mœa mirabilis*) was not published till 1865, the above-mentioned title, given to the same type, from the Island of St. Catherine, off the coast of Brazil, by Dr. Fritz Müller, has the priority. It is a comparatively small form, its slender body being divided into two well-marked regions; while anteriorly two long papillose tentacles are attached to the base of a remarkable spathulate, eyeless snout, which it dextrously uses to perforate sand near low-water mark at St. Andrews and other sandy shores on both east and west coasts.

The structure may be examined under the following heads:—

Cuticle.—This chitinous transparent layer is densest on the snout and anterior region of the body, both being much exposed in the boring-operations. Throughout the rest of the body it attains its maximum thickness over the nerve-cords in the ventral median line. No cilia occur

* From specimens procured by the distinguished botanist, Dr. Greville.

anywhere on its surface, and only faint indications of pores exist in certain regions. Fine motionless palpocils abound all over the cuticle.

The *Hypoderm* forms a very large proportion of the tissues of the flattened snout, the whole region outside the four muscular compartments being occupied by it; so that, in transverse section, it assumes on each side the shape of a long lanceolate process, which much resembles a leaf with its midrib and veins. Throughout the anterior and posterior regions of the body it forms a complete sheath, with various thickenings, and at the tip of the tail ends in two lateral styles, the glandular tissue of which (as in the dorsal and ventral processes) is arranged in a very regular manner. In minute structure the hypoderm much resembles the Nemertian cutis, presenting under pressure in the fresh animal a series of flask-shaped glands or cells, from which the contents escape as clear or granular globules. Moreover, it contains a vast number of bacillary cells, some of which have pigment and a large clear globule. In the hypoderm lie the nervous system and neural canals.

Muscular System.—The four longitudinal muscles of the spathulate snout are arranged within a curious framework of chitinous basement-tissue, which in section assumes various shapes—in front being like a pair of spectacles, then a figure of eight, and for a considerable distance very much resembling a crown. This framework exercises an important influence on the functions of the part. The central pair of muscles are confined to the snout; the lateral pass behind the mouth to constitute the ventral longitudinal pair. In the preoral chamber are a strong transverse muscle (acting as the chief approximator of the sides) and a vertical muscle. The muscles of the body-wall (besides the pair mentioned) are circular, longitudinal dorsal, vertical, oblique, external or lateral vertical and transverse ventral. Anteriorly all are powerfully developed for the peculiar functions of the region, viz. the compression of the blood-channels and the thrusting out of the proboscis. The muscles of the ninth body-segment are modified so as to form great constrictors, which have a slightly spiral arrangement. In addition to those of the body-wall, anteriorly, are the long and short retractor muscles of the proboscis, and various bands acting on the buccal and pharyngeal regions. In the posterior division of the body the transverse ventral muscles become atrophied; but the dorsal and ventral longitudinal muscles, though constricted at the ninth segment, extend throughout; and the other muscles of the body-wall are likewise present.

Digestive System.—A T-shaped slit leads into the buccal region, then follow pharynx, œsophagus, ventricular division, and intestine; while to the junction of the first and second is attached the proboscis. The pharyngeal division is furnished with complex muscular layers and convoluted internal surface, and it is thrust into the base of the proboscis in full expulsion. It is probably the homologue of the *proventriculus* in such Annelids as the Syllidæ. The proboscis, again, is an instance of the separa-

tion and modification of a part of the digestive canal to aid in the ceaseless perforations in the sand. Its internal surface is covered by a thick, transparent chitinous layer, devoid of pores. The relaxation of its own retractors, and the contraction of the muscular anterior region of the body, cause it to yield readily to a powerful stream of blood sent from behind; and it smoothly unrolls from the margin of the lower lip like a very supple membrane. This extrusion goes on until the brownish mass of the pharyngeal region approaches the front of the first body-segment, when its muscular coil slips into the base of the proboscis, like a plug, assisting to retain the blood therein, and giving firmness to the whole organ. Thus, in its progress forward, the flattened snout of the annelid is thrust amongst the fine sand which it haunts (with an undulating and insinuating motion) till it has advanced about its own length; then the proboscis is ejected to its full extent like an india-rubber dilator, so as to make a suitable channel for the occupation of the body, while again pressing onward the exploratory snout. All the retractile arrangements are next brought into play; the fan-shaped vertical muscular fibres pull in the last extruded region, the short and long retractors act on the entire organ, and the withdrawal of the pharyngeal protrusion makes an open channel for the backward stream of blood, which rushes into the vessels of the anterior region of the body out of the returning organ, further constricted by its own circular muscular coat. There is no differentiation between the succeeding œsophageal and ventricular regions, the glandular internal tunic in each being alike. The latter ceases, after a marked constriction, at the beginning of the tenth body-segment; and thereafter the intestine, which has much more lax glandular tissue and abundant fatty globules, proceeds to the dorsal anus near the tip of the tail. The walls of this region are richly furnished with capillaries; and cilia are very evident on the internal surface near the tail.

Circulatory System.—An interesting feature is the fact that the blood is a densely corpusculated fluid, the corpuscles having a pinkish colour. There are two large dorsal vessels which arise, near the tip of the tail, from the bifurcation of the ventral trunk. They pass forward along the dorsal arch of the alimentary canal, receiving in each segment a large branch from the ventral trunk and numerous capillaries from the intestinal wall, until the posterior border of the tenth segment is reached. At this part their dilated walls are supplied with powerful muscles, which, on the relaxation of the great muscles of the ninth segment, enable them to perform the functions of contractile chambers or "hearts," and by vigorous systole send the blood forward in a swift stream along the single dorsal vessel of the anterior region. On arriving at the base of the snout the vessel ends in the efferent branch to the tentacle on each side. The current rushes along the latter (nearly at right angles to the dorsal trunk) to the tips, sending off in each a web of circumferential capillaries throughout the greater part of its length, and terminating in the afferent

vessel, which proceeds backward, collecting, as it goes, the capillary streams, and then ends by turning forward at the base of the snout as the efferent cephalic vessel. The latter has no evident capillaries, but bends round at the tip of the flattened organ to terminate in the afferent cephalic vessel. A curious change takes place in the majority of those *Magelonæ* which are provided with the convoluted lateral organs of the body, mentioned further on, in autumn. The cephalic vessels are much abbreviated, and the direction of the current at the base of the snout is somewhat modified. The blood from the head and anterior region collects into a series of large vascular meshes which occur in the anterior region of the body, and in which the current is for the most part under the control of the greatly developed muscles of the body-wall. Thus it happens, as formerly indicated, that the contraction of the latter, and of the special muscular apparatus which closes the communication with the posterior region at the ninth segment, drives the blood forward to unroll the proboscis. This muscular arrangement in the anterior region and the muscular walls of the vessels themselves at the posterior part of the same division of the body send the current through the relaxed barrier at the ninth segment into the muscular ventral blood-vessel of the posterior region, and onward to the tail, where the trunk ends by bifurcating into the two dorsal vessels. In each segment a lateral branch leaves the ventral trunk at the anterior dissepiment, turns round and proceeds backward to the next dissepiment, and terminates in the branch to the dorsal vessel. Further, as first observed by Dr. Fritz Müller, a sac-like dilatation takes place shortly after the commencement of the latter, and it fills at intervals, the distention being followed by a contraction which sends the blood onward by the branch to the dorsal vessel.

In vigorous specimens, the currents of the blood are as swift and beautiful as in the tails of young salmon and other translucent vertebrates. When examined in the *liquor sanguinis* of the living animal (as in a favourable view of a healthy tentacle) the blood-corpuscles show a pale nucleus.

Nervous System.—The central mass of the nervous system lies in front of the preoral chamber in the fork of the median muscles, and consists of the ordinary ganglion-cells with connective-tissue bands. No eyes or other sense-organs exist, though the animal is extremely sensitive to light and other stimuli, and lives in regions where there is abundance of sunshine. Two main nerve-trunks proceed backward in the hypoderm—at first outside, then under, and finally to the inner border of the ventral longitudinal muscles. At the commencement each is accompanied by a neural canal (the "tubular fibre" of the late M. Claparède); but, before leaving the anterior region of the body, the canals glide inward and coalesce into a single large median one. The whole central nervous system is hypodermic.

So far as present examination goes, the Annelida present four con-

spicuous modifications in regard to the position of the great nerve-trunks :—

(1) Some have the trunks situated within the muscular layers, or in a central hiatus between the ventral longitudinal muscles, the transverse band between the latter as well as the hypoderm being external.

(2) The cords (as in *Magelona*) are distinctly hypodermic in position, the oblique muscles of the body-wall being attached to a transverse band above them, or to the summit or sides of the area containing them.

(3) The trunks may be embraced by the closely approximated (almost connate) ventral or other longitudinal muscles which overlap the nerve-area.

(4) This group is formed by those in which the cords are separate throughout, being

(*a*) in the substance of the ventral longitudinal muscles,
or (*b*) below or at the edge of the same muscles and within the circular coat.

The neural canals, as far as examined, occur in about thirteen families.

Tentacles.—These remarkable organs extend to about two inches, but are capable of even greater elongation. They are composed of cuticle, hypoderm, basement-tissue, circular and longitudinal muscular coats, the latter having a raphe at each pole in transverse section. Each forms a hollow contractile process furnished with a series of large cylindrical papillæ along the anterior border, a series of central longitudinal muscular fibres giving the latter appendages a sucker-action. The afferent vessel is attached to the raphe next the papillæ, the efferent to the raphe at the smooth border. The entire organ is reproduced with considerable rapidity.

Reproductive Organs.—The ova and spermatozoa are present in each sex in great abundance in the posterior region of the body, and attain perfection in summer and autumn. On the sides of the body, also, peculiar convoluted organs occur in processes composed of the cuticle, hypoderm, and basement-tissue.

The systematic position of *Magelona*, with its peculiar external form and internal structure, was a source of uncertainty to Dr. George Johnston, the only author who attempted its consideration in this respect. So puzzled was he that he placed it (as *Mœa mirabilis*) at the end of his Catalogue for the British Museum, under a family specially constituted for itself (viz. Mæadæ). In the Catalogue of the Fauna of St. Andrews it was located between the Chætopteridæ and the Spionidæ ; but the results of further investigation clearly relegate it to the latter group*. It leans, indeed, wholly to the Spionidæ in minute structure, and especially to such forms as *Prionospio* and *Heterospio* ; though it is true that in the marked regional distinctions, and the great length of the posterior division of the

* Proc. Roy. Soc. Edinb. 1875-76, vol. ix. no. 94, p. 123.

body, it approaches *Spiochætopterus*. While it conforms to the Spionidæ in the structure of its body-wall and bristles, it differs in regard to the absence of the dorsal branchiæ; and further, the short, pinnate and ciliated anterior branchial organs of *Prionospio* appear to be the nearest approach to its elongated tentacles. In the mechanism of its proboscis and in the structure of its snout and circulatory organs, again, it presents features *sui generis*.

III. "On a new Form of Tangential Equation." By JOHN CASEY, LL.D., F.R.S., Professor of Higher Mathematics in the Catholic University of Ireland. Received January 24, 1877.

(Abstract.)

If a variable line make an intercept ν on the axis of x, and an angle ϕ with it on the negative side, the equation of this line will be

$$x+y\cot\phi-\nu=0.$$

The quantities ν and ϕ will determine the position of the line, and may therefore be called its coordinates; hence any relation between ν and ϕ, such as $\nu=f(\phi)$, will be the tangential equation of a curve which is the envelope of the line.

The equation $\nu=f(\phi)$ forms the subject of this paper. It is remarkable for the facility with which it can be transformed into the ordinary Cartesian and tangential equation, as well as into the polar and intrinsic equation of a curve. In a great variety of cases it gives, in a simple form, results which, by other methods, are very cumbersome or nearly impracticable. I have illustrated it throughout by numerous examples, many of which are of historical interest.

The following is an outline of the contents of the paper:—

Chapter I. shows how to transform Cartesian and polar equations into the form $\nu=f(\phi)$. In the course of the investigation a remarkable system of curves of the *nth* class, which are concomitants to any curve of the *nth* degree, are introduced, and their leading properties investigated.

Chapters II., III. are occupied with the transformation of the intrinsic equation, and *vice versâ*, and some allied subjects. In these chapters the whole theory of evolutes, involutes, curvature, &c. are fully considered.

Chapters IV., V. are devoted to the investigation of the properties of cycloids and hypocycloids by their tangential equations. A large number of new properties of these curves are given. The following may be taken as specimens:—1st. If three tangents to a cycloid be given, the envelope of the tangent at its vertex is a parabola. 2nd. If two tangents to a cycloid contain a given angle, the locus of the centre of the circle described about the triangle formed by the two tangents and their chord of contact is a right line.

Chapter VI. contains the theory of positive and negative pedals. The

following is a remarkable theorem on this part of the subject. The positive pedal of a bicircular quartic is the inverse of its negative pedal if the centre of one of its circles of inversion be taken as origin.

Chapter VII. is the last. It contains the theory of reciprocating curves from their tangential and intrinsic equation. Thus, if $\nu=f(\phi)$ be the tangential equation, its reciprocal is in polar equations

$$\rho=\frac{k^2}{f(\phi)\sin\phi}.$$

Again, if $s=f(\phi)$ be the intrinsic equation of a curve, the polar equation of its reciprocal is

$$\frac{k^2}{\rho}=\left(\frac{d^2}{d\phi^2}+1\right)^{-1}f'(\phi).$$

This chapter contains also the theory of parallel curves. The following is a remarkable property of these curves :—Every focus of any order of the original curve is a focus of the next highest order of the parallel curve.

The last problem discussed in the paper is the rectification of bicircular quartics by elliptic integrals, and the method can be extended to sphero-quartics. This problem, so far as the author is aware, is now solved for the first time.

The paper is enriched by the addition of a very important annex by Professor Cayley.

IV. "Addition on the Bicircular Quartic." By A. Cayley, LL.D., F.R.S., Sadlerian Professor of Mathematics in the University of Cambridge. Received January 24, 1877.

(Abstract.)

Prof. Casey communicated to me the MS. of the foregoing memoir, and he has permitted me to make to it the present addition, containing further developments on the theory of the bicircular quartic.

Starting from his theory of the fourfold generation of the curve, Prof. Casey shows that there exist series of inscribed quadrilaterals, ABCD, whereof the sides AB, BC, CD, DA pass through the centres of the four circles of inversion respectively; or (as it is convenient to express it) the pairs of points (A, B), (B, C), (C, D), (D, A) belong to the four modes of generation respectively, and may be regarded as depending upon certain parameters (his θ, θ', θ'', θ''', or say) ω_1, ω_2, ω_3, ω respectively, any three of these being in fact functions of the fourth. Considering a given quadrilateral ABCD, and giving to it an infinitesimal variation, we have four infinitesimal arcs, AA′, BB′, CC′, DD′; these are in fact differential expressions, AA′ and BB′ of the form $M_1 d\omega_1$, BB′ and CC′ of the form $M_2 d\omega_2$, CC′ and DD′ of the form $M_3 d\omega_3$, DD′ and AA′ of the form $M d\omega$;

or, what is the same thing, AA′ is expressible in the two forms $Md\omega$ and $M_1d\omega_1$, BB′ in the two forms $M_1d\omega_1$ and $M_2d\omega_2$ &c., the identity of the two expressions for the same arc, of course, depending on the relation between the two parameters. But any such monomial expression $Md\omega$ of an arc AA′ would be of a complicated form, not obviously reducible to elliptic functions. Casey does not obtain them at all; but he finds geometrically monomial expressions for the differences and sum BB′−AA′, CC′−BB′, DD′+CC′, DD′−AA′ (they cannot be all of them differences), and thence a quadrinomial expression $AA'=N_1d\omega_1+N_2d\omega_2+N_3d\omega_3+Nd\omega$ (his $ds'=\rho d\theta+\rho'd\theta'+\rho''d\theta''+\rho'''d\theta'''$), and that without any explicit consideration of the relations which connect the parameters.

I propose to complete the analytical theory by establishing the monomial equations $AA'=Md\omega=M_1d\omega_1$, &c., and the relations between the parameters ω, ω_1, ω_2, ω_3, which belong to an inscribed quadrilateral ABCD, so as to show what the process really is by which we pass from the monomial form to a quadrinomial form $AA'=Nd\omega+N_1d\omega_1+N_2d\omega_2+N_3d\omega_3$, $=dS$, wherein each term is separately expressible as the differential of an elliptic integral, and to further develop the theory of the transformation to elliptic integrals.

V. "On the Influence of Height in the Atmosphere on the Diurnal Variation of the Earth's Magnetic Force." By J. A. Broun, F.R.S. Received January 25, 1877.

In a paper in the Society's Transactions on the earth's magnetic intensity at Bombay, Mr. C. Chambers has examined the question of the influence of height on the diurnal inequality of the horizontal force*. Two instruments were observed simultaneously at 0^h 22^m and 2^h 29^m P.M.: one, a bifilar magnetometer, was 6 feet above the ground; the other, a unifilar absolute-intensity instrument, was 38 feet above the ground.

Mr. Chambers has found that, in the interval between the two times specified, the change of horizontal force given by the bifilar magnetometer was rather more than one third (0·37) of the mean diurnal range, and that the absolute-intensity instrument showed a change about one fifth less than the bifilar. This difference he does not think instrumental; and he considers that, if true, "it suggests the attribution of a very considerable magnetic influence to the state of the medium intervening between the upper and lower places of observation," &c.

The mean changes of horizontal force between 0^h 22^m and 2^h 29^m P.M. by the two instruments were as follow:—

* "The Absolute Direction and Intensity of the Earth's Magnetic Force at Bombay, &c." By Charles Chambers, F.R.S., Superintendent of the Colaba Observatory. Phil. Trans. 1876, p. 84.

By bifilar, 6 feet above ground	=	− 0·00062 X,
By intensity unifilar, 38 „ „ „	=	− 0·00048 X,
Mean diurnal range by the bifilar	=	0·00166 X,

where X is the whole horizontal magnetic force.

Having occupied myself at different times during the last thirty years with questions relating to the magnetic and meteorological variations at different heights in the atmosphere, I have examined my observations of the horizontal magnetic force, as far as they have been reduced, with reference to Mr. Chambers's conclusion.

In 1847 I made two series of simultaneous observations on the highest point of the Cheviot range of hills, and at Makerstoun in Scotland, about 30 miles distant: the difference of height of the two stations is 2440 feet. The first series was made in June with a bifilar at Makerstoun, and an intensity unifilar on Cheviot*. The difference of the daily range of horizontal force at the two stations, as deduced from hourly observations during three days, was certainly less than one twentieth of the whole daily range; but part at least of this difference was probably due to instrumental causes.

For a second expedition in August of the same year (1847) the same instrument was employed on Cheviot; but a second intensity unifilar was observed at Makerstoun, in addition to the bifilar magnetometer. The observatory on the first expedition was under a tent, on the second it was under ground, a deep cutting having been made for this purpose. The result for the horizontal intensity at Makerstoun showed such differences between the two instruments used there (in the same room), that it was evident the unifilar intensity instrument could not be depended on for small differences in the amount of the variations.

In the years 1855 to 1858 two bifilar magnetometers of precisely the same construction in every way, devised by me expressly for these comparisons, were placed, one in the Agustia Malley Observatory, 6200 feet above the sea, the other in the Trevandrum Observatory (lat. 8° 31′ N.), 200 feet above the sea (about 24 miles from the former station).

The first year's observations of the bifilar in the peak observatory were found to be valueless *for this question*, as it was discovered that the thermometer, though in the same box, did not show with sufficient exactness the temperature of the magnet: this was due to the magnet being within a pasteboard box, to protect it more completely from currents of air, while the thermometer bulb was outside this inner box. This source of error was avoided by placing two thermometers with their bulbs on opposite sides of the magnet and within the inner box. The series of hourly observations after the middle of August 1856 is

* Some account of the results for the Magnetic Declination obtained from this expedition was given to the British Association immediately afterwards (see Brit. Assoc. Rep. 1847, p. 19).

believed to be free from all error. The unit-coefficients were determined with the greatest accuracy for both the instrument on the peak and that on the plain, verified by different methods, giving results which did not differ by one five-hundredth of the whole value. The temperature-coefficients were also found with much exactness; and, as hard steel magnets were chosen expressly for these instruments, the temperature-coefficients were small.

The following results from the hourly observations, made during the last four months of 1856, will be sufficient for my present object. Taking the observations at the hours nearest to those for which Mr. C. Chambers has obtained his result (namely, $0^h\ 30^m$ and $2^h\ 30^m$ P.M.), I find the mean change of horizontal force from the former to the latter time—

On the plain, 200 feet above the sea .... = − 0·0009760 X,
On the mountain, 6200 feet above the sea .... = − 0·0009724 X;

so that the change on the mountain-peak was less than on the plain by one two-hundred-and-seventieth. In each of the months October and November the change was exactly the same at the two stations.

If we take double the interval, so as to keep the same hours in the middle, I find the mean change from $11^h\ 30^m$ A.M. to $3^h\ 30^m$ P.M.—

On the plain, 200 feet above the sea .... = − 0·0016556 X,
On the peak, 6200 feet above the sea .... = − 0·0016510 X,

The change was therefore one three-hundred-and-sixtieth less on the peak than on the plain. The mean of the diurnal ranges for the four months was 0·00215 X.

It will be seen that in the interval of four hours the change was as great as the whole mean diurnal range at Bombay; and if the quantities had been given to five places of decimals only as for Bombay, the movements would have appeared exactly the same at the two stations.

It will thus be seen that instead of $\frac{1}{5}$ less for a difference of 32 feet in height, I do not find more than $\frac{1}{360}$ for a difference of 6000 feet when the change during four hours is considered; nor, till I have made a more searching investigation of the whole series of observations, can I vouch that this difference (which is very much less than the probable error of an observation at either station) is not accidental.

Another series of observations was made at the same two stations in 1864, when a unifilar horizontal-force magnetometer, on Dr. Lamont's construction, was employed, as well as the bifilar instrument. These observations are not yet completely discussed; and on that account I do not enter at present into the question as to what difference may exist in the laws of magnetic variations when the height differs by 6000 feet. Meanwhile it will be useful, I believe, to those who may attempt investi-

gations of this kind, to be acquainted with some of the causes of failure which I have met with, and to know how small the difference of the variations probably is when we ascend to a considerable height in the atmosphere. The results I have obtained from two instruments placed in positions so greatly different as those of the cloudy mountain-peak and the sunny plain, will also show the degree of accuracy attainable when the requisite precautions are taken, and accurate methods of correction and reduction have been employed.

VI. "On Heat as a Germicide when Discontinuously Applied." By John Tyndall, F.R.S. Received February 14, 1877.

Royal Institution, Feb. 14th, 1877.

My dear Huxley,—In my "Preliminary Note," communicated to the Royal Society on the 18th of January, various infusions were referred to as manifesting an astonishing resistance to sterilization by heat. This resistance was traced to its source; and I have been since informed that you were good enough to express at the time a very favourable opinion as to the significance and value of the results indicated.

It will, I think, now interest you to learn that the most obstinate of the infusions referred to in the "Note" have been since rendered tractable by the application of very simple means. Following up the plain suggestions of the germ theory, I have been able, even in the midst of a virulently infective atmosphere, to sterilize all the infusions by a temperature lower than that of boiling water.

It is known that the prolonged application of a low temperature is often equivalent to the brief application of a higher one; and you may therefore be disposed to conclude that in the experiments here referred to I have substituted time for intensity. This, however, is not the case. The result depends solely upon the manner in which the heat is applied. For example, I boil an infusion for fifteen minutes, expose it to a temperature of 90° Fahr., and find it twenty-four hours afterwards swarming with life. I submit a second sample of the same infusion to a temperature lower than that of boiling water for five minutes, and it is rendered permanently barren.

The secret of success here is an open one. I have already referred to the period of latency which precedes the clouding of infusions with visible *Bacteria*. During this period the germs are being prepared for their emergence into the finished organism. They reach the end of this period of preparation successively—the period of latency of any germ depending upon its condition as regards dryness and induration. This, then, is my mode of proceeding:—Before the latent period of any of the germs has been completed (say a few hours after the preparation of the infusion), I subject it for a brief interval to a temperature which may be

under that of boiling water. Such softened and vivified germs as are on the point of passing into active life are thereby killed; others not yet softened remain intact. I repeat this process well within the interval necessary for the most advanced of those others to finish their period of latency. The number of undestroyed germs is further diminished by this second heating. After a number of repetitions, which varies with the character of the germs, the infusion, however obstinate, is completely sterilized.

The periods of heating need not exceed a fraction of a minute in duration. Sum them up in the case of an infusion which they have perfectly sterilized; they amount altogether to, say, five minutes. Boil another sample of the same infusion continuously for fifteen or even sixty minutes, you fail to sterilize it, although the temperature is higher and its time of application more than tenfold that which, discontinously applied, infallibly produces barrenness.

In a few weeks I hope to bring this entire subject under the notice of the Royal Society; meanwhile, if you think it would interest them, I should be glad if you would communicate to the Fellows this general statement of the most recent results of experiment.

Believe me,

Ever faithfully yours,

JOHN TYNDALL.

T. H. Huxley, Esq., Sec. R.S.

Presents received, February 1, 1877.

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"On the Variations of the Daily Range of Atmospheric Temperature as recorded at the Kew Observatory." By Balfour Stewart, LL.D., F.R.S., Professor of Natural Philosophy at Owens College, Manchester. Received May 25, 1876*.

1. There are reasons for supposing that differences of temperature in the various portions of our globe and of its atmosphere are, in whatever manner these may be produced, the chief primary causes of meteorological activity. Such differences of temperature produce differences of atmospheric pressure, and these in their turn produce winds.

For this reason the daily range of atmospheric temperature has been chosen as an element which serves well, in many respects at least, to indicate the varying meteorological activity of the place, and also because, if it be found capable of indicating useful results, it has the advantage of being observed with little labour, while the observations are comparatively easy of reduction.

The records of the Kew Observatory have been chosen because there the atmospheric temperature has been observed for a long series of years, particular attention being paid to the construction and exposure of the stand containing the thermometers †. Twenty-one years of these records have been reduced, beginning with the year 1855, a year just preceding the minimum of sun-spot activity, and ending with 1875, which may also be reckoned a minimum sun-spot year.

Two complete sun-spot periods are thus embraced in these observations.

A. *Annual Variation of Temperature-Range.*

2. It is already well known that the daily range of atmospheric temperature is greatest in summer and least in winter. The following Table exhibits the various monthly means of the daily temperature-range:—

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* Read June 15, 1876. See *antè*, p. 156.

† The writer is indebted to the kindness of the Kew Committee for giving him access to the records of the maximum and minimum temperatures taken at the Kew Observatory.

TABLE I.—Containing Monthly Means of Daily Temperature-Range expressed in Fahrenheit degrees.

Year.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean of year.
1855.	7°·0	9°·1	11°·8	17°·2	16°·0	16°·6	15°·9	16°·5	16°·6	10°·9	8°·0	8°·8	12°·87
1856.	7·4	7·7	10·2	15·0	13·1	16·9	17·2	17·5	15·2	12·0	10·1	10·0	12·69
1857.	7·7	12·9	11·9	13·5	17·9	21·2	19·2	17·9	14·8	12·3	8·9	8·9	13·92
1858.	11·3	9·2	14·3	15·9	16·6	19·8	17·5	19·3	15·1	13·5	11·2	9·4	14·42
1859.	9·5	12·1	12·1	15·8	16·6	17·1	21·0	18·8	15·4	12·8	12·5	10·5	14·52
1860.	10·3	11·6	11·5	15·2	17·4	13·0	15·8	13·0	15·0	11·9	9·6	9·3	12·80
1861.	10·2	10·8	12·1	16·4	16·6	15·1	15·1	18·2	16·3	12·9	13·4	10·1	13·93
1862.	9·2	7·2	10·3	12·7	15·1	15·1	15·9	15·9	14·8	12·4	11·2	9·6	12·45
1863.	9·8	12·2	15·4	17·1	17·1	16·6	21·4	16·9	15·9	11·5	11·4	10·5	14·65
1864.	9·3	9·8	14·1	16·7	17·3	17·1	20·7	20·7	15·4	11·5	13·3	9·2	14·59
1865.	9·4	9·5	10·8	21·5	18·6	19·5	18·5	17·0	20·5	14·8	12·4	7·5	15·00
1866.	9·7	10·5	11·5	14·3	17·9	18·5	17·5	16·2	12·3	11·9	12·3	10·7	13·61
1867.	11·2	9·6	10·6	14·2	17·3	18·4	17·7	17·9	16·0	14·7	12·1	10·1	14·15
1868.	8·5	12·3	13·8	15·8	19·9	21·9	22·4	16·8	19·1	15·7	10·4	8·9	15·45
1869.	10·0	10·1	10·5	17·6	14·8	17·9	21·1	18·9	15·2	14·3	11·8	9·1	14·27
1870.	8·7	8·7	11·7	22·1	21·4	21·1	20·2	18·7	18·9	14·6	12·4	9·1	15·63
1871.	8·4	9·5	15·2	13·5	19·5	15·6	15·9	20·9	13·9	16·6	10·6	8·7	14·02
1872.	9·4	11·2	13·2	16·3	16·3	18·1	20·6	18·1	15·4	13·3	8·7	7·4	14·00
1873.	6·8	7·3	14·4	15·9	16·6	17·3	18·8	16·4	15·8	14·5	9·9	9·2	13·57
1874.	10·2	10·3	14·5	16·8	16·7	18·4	21·1	17·0	15·1	11·6	10·9	8·2	14·23
1875.	8·5	8·7	11·5	17·2	17·6	17·9	14·7	17·3	16·7	11·8	9·8	7·3	13·25
Mean of 21 years.	9·17	10·01	12·45	16·22	17·16	17·77	18·49	17·61	15·88	13·12	11·00	9·17	14·00

B. *Variations of Long Period.*

3. It will be seen from Table I. that the various yearly means exhibit considerable differences amongst themselves.

Thus the first two years (1855 and 1856), as well as the last year (1875), exhibit comparatively small values, and suggest the interesting question whether the yearly mean daily range does not depend, among other things, on the state of the sun's surface with regard to spots. There are three minimum sun-spot years in Table I., say 1856, 1866, and 1875, and two maximum years, say 1859 and 1870; and accordingly we find that the three former correspond to small ranges, namely 12°·69, 13°·61, 13°·25, and the two latter to large ranges, namely 14°·52, 15°·63. But on the other hand, and against this evidence, Table I. records a temperature-oscillation between 1859 and 1866 as great, or nearly as great, as any which apparently corresponds to sun-spot variation.

4. Before discussing this point further, it will be desirable to reconstruct the elements of Table I. on a somewhat different principle.

To perceive this let us assume that the influence of the sun upon the daily range of temperature is really greater during years of maximum than during years of minimum sun-spot frequency. Now if this influence is identical with or follows laws similar to the heating-influence of

the sun, it will of course be least in winter and greatest in summer. A given increase of the range in winter will thus denote a greater relative increase in the power of the sun than the same absolute increase of range in summer. Thus if the sun's influence were doubled, the range for December (Table I.) might be supposed to rise from 9°·17 to 18°·34, and the range for July from 18°·49 to 36°·98. In fine, what we have to do in order, according to this hypothesis, to obtain the apparent increase in solar activity is to find *in what proportion* the normal monthly range is increased. This is done in Table II., in which the normal range for each month is reckoned =100. But here we must bear in mind that while Table II. is constructed in conformity with a certain hypothesis regarding the solar action, it does not necessarily follow that this hypothesis is correct; indeed Table II. is only to be regarded as exhibiting a different way of arranging the results.

TABLE II.—Exhibiting the Proportional Solar Activity, the Normal for each Month being reckoned =100.

Year.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean of the year.
1855.	76	91	94	106	93	93	86	94	105	83	73	96	90·8
1856.	81	77	82	92	76	95	93	99	96	91	92	109	90·2
1857.	84	129	96	83	104	119	104	102	93	94	81	97	98·8
1858.	123	92	115	98	97	112	95	109	95	103	102	102	103·6
1859.	103	121	97	97	97	96	114	107	98	97	114	114	104·6
1860.	112	116	92	94	101	73	85	74	95	90	87	102	93·4
1861.	111	108	97	101	97	85	82	103	103	97	122	110	101·3
1862.	100	72	83	78	88	85	86	90	93	95	102	105	89·7
1863.	107	122	124	106	100	93	116	96	100	88	104	114	105·8
1864.	101	98	113	103	101	97	112	118	98	88	121	100	104·2
1865.	102	95	87	133	108	109	100	97	129	113	113	82	105·7
1866.	106	105	92	88	104	104	95	92	77	91	112	117	98·6
1867.	122	96	86	88	101	103	96	102	101	112	110	110	102·2
1868.	92	123	111	97	116	123	121	95	120	119	95	97	109·1
1869.	109	101	84	109	86	101	114	107	96	109	107	99	101·8
1870.	95	87	94	136	125	119	109	106	119	111	113	99	109·4
1871.	92	95	122	83	114	88	86	119	87	126	96	95	100·2
1872.	103	112	106	101	95	102	111	103	97	101	79	81	99·2
1873.	74	73	116	98	97	98	102	93	99	110	90	100	95·8
1874.	111	103	115	103	97	104	114	97	95	88	99	89	101·3
1875.	93	87	92	106	102	101	79	98	105	89	89	79	93·3

5. It will be seen that, as far as correspondence with sun-spot periods is concerned, Table II. gives results similar to those of Table I., exhibiting the same kind of general correspondence, but exhibiting also a temperature-oscillation of considerable magnitude, which may perhaps be identified with a subsidiary solar-spot fluctuation as exhibited in the curves of Messrs. De La Rue, Stewart, and Loewy (see Phil. Trans. for 1870), but which is out of proportion to it in relative magnitude.

6. The dates of minimum and maximum spot frequency, as given by Messrs. De La Rue, Stewart, and Loewy, in the above-mentioned paper are as follows:—

Minimum......April 1856,
Maximum......September 1859,
Minimum......February 1867;

(to which we may add)—

Maximum......Somewhere in 1870?
Minimum......Somewhere in 1875?

taking yearly periods, Table II. gives corresponding fluctuations as follows:—

Minimum......January 1856,
Maximum......July 1859,
Minimum......May 1866,
Maximum......October 1870,
Minimum......Probably end of 1875*.

7. If we still regard it as most likely, though not proven, both from the evidence herein recorded and from collateral considerations, that there is some connexion between the daily temperature-range and the state of the sun with regard to spots, then we may suppose that the redundant temperature-oscillation between 1859 and 1866, already alluded to, is a local phenomenon which will disappear when a sufficient number of stations are discussed. There would seem, however, to be another possible mode of explaining the circumstance, and allusion will be made to this in another part of this paper (art. 17).

C. *Lunar Annual Variation.*

8. It will be of interest to determine whether the temperature-range has any reference to the relative position of the sun and moon. For this purpose the whole period of observation has been portioned out into lunations, beginning with new moon. Each lunation is divided into 8 parts, entitled:—(0), (1), (2), (3), (4), (5), (6), (7)—(0) denoting new and (4) full moon.

The various lunations with the corresponding values of the temperature-range are exhibited in Table III. It will, however, be here necessary to state how these values have been obtained. Take the dates (civil time) of the four quarterly phases of the moon as given by the Nautical Almanac, and under each of these dates, as a centre, group seven observations. Each value in Table III. corresponding to (0), (2), (4), (6) is thus the mean of seven separate observations of daily range.

* This is only a rough comparison, and must not be regarded as indicating that meteorological phenomena precede certain corresponding solar phenomena with which they are supposed to be connected.—[Added March 5, 1877.]

The half-quarterly phases (1), (3), (5), (7) are then interpolated in point of time, so that sometimes their date will fall upon a given civil day, and sometimes between one civil day and another. In the former case the mean of seven observations, and in the latter the mean of six, is taken.

The following numerical illustration will render the method of procedure clear:—

March 1858.

Civil day.	Temp. range.	Lunar phase.	Civil day.	Temp. range.	Lunar phase.
12.	15·8		22.	25·9	(2)
13.	17·9		23.	31·7	
14.	11·0		24.	29·3	
15.	10·0	(0)	25.	5·5	(3)
16.	13·4		26.	22·2	
17.	9·8		27.	20·6	
18.	11·8	(1)	28.	12·6	
19.	15·1		29.	21·0	(4)
20.	12·9		30.	23·9	
21.	25·8		31.	6·7	
			(April 1) 32.	2·7	

The mean of the observations for 12–18 are taken for (0).
The mean of those for 16–21 " " (1).
" " " 19–25 " " (2).
" " " 23–28 " " (3).
" " " 26–32 " " (4).

These means will be found in Table III. under lunation No. 40.

The above method is easily worked, and it is probably sufficiently accurate for the purpose of this research.

TABLE III.—Exhibiting the Temperature-ranges grouped according to Lunations.

No. of lunation.	Date of new moon, beginning lunation.	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		°	°	°	°	°	°	°	°
1.	Jan. 18, 1855.	7·9	8·4	9·2	7·3	7·2	6·7	6·4	10·2
2.	Feb. 16, "	10·9	12·6	9·9	10·2	13·7	13·7	11·7	11·7
3.	Mar. 18, "	12·5	9·1	9·1	12·7	18·0	17·0	13·7	12·8
4.	Apr. 16, "	18·0	19·3	20·5	14·7	16·6	17·9	16·2	13·1
5.	May 16, "	14·5	15·7	18·7	17·6	12·2	16·6	17·4	18·5
6.	June 14, "	14·8	14·7	17·2	17·2	17·7	16·6	20·2	17·2
7.	July 14, "	13·4	15·5	15·5	11·5	14·6	15·8	15·8	16·0
8.	Aug. 12, "	16·3	19·0	16·1	15·6	17·1	16·9	14·7	20·0
9.	Sept. 11, "	21·2	11·9	15·1	17·7	19·4	16·6	11·0	12·8
10.	Oct. 11, "	12·5	13·4	12·9	9·4	9·8	6·9	6·5	9·1

TABLE III. (*continued*).

No. of lunation.	Date of new moon, beginning lunation.	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		°	°	°	°	°	°	°	°
11.	Nov. 9, 1855.	9·3	7·9	9·4	4·8	5·1	7·2	7·8	6·6
12.	Dec. 9, ,,	5·9	9·4	10·8	6·9	11·4	7·0	8·0	6·1
13.	Jan. 7, 1856.	4·6	5·5	10·0	8·8	5·8	8·7	8·1	9·9
14.	Feb. 6, ,,	9·8	6·3	7·0	5·1	5·3	11·4	6·6	6·2
15.	Mar. 6, ,,	8·8	13·7	10·9	7·2	8·0	10·7	19·3	20·7
16.	Apr. 5, ,,	12·9	12·4	12·6	13·4	17·6	19·8	14·7	13·4
17.	May 4, ,,	11·0	10·6	12·9	12·7	16·7	16·3	12·3	12·8
18.	June 2, ,,	15·4	18·7	16·6	15·6	16·7	12·9	15·9	18·8
19.	July 2, ,,	19·0	15·1	14·8	12·8	13·6	17·3	20·6	23·4
20.	July 31, ,,	24·9	26·0	22·5	20·0	15·8	10·7	10·7	13·8
21.	Aug. 30, ,,	14·5	20·9	20·7	14·8	13·9	15·0	13·7	11·0
22.	Sept. 29, ,,	10·9	11·3	6·5	8·6	9·8	10·6	13·4	14·4
23.	Oct. 28, ,,	16·8	12·8	11·0	10·5	8·9	12·6	12·8	7·5
24.	Nov. 27, ,,	8·2	10·9	14·4	11·7	5·2	9·8	9·6	8·0
25.	Dec. 27, ,,	10·2	9·0	5·5	5·8	7·2	8·9	10·4	8·2
26.	Jan. 26, 1857.	6·5	9·4	10·5	10·5	11·0	12·1	11·8	14·0
27.	Feb. 24, ,,	17·1	16·1	12·9	13·0	10·5	13·3	12·3	9·0
28.	Mar. 25, ,,	12·1	10·0	10·6	11·0	13·3	15·8	19·5	14·6
29.	Apr. 24, ,,	9·7	10·1	14·4	16·8	18·9	19·0	22·0	17·8
30.	May 23, ,,	14·9	18·5	18·6	22·2	18·3	19·3	21·7	19·7
31.	June 21, ,,	21·4	26·7	21·6	12·6	14·7	21·2	23·7	23·2
32.	July 21, ,,	20·7	18·9	19·1	21·4	18·4	15·1	17·3	17·7
33.	Aug. 19, ,,	16·2	17·9	18·2	15·7	16·0	15·9	13·2	15·2
34.	Sept. 18, ,,	17·2	14·0	13·9	16·4	13·0	12·0	12·9	11·5
35.	Oct. 17, ,,	12·3	10·6	12·9	15·3	12·7	7·4	5·9	12·0
36.	Nov. 16, ,,	10·1	9·7	10·0	9·4	8·5	10·6	11·4	7·3
37.	Dec. 16, ,,	5·5	8·9	6·8	7·9	9·4	7·9	10·0	11·7
38.	Jan. 15, 1858.	13·8	10·1	12·0	14·5	10·6	8·4	12·0	8·7
39.	Feb. 13, ,,	6·4	8·0	9·2	9·7	7·8	7·9	10·1	12·6
40.	Mar. 15, ,,	12·8	14·8	20·9	20·3	15·7	12·5	10·1	12·4
41.	Apr. 13, ,,	17·8	20·7	23·2	20·4	13·7	13·5	17·3	17·5
42.	May 13, ,,	16·2	14·6	14·5	16·3	18·5	21·5	19·5	19·2
43.	June 11, ,,	21·8	22·4	21·8	19·4	18·4	16·6	15·4	15·3
44.	July 10, ,,	15·9	18·4	20·9	17·5	15·8	21·1	23·1	21·6
45.	Aug. 9, ,,	24·1	21·0	17·2	16·3	15·7	15·8	18·3	15·3
46.	Sept. 7, ,,	13·4	20·6	22·1	12·4	13·7	13·5	12·1	14·3
47.	Oct. 7, ,,	14·7	16·2	14·4	11·0	9·8	13·3	15·3	17·1
48.	Nov. 5, ,,	14·1	13·9	12·5	6·2	11·0	10·6	7·8	10·9
49.	Dec. 5, ,,	12·3	6·8	5·8	9·5	12·2	11·1	9·3	7·6
50.	Jan. 4, 1859.	7·5	11·4	10·6	10·0	12·7	11·2	9·8	7·8
51.	Feb. 3, ,,	10·2	10·4	9·1	10·2	12·1	11·3	16·1	17·5
52.	Mar. 4, ,,	15·1	15·1	9·6	9·0	12·7	12·4	9·9	12·7
53.	Apr. 3, ,,	23·5	21·0	13·4	13·4	16·0	17·8	13·9	9·4
54.	May 2, ,,	12·1	19·8	19·1	16·8	14·7	11·9	19·2	19·0
55.	June 1, ,,	16·2	16·3	16·4	15·9	18·2	16·7	18·5	18·9
56.	June 30, ,,	16·7	20·0	22·8	25·3	27·1	23·0	18·7	16·4
57.	July 29, ,,	15·6	18·2	19·1	14·3	13·9	19·1	24·5	26·0
58.	Aug. 28, ,,	18·4	15·9	16·5	14·6	17·1	15·6	15·5	14·6
59.	Sept. 26, ,,	12·6	11·8	14·7	13·4	10·1	10·4	10·6	16·2
60.	Oct. 26, ,,	17·5	13·7	11·1	10·0	12·8	16·4	15·0	16·8
61.	Nov. 24, ,,	12·4	8·0	10·9	12·1	9·3	8·5	9·5	12·8
62.	Dec. 24, ,,	10·4	9·9	7·4	7·0	9·2	9·3	11·1	12·2
63.	Jan. 23, 1860.	12·1	13·0	9·4	11·0	11·9	11·3	12·3	10·7
64.	Feb. 21, ,,	10·4	13·9	13·9	13·9	11·0	12·5	13·4	11·4
65.	Mar. 22, ,,	10·3	11·3	10·1	12·2	16·5	15·2	14·7	14·2

TABLE III. (*continued*).

No. of lunation.	Date of new moon, beginning lunation.	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		°	°	°	°	°	°	°	°
66.	Apr. 21, 1860.	11·5	11·6	20·8	24·7	19·6	17·6	15·4	15·5
67.	May 20, „	21·1	20·7	14·6	13·0	12·6	12·3	12·9	12·4
68.	June 19, „	14·7	13·7	12·4	14·0	17·0	17·2	17·6	15·9
69.	July 18, „	14·2	12·1	14·0	16·0	13·4	14·4	14·1	14·9
70.	Aug. 16, „	13·4	11·5	10·7	12·1	14·9	19·6	19·4	20·3
71.	Sept. 15, „	14·8	9·4	10·1	13·0	12·3	13·1	13·1	11·6
72.	Oct. 14, „	11·6	10·7	11·7	11·1	11·0	14·2	12·5	8·6
73.	Nov. 13, „	7·9	8·1	10·3	10·1	7·8	5·9	6·6	8·6
74.	Dec. 12 „	7·3	7·7	8·0	11·4	16·2	12·5	9·3	11·9
75.	Jan. 11, 1861.	13·4	8·4	8·3	9·6	10·6	11·9	11·3	8·3
76.	Feb. 9, „	6·7	13·5	12·7	9·9	10·8	12·0	12·3	12·6
77.	Mar. 11, „	13·2	13·9	12·1	12·1	12·1	12·6	12·8	13·6
78.	Apr. 10, „	17·2	16·2	15·9	17·8	18·5	17·8	17·9	13·1
79.	May 9, „	11·3	16·9	24·7	22·2	17·8	15·4	14·3	11·2
80.	June 8, „	11·5	16·2	19·1	19·9	15·8	15·3	16·1	15·5
81.	July 8, „	17·5	16·7	14·8	13·9	12·0	13·9	16·3	17·4
82.	Aug. 6, „	16·7	18·2	16·6	16·5	18·7	16·5	20·2	22·8
83.	Sept. 4, „	18·1	15·2	16·9	16·5	17·4	13·5	14·7	13·1
84.	Oct. 4, „	14·1	14·4	14·9	14·2	14·9	12·1	9·3	9·3
85.	Nov. 2, „	11·8	15·3	15·2	12·5	13·3	14·8	12·6	10·3
86.	Dec. 2, „	14·1	16·9	14·6	10·4	5·9	4·9	7·3	9·3
87.	Dec. 31, „	9·1	9·3	11·6	10·5	7·2	9·0	9·5	7·3
88.	Jan. 30, 1862.	6·5	6·4	7·5	10·6	8·4	8·7	8·8	3·9
89.	Feb. 28, „	5·5	12·5	12·2	8·2	8·8	7·6	12·2	12·7
90.	Mar. 30, „	9·3	9·0	6·6	8·3	13·0	12·4	12·7	16·8
91.	Apr. 28, „	21·2	18·9	16·8	11·2	11·8	18·7	19·7	14·9
92.	May 28, „	12·7	14·8	18·2	18·0	14·1	13·8	11·9	12·8
93.	June 27, „	16·4	14·5	13·7	12·8	15·0	16·3	15·0	15·3
94.	July 26, „	18·2	19·7	19·6	15·8	14·0	14·1	12·4	17·2
95.	Aug. 25, „	18·7	16·6	15·5	16·5	16·4	14·7	13·9	12·9
96.	Sept. 23, „	13·9	13·2	12·3	13·6	10·6	10·5	10·5	11·9
97.	Oct. 23, „	12·4	12·3	13·6	13·3	12·6	13·6	13·4	8·5
98.	Nov. 21, „	10·0	12·7	10·8	8·2	6·8	9·9	12·8	10·9
99.	Dec. 21, „	8·9	9·2	9·4	9·3	9·1	11·3	9·7	9·6
100.	Jan. 19, 1863.	8·1	7·8	11·9	11·0	9·0	10·0	11·2	17·3
101.	Feb. 18, „	17·4	11·4	12·2	14·9	16·0	11·8	15·1	14·6
102.	Mar. 19, „	14·7	19·8	16·3	16·4	18·3	12·3	12·7	18·2
103.	Apr. 18, „	21·3	20·0	17·3	17·0	20·4	20·7	17·1	12·1
104.	May 17, „	11·8	13·9	20·7	19·9	19·9	18·6	13·9	17·4
105.	June 16, „	15·6	15·7	16·5	18·2	19·5	20·2	21·4	22·7
106.	July 15, „	23·7	19·3	17·1	23·0	23·4	21·1	17·5	18·4
107.	Aug. 14, „	18·7	15·6	14·5	15·5	15·7	14·2	13·2	13·9
108.	Sept. 13, „	13·1	14·5	15·4	17·8	22·0	15·9	11·4	12·8
109.	Oct. 12, „	9·7	10·7	9·4	13·5	15·8	11·6	10·5	11·7
110.	Nov. 11, „	14·7	15·6	10·7	10·5	7·8	11·5	13·4	9·8
111.	Dec. 10, „	9·4	10·6	10·7	11·6	8·7	6·6	7·8	11·0
112.	Jan. 9, 1864.	13·6	9·1	5·0	7·0	8·4	10·0	11·8	9·9
113.	Feb. 7, „	10·9	15·2	11·9	6·5	7·0	8·4	12·4	12·6
114.	Mar. 8, „	9·7	12·1	12·9	15·8	17·4	18·0	14·6	13·2
115.	Apr. 6, „	12·0	14·9	16·5	22·1	23·5	16·1	14·1	11·0
116.	May 6, „	10·5	10·0	17·9	25·7	22·0	18·8	17·8	15·0
117.	June 4, „	17·9	21·9	18·9	17·4	17·5	16·0	14·9	15·8
118.	July 4, „	18·3	19·2	22·0	23·5	22·2	21·3	19·9	20·9
119.	Aug. 2, „	23·9	23·6	22·4	25·3	20·1	18·2	18·0	20·2
120.	Sept. 1, „	18·0	15·2	14·0	12·0	13·9	14·5	15·7	19·9

TABLE III. (*continued*).

No. of lunation.	Date of new moon, beginning lunation.	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		°	°	°	°	°	°	°	°
121.	Sept. 30, 1864.	16·8	14·1	15·8	9·3	10·6	12·6	13·8	10·1
122.	Oct. 30, "	6·2	12·6	15·3	12·6	11·9	13·2	11·4	13·5
123.	Nov. 29, "	16·7	12·9	10·3	10·9	7·4	9·0	8·7	4·2
124.	Dec. 28, "	8·4	9·9	11·5	10·1	9·8	8·3	8·9	8·4
125.	Jan. 27, 1865.	8·6	14·1	9·3	7·0	7·1	11·2	11·4	8·3
126.	Feb. 25, "	10·3	9·6	10·5	9·8	7·6	7·7	10·8	12·7
127.	Mar. 27, "	12·8	15·9	16·6	21·8	24·3	17·4	18·1	25·2
128.	Apr. 25, "	30·9	24·7	17·2	19·2	16·1	15·0	20·9	21·7
129.	May 24, "	22·3	18·7	14·7	17·1	19·3	22·1	19·9	24·7
130.	June 23, "	25·4	13·0	19·0	21·2	14·0	15·7	17·9	17·3
131.	July 22, "	17·5	20·5	19·5	18·6	20·3	17·1	14·7	15·5
132.	Aug. 21, "	16·8	17·1	18·0	19·6	23·0	18·8	17·7	22·8
133.	Sept. 19, "	18·9	18·1	23·0	20·8	22·4	19·4	14·6	14·6
134.	Oct. 19, "	12·2	12·2	14·0	13·8	15·5	13·9	10·9	15·0
135.	Nov. 18, "	13·2	8·9	9·3	8·1	6·6	6·3	4·2	7·5
136.	Dec. 18, "	7·7	5·7	9·7	10·6	9·4	9·4	8·1	11·3
137.	Jan. 16, 1866.	11·1	8·4	10·3	9·8	8·3	11·2	10·3	9·8
138.	Feb. 15, "	10·1	11·8	14·5	8·9	9·4	11·6	11·2	12·5
139.	Mar. 16, "	12·7	8·8	11·4	13·3	11·2	11·7	10·1	10·1
140.	Apr. 15, "	15·1	17·7	17·9	19·3	14·4	15·0	18·6	15·0
141.	May 14, "	16·3	23·0	20·3	18·4	19·7	16·8	18·8	19·0
142.	June 12, "	18·1	16·5	18·3	20·2	19·4	15·6	15·4	18·5
143.	July 12, "	22·0	18·7	18·8	19·4	14·6	14·5	15·2	14·5
144.	Aug. 10, "	15·7	14·3	16·8	18·1	16·3	14·7	14·6	10·9
145.	Sept. 9, "	9·4	11·5	13·6	11·9	13·1	14·7	10·1	7·0
146.	Oct. 8, "	8·1	12·4	16·2	12·8	11·7	13·2	16·2	12·3
147.	Nov. 7, "	12·7	15·0	12·8	12·4	9·9	10·2	11·7	9·5
148.	Dec. 7, "	11·3	14·2	12·7	11·4	9·9	9·3	8·3	10·9
149.	Jan. 6, 1867.	18·9	9·4	10·7	11·2	9·1	10·0	9·3	8·6
150.	Feb. 4, "	11·5	10·5	8·1	8·8	10·5	10·8	9·2	9·5
151.	Mar. 6, "	9·3	7·2	8·2	11·0	12·2	11·7	12·8	17·3
152.	Apr. 4, "	16·9	14·7	14·0	13·2	11·7	9·9	14·8	20·8
153.	May 4, "	23·0	24·3	17·0	9·6	13·2	14·0	18·9	20·7
154.	June 2, "	19·4	13·3	19·3	17·1	12·8	16·2	21·0	25·5
155.	July 1, "	21·0	16·3	22·1	19·8	13·8	14·9	15·1	17·9
156.	July 31, "	18·5	15·3	16·6	22·5	17·7	14·8	19·9	20·6
157.	Aug. 29, "	17·0	16·3	15·7	16·7	15·2	13·9	14·5	17·5
158.	Sept. 27, "	15·9	14·0	16·2	15·1	15·3	14·6	15·2	13·8
159.	Oct. 27, "	13·8	12·2	13·5	14·1	12·8	10·4	7·3	9·4
160.	Nov. 26, "	13·1	13·1	9·6	10·3	9·8	8·3	9·8	15·2
161.	Dec. 25, "	12·0	7·6	5·3	4·9	5·9	9·2	9·9	6·0
162.	Jan. 24, 1868.	9·3	13·5	13·6	13·3	13·6	13·5	12·3	13·2
163.	Feb. 23, "	14·1	11·6	10·6	10·9	13·5	14·6	12·3	12·2
164.	Mar. 24, "	13·7	17·0	17·9	21·3	19·8	12·9	17·5	15·6
165.	Apr. 22, "	11·4	13·7	18·3	20·3	22·3	23·5	18·3	19·1
166.	May 22, "	16·9	18·6	20·2	18·5	19·0	22·5	24·9	21·6
167.	June 20, "	20·1	20·9	23·0	19·3	18·7	25·2	26·1	23·4
168.	July 19, "	24·6	24·5	20·2	23·6	22·7	20·4	18·2	14·9
169.	Aug. 18, "	11·7	11·7	14·6	17·6	26·5	28·3	23·0	18·2
170.	Sept. 16, "	14·4	14·5	17·1	14·4	12·3	13·8	17·2	18·9
171.	Oct. 15, "	17·1	16·9	17·7	14·3	10·8	8·5	12·4	10·0
172.	Nov. 14, "	7·1	12·1	14·0	9·3	8·2	8·0	6·0	8·9
173.	Dec. 14, "	9·8	10·0	10·4	9·6	11·1	12·3	12·7	7·8
174.	Jan. 12, 1869.	6·0	10·1	8·3	9·7	12·3	12·2	12·8	10·7
175.	Feb. 11, "	8·6	8·0	9·2	10·2	9·9	9·8	13·3	11·0

TABLE III. (*continued*).

No. of lunation.	Date of new moon, beginning lunation.	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		°	°	°	°	°	°	°	°
176.	Mar. 13, 1869.	7·9	9·5	10·6	10·0	11·1	13·3	14·4	14·3
177.	Apr. 12, „	21·6	14·8	14·9	17·9	18·8	20·4	16·1	13·2
178.	May 11, „	14·7	16·0	15·9	17·8	13·9	14·3	18·6	21·2
179.	June 10, „	19·3	16·8	14·4	13·7	18·5	18·4	18·0	19·3
180.	July 9, „	22·1	26·1	22·5	24·0	23·8	20·3	16·5	16·3
181.	Aug. 7, „	15·1	16·6	16·9	16·7	21·6	24·9	18·5	22·5
182.	Sept. 6, „	18·4	13·0	11·8	9·4	12·9	15·7	16·4	14·4
183.	Oct. 5, „	15·6	20·7	18·0	11·0	12·6	10·8	12·2	11·9
184.	Nov. 3, „	10·0	15·1	17·1	13·5	12·5	7·9	8·4	6·1
185.	Dec. 3, „	8·0	6·9	9·1	13·2	10·4	6·0	8·9	12·0
186.	Jan. 2, 1870.	7·1	7·7	9·7	9·9	5·8	4·8	7·6	13·8
187.	Jan. 31, „	13·1	7·4	6·3	6·9	7·2	7·5	11·1	12·9
188.	Mar. 2, „	10·4	9·7	10·5	15·2	14·9	12·6	12·5	11·0
189.	Apr. 1, „	19·0	28·0	23·5	19·1	22·5	17·8	23·9	15·2
190.	Apr. 30, „	14·0	17·9	22·9	19·3	17·6	25·3	25·1	24·5
191.	May 30, „	22·1	18·0	20·7	20·3	23·4	24·4	22·5	19·4
192.	June 28, „	17·2	17·9	19·9	20·5	20·7	21·6	22·7	23·2
193.	July 28, „	17·5	15·5	17·9	20·2	17·5	19·2	21·5	19·7
194.	Aug. 26, „	16·8	20·6	19·0	13·6	14·4	18·1	21·1	22·8
195.	Sept. 25, „	25·3	25·3	21·3	15·9	17·7	20·3	15·6	12·0
196.	Oct. 24, „	10·6	10·1	12·1	13·3	9·6	11·5	15·9	16·5
197.	Nov. 23, „	12·3	10·6	8·7	8·5	8·6	9·8	9·3	8·0
198.	Dec. 22, „	10·8	12·5	10·7	10·9	9·6	8·7	10·5	9·9
199.	Jan. 20, 1871.	7·7	6·4	5·1	6·1	8·3	8·4	10·4	9·9
200.	Feb. 19, „	9·3	9·0	11·3	16·3	12·6	10·1	9·7	13·3
201.	Mar. 21, „	21·9	20·2	11·8	11·5	14·9	18·0	17·0	12·3
202.	Apr. 19, „	9·4	12·3	13·4	16·3	20·0	19·6	15·9	16·9
203.	May 19, „	21·0	22·7	22·2	20·0	17·2	15·3	16·1	16·8
204.	June 18, „	13·2	11·6	15·5	19·2	16·2	14·6	15·6	15·6
205.	July 17, „	16·1	15·2	14·7	17·6	20·7	21·7	24·5	25·3
206.	Aug. 16, „	19·2	17·1	16·0	19·1	21·2	16·4	17·0	13·6
207.	Sept. 14, „	13·2	12·1	13·6	13·7	11·6	14·2	15·3	19·9
208.	Oct. 14, „	22·1	18·2	16·7	17·1	12·5	7·1	8·5	13·5
209.	Nov. 12, „	17·4	14·3	12·5	9·0	6·4	7·4	9·2	10·4
210.	Dec. 12, „	10·3	9·8	8·7	7·1	6·5	7·6	9·5	8·8
211.	Jan. 10, 1872.	10·8	13·2	10·3	8·2	6·8	7·5	8·5	8·1
212.	Feb. 9, „	11·5	11·7	10·7	12·1	11·4	11·1	13·4	14·7
213.	Mar. 9, „	17·5	18·9	12·0	8·6	12·7	12·3	9·5	12·0
214.	Apr. 8, „	16·9	20·7	17·6	14·8	17·6	15·9	20·9	19·3
215.	May 7, „	12·1	12·2	11·8	16·1	22·1	19·7	17·1	16·8
216.	June 6, „	14·5	13·7	21·2	23·4	20·7	17·0	17·6	20·5
217.	July 5, „	22·0	20·8	19·9	18·5	22·9	22·8	19·4	16·8
218.	Aug. 4, „	14·7	15·3	18·3	20·9	22·7	19·3	18·5	17·0
219.	Sept. 3, „	17·6	15·1	15·5	15·7	13·2	16·3	16·0	14·7
220.	Oct. 2, „	14·4	17·2	16·9	15·6	14·9	10·0	11·1	10·1
221.	Nov. 1, „	10·4	10·4	9·4	7·0	7·0	8·5	9·8	7·0
222.	Nov. 30, „	6·2	8·5	8·5	8·1	9·4	7·8	6·9	5·6
223.	Dec. 30, „	4·7	6·4	6·7	5·6	5·0	5·1	7·5	10·3
224.	Jan. 28, 1873.	7·9	5·6	6·8	6·1	7·8	5·9	6·5	11·0
225.	Feb. 27, „	11·8	13·4	12·0	10·9	10·9	8·4	12.4	21·1
226.	Mar. 28, „	22·7	19·5	14·4	11·1	16·2	21·7	18·0	16·8
227.	Apr. 26, „	13·9	16·1	16·0	16·3	17·3	13·8	14·3	19·1
228.	May 26, „	18·6	16·1	16·7	18·4	19·3	18·7	18·3	17·1
229.	June 24, „	16·2	15·5	16·2	21·6	18·7	16·3	16·3	20·7
230.	July 24, „	20·6	19·5	19·0	17·7	16·6	14·7	17·3	17·3

TABLE III. (*continued*).

No. of lunation.	Date of new moon, beginning lunation.	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		°	°	°	°	°	°	°	°
231.	Aug. 23, 1873.	17·6	14·8	11·7	12·3	14·1	13·0	13·6	14·3
232.	Sept. 21, „	15·5	22·3	21·9	15·5	13·6	14·0	14·0	16·2
233.	Oct. 21, „	11·0	12·2	15·5	17·0	12·8	6·8	8·1	7·7
234.	Nov. 20, „	8·5	13·2	10·8	9·9	7·3	7·5	8·4	10·4
235.	Dec. 19, „	9·2	9·4	8·9	11·0	10·8	9·4	10·8	9·3
236.	Jan. 18, 1874.	9·0	10·8	11·8	8·9	7·7	9·6	12·6	10·3
237.	Feb. 16, „	8·2	12·4	10·4	12·6	14·7	15·0	14·5	13·2
238.	Mar. 18, „	12·8	14·5	18·9	15·8	11·4	13·3	15·6	13·0
239.	Apr. 16, „	14·1	19·0	21·8	20·5	17·7	14·9	15·6	13·6
240.	May 15, „	15·2	21·4	18·9	18·3	18·5	22·5	23·1	23·0
241.	June 14, „	18·8	13·4	17·1	17·0	12·4	16·1	22·7	24·6
242.	July 13, „	19·8	25·6	22·5	17·8	17·0	17·3	14·2	13·3
243.	Aug. 12, „	12·6	15·3	21·9	23·7	20·6	15·5	12·3	10·8
244.	Sept. 10, „	13·0	14·5	16·0	15·7	19·9	17·8	11·6	14·1
245.	Oct. 10, „	14·5	11·5	11·1	12·3	11·0	6·6	9·4	14·6
246.	Nov. 9, „	13·4	10·6	10·1	8·5	7·8	10·1	9·8	11·1
247.	Dec. 9, „	10·6	6·9	5·0	7·6	7·9	6·8	9·0	10·2
248.	Jan. 7, 1875.	7·5	8·0	6·9	6·8	8·2	8·4	11·1	13·2
249.	Feb. 6, „	9·2	6·4	9·0	9·6	6·1	8·3	6·3	9·0
250.	Mar. 7, „	12·4	10·0	10·7	11·9	13·1	15·1	11·9	11·1
251.	Apr. 6, „	11·7	10·6	10·8	22·5	25·5	19·8	20·7	17·4
252.	May 5, „	15·4	13·6	21·6	21·5	16·2	18·1	17·2	20·9
253.	June 3, „	24·0	20·7	16·0	12·8	15·0	19·2	18·6	14·3
254.	July 3, „	13·4	14·0	13·5	11·8	10·5	12·7	18·6	21·5
255.	Aug. 1, „	19·4	18·7	14·9	14·5	17·8	18·4	19·4	17·3
256.	Aug. 30, „	16·7	18·3	21·3	17·4	18·0	19·5	13·3	11·4
257.	Sept. 29, „	13·7	14·1	14·0	15·4	16·5	11·3	7·8	9·5
258.	Oct. 29, „	6·3	7·9	10·8	11·5	13·5	12·2	9·5	7·5
259.	Nov. 27, „	5·7	5·1	6·6	7·0	8·8	10·6	9·1	9·1

9. Making use of the whole series of lunations of Table III., we obtain the following result :—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Value of range......	14·08	14·20	14·29	14·05	13·95	13·83	14·04	14·17	(A)

a series which presents the appearance of a double period. It will also be noticed that the sum of the four left-hand numbers is larger than that of the four right-hand numbers, the former being 56·62 and the latter 55·99.

If we now divide the whole series into two parts, we obtain as follows :—

Phase of lunation ..	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Range (1855–65)...	13·93	14·11	14·13	13·82	13·79	13·69	13·73	13·88	(B)
„ (1866–75)...	14·24	14·31	14·48	14·31	14·15	13·98	14·39	14·49	(C)

It will be seen that the order followed by the numbers in series (B) and (C) is similar to, though not identical with, that followed in series (A), and also that in series (B) and (C) the sum of the four left-hand numbers is greater than the sum of the four right-hand numbers. We may

perhaps conclude that, taking the whole year, the lunar variation presents the appearance of a curve with a double period superposed upon one of a single period. The range of this variation is not, however, very great.

Series (A) is represented in Fig. 1.

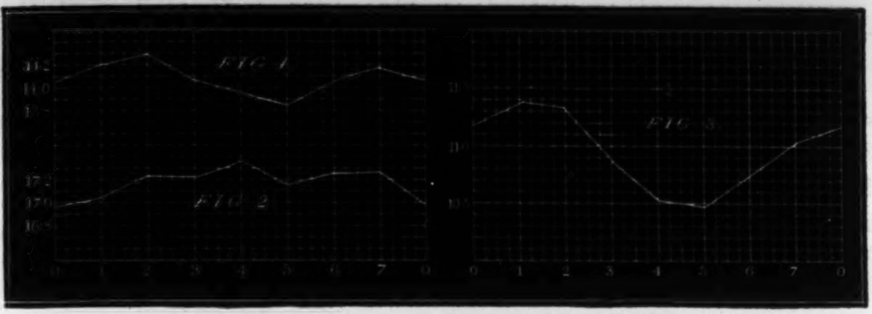


Fig. 1 denotes the lunar variation for the whole year.
Fig. 2 denotes the lunar variation for the summer months.
Fig. 3 denotes the lunar variation for the winter months.

D. *Semiannual Lunar Variation.*

10. If we now make use of the lunations corresponding to the six winter months (October to March), employing for this purpose lunations 1–2, 10–15, 22–27, 34–40, 47–52, 59–64, 71–77, 84–89, 96–101, 109–114, 121–126, 133–139, 146–151, 158–163, 170–176, 183–188, 195–200, 208–213, 220–225, 232–237, 245–250, 257–259, we obtain the following result:—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Value of range......	11·28	11·41	11·32	10·82	10·44	10·43	10·79	11·09	(D)

But before making use of these numbers, we must apply to them the following small correction :—

The winter lunations represent observations of which new moon corresponds, we may say, on an average to the beginning of the various winter months, while full moon corresponds to the middle and (7) to a few days before the end. It is possible, however, that the sum of the various new-moon observations for any six winter months (inasmuch as they occur at dates preceding those of the corresponding full-moon observations, or observations for other phases) may be affected differently from the latter by the annual variation which is indicated in Table I.

The correction applicable on this account has been obtained in the following manner:—The mean monthly values of Table I. have been embodied in a curve, and from this curve has been tabulated the values of temperature-range corresponding to the eight divisions of each of the six winter months.

By this means it has been found that the following correction is applicable to the numbers of (D) :—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Correction applicable to winter months.	−·10	−·04	00	+·06	+·08	+·06	00	−·04	(E)

Applying this correction to (D), we obtain the following corrected results for the winter lunations of the whole series:—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Correct value of winter lunar range...	11·18	11·37	11·32	10·88	10·52	10·49	10·79	11·05	(F)

Series (F) is represented in Fig. 3.

11. If we now make use of the lunations corresponding to the six summer months (April to September), employing for this purpose these lunations, not specified in the winter list already given, we obtain the following result:—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Value of range......	16·86	16·98	17·25	17·26	17·43	17·21	17·26	17·23	(G)

To these values must be applied a small residual correction of the same nature as that represented by (E), but opposite in sign to it. This correction, obtained in the same manner as (E), is as follows:—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Correction applicable to summer months.	+·10	+·04	−·02	−·04	−·08	−·06	−·02	+·04	(H)

Applying this correction to (G), we obtain the following corrected results for the summer lunations of the whole series:—

Phase of lunation ...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Correct value of summer lunar range...	16·96	17·02	17·23	17·22	17·35	17·15	17·24	17·27	(I)

Series (I) is represented in Fig. 2.

12. If we look at Fig. 1, which represents the lunar variation for the whole year, we shall observe, as already remarked, that it seems capable of being represented by a curve of double period superposed upon one of single period, which latter has its left-hand values greatest, or, in other words, is similar to Fig. 3. If we now look at Fig. 2, which represents the lunar variation for the summer months, we perceive an irregularity as if two variations were superposed on each other, one a double variation and another a single variation of opposite nature to that of Fig. 3. Fig. 3 speaks for itself; the variation is here extremely marked; and if we suppose it due to the superposition of two curves, a double and a single one, we must suppose the single curve to be much more pronounced than the double one.

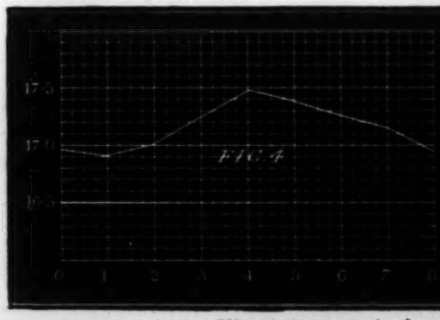
13. The hypothesis of a double curve common to both periods of the year, and a single curve going in one way and very marked for the winter months, and in the opposite way and not so marked for the summer months, would appear to be one which would at any rate enable the results to be presented to the eye in a more simple manner. Let us now deduct the variation implied in Fig. 1, or the whole annual variation, from the values of Fig. 3, or the winter values, and we obtain as follows:—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Winter values (F)...	11·18	11·37	11·32	10·88	10·52	10·49	10·79	11·05	
Correction for (A)...	00	−·12	−·21	+·03	+·13	+·25	+·04	−·09	
Winter difference series.	11·18	11·25	11·11	10·91	10·65	10·74	10·83	10·96	(J)

Performing the same operation for the summer months, we obtain as follows:—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Summer difference series.	16·96	16·90	17·02	17·25	17·48	17·40	17·28	17·18	(K)

The summer difference and the winter difference series are represented by Figs. 4 and 5.



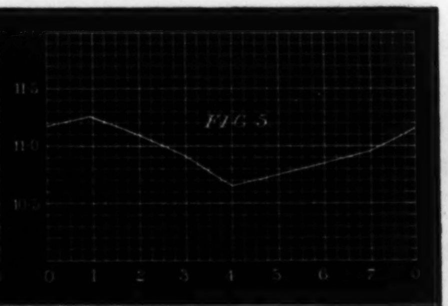


Fig. 4 denotes the lunar summer difference.
Fig. 5 denotes the lunar winter difference.

14. It may here be desirable to exhibit separately the winter lunar variation for the years 1855–65 and that for the years 1866–75.

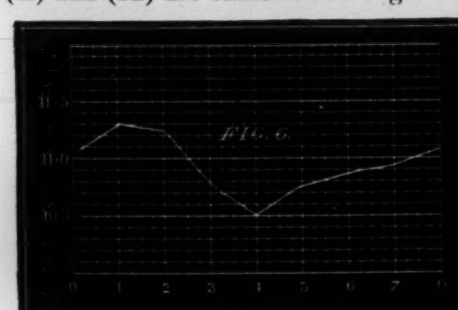
In the former case we obtain—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Value of range corrected for (E)	11·08	11·30	11·24	10·77	10·50	10·76	10·89	10·93	(L)

while in the latter case we obtain—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Value of range corrected for (E)	11·29	11·44	11·40	11·00	10·57	10·17	10·69	11·18	(M)

(L) and (M) are exhibited in Figs. 6 and 7.



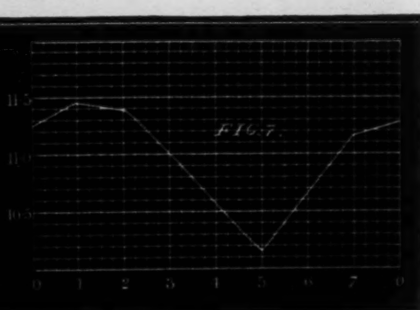


Fig. 6 denotes the lunar winter variation for the years 1855–65.
Fig. 7 denotes the lunar winter variation for the years 1866–75.

E. *Possible Variation of the Lunar Effect with the Sun-spot Period.*

15. In order to determine this point as well as we can from the observations, let us consider as minimum solar years the years 1855–57, 1862–67, 1873–75, and as maximum solar years the years 1858–61, 1868–72, and we thus obtain two values for the winter variation, one corresponding to minima and the other to maxima solar years. Subtracting from these the mean winter variation (F), the residual difference, if any, may be supposed to represent an effect possibly due to the solar period.

By these means the following results have been obtained:—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Winter variation (min. period), corrected for (E)	10·75	10·86	10·99	10·58	10·13	10·33	10·56	10·83	
Deduct (F)	11·18	11·37	11·32	10·88	10·52	10·49	10·79	11·05	
Supposed effect of solar minimum.	−·43	−·51	−·33	−·30	−·39	−·16	−·23	−·22	(N)

and in like manner—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Winter variation (max. period), corrected for (E)	11·76	12·05	11·75	11·29	11·06	10·69	11·12	11·34	
Deduct (F)	11·18	11·37	11·32	10·88	10·52	10·49	10·79	11·05	
Supposed effect of solar maximum.	+·58	+·68	+·43	+·41	+·54	+·20	+·33	+·29	(P)

There would thus appear to be, judging by the winter observations, two effects of a solar minimum and maximum.

The first of these is a general decrease for minimum years and increase for maximum years of the temperature-range, a result already indicated by Tables I. and II.

The second of these is a general decrease for minimum years and increase for maximum years of the difference in temperature-range caused by the varying positions of the moon.

Thus it will be seen from (N) that the minimum range lags most behind the average range at the point (1), for which the average range is highest; while it lags least behind the average range at the point (5), for which the average range is lowest. (P), of course, indicates a behaviour of exactly the opposite description.

16. Let us now direct attention to the prominent character of the winter lunar effect.

Gathering together the various values of this effect, we obtain as follows:—

Phase of lunation...	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Winter lunar effect, whole series (F).	11·18	11·37	11·32	10·88	10·52	10·49	10·79	11·05
Do, 1855–65 (L) ...	11·08	11·30	11·24	10·77	10·50	10·76	10·89	10·93
Do. 1866–75 (M)...	11·29	11·44	11·40	11·00	10·57	10·17	10·69	11·18
Do. (min. years) (N)	10·75	10·86	10·99	10·58	10·13	10·33	10·56	10·83
Do. (max. years) (P)	11·76	12·05	11·75	11·29	11·06	10·69	11·12	11·34

Judging from these results, we would remark that the great and persistent effect of the lunar winter variation renders it difficult to entertain any doubt of its existence.

17. It will now be desirable to indicate one result of the supposition that the lunar effect is somehow connected with the state of our luminary *.

Mr. J. A. Broun, in a series of very interesting investigations, has indicated the possibility that the sun is one-sided in his action on the earth, in other words, that a certain heliocentric longitude or section of the sun is especially influential. From the results of this paper it may likewise be imagined that a particular configuration of the sun and moon is especially influential in a meteorological point of view.

Suppose now, that, although not a year of maximum sun-spots, there are yet a great number, and that the influential solar longitude is turned towards the earth when the moon is in her most favourable position. This favourable conjunction may occur several times in succession, inasmuch as the period of the sun's rotation does not differ greatly from the lunar period, and thus we might have a meteorological effect produced even rivalling that of the solar maximum sun-spot period.

In fine, without confining ourselves to a particular hypothesis, it is evident that if the sun and moon have both to be taken into account as regards meteorological action, the meteorological influence may not be always proportional to the sun-spot area. It may perhaps be thought to militate against the hypothesis of a connexion between the lunar effect and the sun period, when we see, as above, that the first eleven years of the series, taken consecutively, exhibit a less lunar effect than the last ten years. To this, however, it may be replied that undoubtedly the last ten years, embracing the maximum of 1870, represent greater solar activity than the first eleven years.

Again, if the range of temperature for different years were merely a solar effect, having no connexion with the moon, we might expect the summer range and the winter range to be increased and diminished in the same or nearly the same proportion. Now, if in Table I. we regard the years 1858–61, 1868–72 as representing both years of solar maximum and years of maximum temperature-range, we shall find that the sum of the mean ranges for the six winter months of

* This is the hypothesis assumed by Mr. J. A. Broun to explain the facts of terrestrial magnetism.

these years is 67·22, while the sum of the mean ranges for these months for the whole series is 64·92. On the other hand, the sum of the mean ranges for the six summer months of the maximum years is 104·87, while the similar sum for the whole series is 103·13. Thus the increase for the winter months of the maximum years is not only relatively but even absolutely greater than that for the summer months.

18. It may be remarked, in conclusion, that the method of dealing with meteorological observations herein indicated may be applied in various ways to the elements of the same station and also to those of different stations.

The author forbears to mention these various ways, which will occur to every meteorologist. He cannot, however, help thinking that an extension of the present method of investigation is likely to lead to interesting results.

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END OF THE TWENTY-FIFTH VOLUME.

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